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Risk of Renewable Energy Sources: A Critique of the Inhaber Report

John P. Holdren
Kent Anderson
Peter H. Gleick
Irving Mintzer
Gregory Morris
Kirk R. Smith

ENERGY AND RESOURCES GROUP

Room 100, Building T-4
University of California
Berkeley, California 94720

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Energy and Resources Group, University of California
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Kirk R. Smith

Resource Systems Institute, East-West Center
Honolulu, Hawaii 96848

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ABSTRACT

Herbert Inhaber's report, Risk of Energy Production (Atomic Energy Control Board Report AECB-1119, Ottawa, Canada, 1978) has been described by its author and its sponsors as a pioneering, comprehensive, consistent, and unbiased comparison of the health hazards of conventional and unconventional energy technologies. None of these descriptions is accurate. We show here that the report's approach is not original, that its coverage is not complete, that its calculations are not consistent, and that it is biased against unconventional energy technologies and in favor of nuclear power.

The report's widely circulated and potentially influential conclusion is that the health hazards of deriving energy from wood, wind, and sunlight are comparable to those of using coal and oil and much greater than those of using nuclear power. This conclusion is in no sense derived from the actual characteristics of the technologies involved. It is based entirely on mistakes of all varieties: conceptual confusions, inappropriate selection of systems and data, misreadings and misrepresentations of literature, improper calculational procedures, and untenable assumptions and contentions. The nature of these mistakes is more than occasionally obscured by a layer of important typographical and arithmetic mistakes.

When the effects of the most important and easily corrected errors are removed, the Inhaber report's conclusions change drastically. The estimated occupational health hazards of all the unconventional technologies considered fall by 6 to 50 times; the estimated public health hazards of wind, photovoltaics, solar-thermal-electric plants, and biomass (the unconventional technologies judged most dangerous in the report) fall by 9 to 900 times; and the upper-limit estimate of the public health hazard of nuclear power increases by almost 50 times. These changes turn the report's ranking virtually upside down. Based on the ranking criterion used by Inhaber--the upper limits of combined occupational and public risks--nuclear power becomes third from worst

(superior only to coal and oil), and the unconventional renewables rank as superior to nuclear power and far superior to coal and oil.

Inhaber's errors and the consequences of correcting them are described here in documented detail. Also discussed are the circumstances--including the roles of the author, the sponsors, and the knowledgeable technical community--that permitted so error-riddled a report to gain widespread credibility. We have given the matter such detailed attention for two main reasons: first, the Inhaber report's erroneous conclusions bear directly on issues at the heart of current national and international energy dilemmas, and could easily cause or be used to justify poor policy choices; second, the widespread notice that Inhaber's claims have drawn to the topic of comparative environmental assessment provides a good opportunity to call attention to the pitfalls as well as the potential of this important field.

PREFACE

This report was originally scheduled for publication in April 1979. It was delayed to permit expanded coverage of points, both technical and historical, whose importance became clearer to us from the responses to abbreviated versions of our critique (in the form of draft letters to Science) circulated to colleagues in March and April.

In our draft letters to Science, we indicated that the task of elucidating and documenting all the errors in AECB-1119 "would require a small book". Now that we have written one--a document somewhat longer than the subject of its critique--we know we were too optimistic: there are many additional errors we could not take the time or space to elucidate here. We hope, nevertheless, that the reader with the patience to wade through it all will both find our critique sufficient and agree with us that it was necessary.

The very long first chapter, called "Overview", summarizes all the important points, is self-contained, and will probably be enough for most readers. Chapters 2 through 10 provide additional detail on a technology-by-technology basis.

JH, KA, PG, IM, GM, KS
Berkeley and Honolulu
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CHAPTER 1: OVERVIEW

I. INTRODUCTION

This report is a detailed critique of Herbert Inhaber's study of the comparative risks to human health associated with conventional and unconventional energy technologies. The Atomic Energy Control Board of Canada (AECB), where Dr. Inhaber is employed as Scientific Advisor in the Technology Impact Division, made this study public in April 1978 in the form of his report, Risk of Energy Production (Inhaber 1978a), hereinafter referred to as AECB-1119. Subsequent editions, designated AECB-1119/REV-1 and AECB-1119/REV-2, were released in May and November 1978, respectively. (Unless otherwise specified, our critique's substantive comments about AECB-1119 refer to the third and so far final edition.)

AECB-1119's conclusion, which its author claims is based on a detailed literature review and consistent application of an at least superficially plausible methodology, brought the report prompt and widespread attention. The report asserts that the health hazards of deriving energy from wood, wind, and sunlight are comparable to those of using coal and oil and much greater than those of using nuclear power and natural gas. The propagation of this surprising news via dissemination of copies of AECB-1119 itself was considerably augmented by publication of articles summarizing the report's conclusions in New Scientist (Inhaber 1978b), Energy, The International Journal (Inhaber 1978c), and Science (Inhaber 1979a); by Lord Rothschild's reliance on Inhaber's results in a lecture on risk broadcast nationally in the United Kingdom by the BBC (Rothschild 1978); and by a number of published accounts stimulated by these events (Nuclear News 1978, Knox 1978, Greider 1979, Wall Street Journal 1979).

Our own decision to perform a detailed review of AECB-1119 emerged only gradually, and was the result of a combination of circumstances. The AECB sent the senior author a copy of the first edition of the report shortly after it was published in spring 1978, but he had no occasion to look at it until, several months later, its conclusions were called to the attention of a

National Academy of Sciences committee on which he sits. As that point, a few hours' perusal of AECB-1119 by two of us (Morris and Holdren) revealed such pervasive errors and inconsistencies that we concluded it was not to be taken seriously, and we so informed the committee.

Subsequently, however, we learned that Inhaber's conclusions already had been taken very seriously indeed by New Scientist, Nuclear News, and Lord Rothschild, and that, notwithstanding a growing degree of controversy surrounding the work, it was being quoted approvingly by a surprising number of students of the energy predicament (Wildavsky 1979, Wall Street Journal 1979). It was apparent that few if any of the individuals quoting Inhaber's conclusions had actually read the appendices to AECB-1119 containing his calculations. (Neither the published summaries nor the main body of AECB-1119 contain any calculations at all; they merely describe the method and state the conclusions.) Our attention was also drawn to Inhaber's habit, in his summaries for journal publication and in speeches on the subject around the world, of emphasizing his reliance on our own work as evidence of his lack of pronuclear bias. (A report on which two of us were coauthors--Smith, Weyant, and Holdren 1975--accounts for 30 out of 163 numbered citations in the first edition of AECB-1119.) These circumstances persuaded us of the need to respond formally and in detail.

In early 1979, Holdren wrote to Nuclear News and to the Atomic Energy Control Board (with a copy to Inhaber) to point out that AECB-1119 had misrepresented and misused the work of Smith et al. and that these and other major errors invalidated the report's conclusions. Smith wrote to Lord Rothschild and the Wall Street Journal with the same message. A more detailed review--the forerunner of the present critique--was begun and then modified into a letter to Science (Holdren, Smith, and Morris 1979) when that journal published Inhaber's summary of AECB-1119 in late February (Inhaber 1979a).

Two factors that have been operating throughout our involvement with this issue have encouraged us to supply, in this critique, a level of detail far beyond what we ourselves would have considered necessary or appropriate at the outset. The first is the remarkable tendency of Inhaber and the AECB, in their responses to previous criticisms of AECB-1119 by us and many others, to deny, to misrepresent, and to minimize the errors that the critics have

plainly shown the report to contain. It has become clear that any error not elucidated and documented in excruciating detail is a candidate for continuing obfuscation by the report's purveyors. The second factor is the tenacity of some members of the technical community in clinging to the view that the errors in the report do not invalidate its overall conclusion. We attribute this tenacity to a combination of phenomena: a predisposition, on the part of those who have always been skeptical of unconventional energy sources, to want to believe Inhaber's results; and a (not entirely unrelated) refusal to look at the calculations and relevant literature in sufficient detail to reach an independent opinion. We hope, by means of the level of detail supplied here, both to preclude further obfuscation and to encourage technically trained individuals to draw their own conclusions.

We are also motivated by the hope that this critique will serve a useful purpose beyond the mere refutation of Inhaber's erroneous results. For Inhaber's report and the controversy surrounding it have attracted far more notice to the field of comparative environmental assessment of energy technologies than have earlier studies, which made less dramatic claims. This unprecedented attention to the topic provides us the opportunity, in dissecting Inhaber's errors, to communicate to a sizable audience some of the complexities and pitfalls that make environmental assessment as challenging as it is important.

Because of the dual purpose of our critique, we devote attention not only to the many outright and indisputable errors in AECB-1119, but also to more subtle issues that would be considered by some respectable students of these problems to be controversial or unresolved. We trust that our attention to these more subtle issues will not obscure (or be used by defenders of AECB-1119 to try to obscure) our most fundamental conclusion--that AECB-1119's outright errors alone completely invalidate its results.

The remainder of this overview chapter is organized as follows. Section II describes AECB-1119's approach in the context of other attempts at comparative environmental assessment. Section III summarizes some of the report's more striking errors, organized under seven headings: III.A. Inconsistencies, III.B. Conceptual Confusions, III.C. Inappropriate Selection of Systems and Data, III.D. Misreadings and Misrepresentations of Literature, III.E.

Improper Calculational Procedures, III.F. Untenable Assumptions and Contentions, and III.G. Typographical Errors, Internal Discrepancies, and Arithmetic Mistakes. Section IV traces the evolution of Inhaber's results through the three editions of AECB-1119 and the various published summaries, and indicates what the results would be if the larger and more easily corrected errors we have identified were removed. Section V discusses the circumstances that led to the wide dissemination and acceptance of AECB-1119's conclusions, and speculates on the lessons this affair may hold for those interested in preserving the rationality of the energy debate.

The rest of our critique--Chapters 2 through 10--examines Inhaber's calculations on a technology-by-technology basis, following to some extent the organization of the Appendices in AECB-1119.

II. INHABER'S APPROACH IN CONTEXT

AECB-1119 compares eleven energy-supply technologies in terms of their direct risks to human life and health. "Risk" is given its usual meaning as probability times consequences, summed over possible events with health implications, and both public and occupational health are considered. The technologies examined are: nuclear power (based on light-water reactors); electricity generation with conventional steam power plants burning coal, oil, and natural gas; hydropower; solar-thermal-electric power plants; photovoltaics; wind power; ocean thermal energy conversion (OTEC); flat-plate solar heating systems; and production of methanol from commercial timber, a form of biomass.

In AECB-1119 and in the published summaries of it, Inhaber emphasizes the necessity to consider the full fuel cycle for each technology to be compared:

> "However, to be complete we should evaluate the risk from the entire energy cycle, not merely the end the public sees. This is what this paper attempts to do." (AECB-1119, p. 2)
>
> "The entire cycle for producing energy was considered, not just part." (Inhaber 1979a, p. 718)

His diagrams and descriptions of how he achieved the desired coverage, however, reveal an inability to distinguish among fuel-cycle steps, operations

that take place at all steps, and categories of risk:

> "While it cannot be proved that all sources of risk have been accounted for, the seven major ways shown in Figure 1 probably comprise almost all the risk in energy production. These seven are: material and fuel production, component fabrication, plant construction, operation and maintenance, public health, transportation and waste disposition." (AECB-1119, p. 8)

The first item on this list lumps together a fuel-cycle step (fuel production) with an operation common to all steps (material production). Of the remaining six items: three are operations associated with most fuel-cycle steps (component fabrication, plant construction, operation and maintenance); two (transportation and waste disposition) can be considered either fuel-cycle steps or operations connected with many steps, depending on the context; and one (public health) is a value at risk from all operations at all fuel-cycle steps.

This bag of apples and oranges should have given knowledgeable readers early cause to question Inhaber's grasp of the topic he was tackling. Certainly there is no sign that he had read any of the substantial body of earlier work dealing with approaches to systematic comparative assessment of environmental costs of energy (see, e.g., Council on Environmental Quality 1975, Science and Public Policy Program 1975, Energy/Environmental Data Group 1975, Budnitz and Holdren 1976, Holdren 1977).

Inhaber's coverage of the range of environmental effects of energy technologies is as incomplete and haphazard as his taxonomy of the sources of those effects. Public disease from energy activities, for example, is represented in AECB-1119 in only two forms: days of disability and premature deaths from respiratory illnesses associated with emissions of sulfur oxides in the presence of particulates; and deaths and days lost due to cancer caused by radiation from nuclear-power activities. Inhaber offers no estimates of disease effects of oxides of nitrogen, hydrocarbons, or trace metals (mercury, lead, cadmium, nickel, etc.) emitted to the air by combustion of fossil fuels. He entirely neglects public disease from water pollution from nonnuclear energy activities--e.g., contamination by hydrocarbons and trace metals released in extracting, processing, and transporting fossil fuels. And he

ignores all disease effects in generations beyond the present one--e.g., genetic illnesses caused by chemical mutagens from fossil fuels and radiation from nuclear power, and cancers produced in future generations by radiation from uranium-mill tailings and carbon-14 released in nuclear-fuel reprocessing. Some of these omissions are not Inhaber's fault, in the sense that the data needed to make meaningful estimates of the effects are simply not available. We do find fault with Inhaber's willingness to draw sweeping and quantitative conclusions about comparative public health risks from so scanty a data base, with almost no mention of the possible significance of what has been left out.

The component of risk to which Inhaber has given the most detailed and systematic attention is that associated with the creation of energy facilities: the acquisition and transportation of the raw materials (e.g., steel, copper, glass, plastics); the fabrication of these into components (e.g., heliostats, turbines, generators); and the on-site construction operations that transform the fabricated components plus additional materials (structural steel, cement) into finished facilities. Many of the health effects associated with these activities are occupational--mainly accidental deaths and injuries in the jobs involved, plus some job-related disease. Some public effects also arise in Inhaber's calculus, however, owing to transportation accidents and the sulfur emissions from the use of coal to make steel.

Several pages in AECB-1119 are devoted to a description of the method Inhaber has used to tally up the occupational effects of facility creation. He starts with estimates of materials requirements for the facilities under investigation, and normalizes by dividing by lifetime energy output to get tons per megawatt-year. Then he uses labor statistics to obtain worker-hours per ton of material obtained or fabricated and combines these figures with occupational health and safety data (deaths, injuries, illnesses, and workdays lost per thousand worker-years) to obtain occupational risk per unit of energy. The contribution for on-site construction is obtained by multiplying estimated construction labor in various categories by the corresponding incidences of occupational injuries and illnesses. The occupational risk for operation and maintenance is handled in the same way.

Inhaber makes it clear that he considers this approach to computing

occupational effects to be the centerpiece of his analysis, and one presumes it is to this approach that the President of the Atomic Energy Control Board refers when he writes in the Preface to AECB-1119: "The author has developed a new methodology for processing and interpreting available data." Certainly nothing else in AECB-1119's approach could justify this claim to originality, because all the rest of it differs from what is standard in the literature mainly in being somewhat less coherently structured. Alas, the treatment of occupational effects does not justify the claim either, because this approach was taken almost verbatim--not only the method, but also many of the data and some significant errors--from one of Inhaber's references, an unpublished internal memorandum from the Jet Propulsion Laboratory (AECB-1119 reference 16: McReynolds 1976). The JPL study that spawned this memorandum compared public and occupational health effects of coal-fired, oil-fired, and nuclear electricity generation with those of three kinds of solar power plants and was published in 1977 (Caputo 1977). The various components of the JPL project, which include the previously mentioned work of two of us (Smith _et al_. 1975), account for about a fourth of the 190 numbered citations in AECB-1119/REV-2.

Originality aside, the general approach taken by Inhaber would be a useful _piece_ of a comprehensive comparative risk assessment, if it were carried out correctly and consistently. As we shall show, these conditions are not met by Inhaber's analysis. Even if they _were_ met, however, no conclusions could be drawn about the relative environmental attractiveness of energy alternatives on the basis of this partial assessment alone. The reason is that different energy technologies distribute their environmental costs very differently among the various categories of harm--direct damage to human health, damage to property, disruption of climatological and ecological processes, and sociopolitical impacts. While the occupational health risks given the most attention by Inhaber's approach may well be the most serious liability of most unconventional energy technologies, they are a less serious liability of most conventional sources than the public-health damages he treats more sketchily, which in turn may be less serious than the climatological, ecological, and sociopolitical risks he treats not at all. It is quite likely, for example, that the greatest risks to human well-being from coal burning come from carbon dioxide's threat to climate and acid rain's threat to ecosystems, that the greatest risk from oil burning is the threat excessive imports pose to international political stability, and that the greatest risk

from nuclear power is its link to nuclear weapons. It is difficult to escape the conclusion that, by focusing most of his attention on the class of risks (occupational injuries and illnesses) where renewables have their main liability, while ignoring the climatological, ecological, and sociopolitical risks that represent the biggest shortcomings of conventional energy sources, Inhaber skewed his comparisons from the start.

III. A CATALOG OF MISTAKES

We have shown in the foregoing section that the procedure Inhaber says he used in AECB-1119 is incapable in principle of producing a fair or comprehensive comparison. In this section, we summarize our case that, in addition, (a) he did not even do consistently what he says he did, and (b) what he did do, he did wrong.

This section's summary survey of Inhaber's mistakes is organized according to types of errors: inconsistencies; conceptual confusions; inappropriate selection of systems and data; misreadings and misrepresentations of literature; improper calculational procedures; untenable assumptions and contentions; and typographical errors, internal discrepancies, and arithmetic mistakes. Subsequent chapters in our report describe these errors in more detail in the context of the individual energy technologies to which they relate.

As will become clear, some of Inhaber's mistakes are of little quantitative importance in affecting his conclusions, while others are so big that they turn his conclusions upside down. Some of the mistakes are unfavorable to the conventional technologies, while others are unfavorable to the unconventional ones. Still others, of a methodological character, so muddle his analysis that it is impossible to determine their exact effect without undertaking a new analysis from scratch. The general pattern is clear, however, notwithstanding a substantial amount of purely random incompetence: the biggest errors are those that overstate the risks of renewables and understate the risks (or the uncertainties surrounding the risks) of nuclear power.

III.A. INCONSISTENCIES

As noted in Section II, above, Inhaber strongly implies in AECB-1119 and in th. published summaries of it that he has treated all the energy technologies considered on a consistent basis, counting all the health risks associated with producing a unit of energy by these various means. He writes (AECB-1119, p. 18):

> "Generally speaking, the risk from non-conventional systems was calculated in the same way as for conventional systems. Because the former group is built and operated differently from the latter group, the risk components were rearranged to bring out the differences more clearly. However, the basis of the methodology is the same for all systems." [Emphasis added.]

He also makes clear that the heart of his approach is the calculation of the risks of creating energy facilities--acquiring the raw materials, fabricating components, and constructing the facilities. One reads (AECB-1119, p. 10):

> "Much of the detailed risk calculations in this paper is centered on three of the items of Figure 1: material and fuel production, component fabrication, and plant construction. While the other four items are also important, the risk attributable to them is estimated by different and less complicated methods."

A few pages later, Inhaber emphasizes the consistency with which he has treated the materials-related risks of different technologies (AECB-1119, p. 13):

> "The final materials required for system construction often require intermediate and raw materials. For example, steel requires iron ore, coal and other basic materials. For this case, we have assumed that 1.67 kg of iron ore and 1.5 kg of coal are required to produce 1 kg of steel (20). This is a broad simplification. Since the same assumption is made for each of the energy systems, all are treated on an equal basis. [Emphasis added.]

The statements to which we have added emphasis in the first and last passages quoted are simply false. While Inhaber has considered intermediate materials

and has estimated risks of materials acquisition, component fabrication, and plant construction for his unconventional energy sources and for hydropower, and while his report and his various published summaries lead the casual reader to believe he used the same approach for coal, nuclear power, oil, and natural gas, *he in fact has not counted the risks of creating these conventional energy facilities at all*. The numbers tabulated in AECB-1119 for occupational deaths and injuries in the coal, oil, gas, and nuclear fuel cycles come virtually entirely from Smith *et al*. (1975) and Comar and Sagan (1976) and are for operation and maintenance only; they include no contribution from materials acquisition, component manufacture, or plant construction.

Contributing to the concealment of this massive inconsistency is the fact that AECB-1119 and the more extensive published summaries of it do show some estimated materials requirements and partial labor requirements (on-site construction but not materials acquisition and component manufacture) for the conventional technologies. But Inhaber never even attempts to translate this information into occupational and public risks, as he has done for the renewables. If he had done so for nuclear power (for example), the lower bound of nuclear's occupational risk as presented in AECB-1119 would have been about 1.7 times higher and the upper bound about 1.15 times higher.

Leaving aside the failure to calculate the risks of creating conventional energy systems, AECB-1119's comparison of the labor requirements of creating conventional and unconventional energy technologies is itself inconsistent. As just noted, Inhaber's labor figures for coal, oil, gas, and nuclear power include only on-site construction, while those for the renewable energy sources include not only on-site construction but also materials acquisition and component manufacture. We are certain of this because Inhaber's data for the labor requirements of the conventional technologies come directly from the work of two of us (Smith *et al*. 1975), and he lays out the calculations for the unconventional sources in detail in the appendices to AECB-1119. The distortion introduced by this particular inconsistency is large: use of input-output models has shown that a typical solar-thermal-electric power plant needs about 2.5 times as much labor to build (including all materials acquisition and component manufacture) as a coal plant of the same average output (Davidson and Grether 1977), in contrast to the factor of 28 difference shown in AECB-1119.

Yet another inconsistency in Inhaber's tabulation of labor requirements in AECB-1119 is his bizarre decision to include the logging-industry labor supplying wood as the feed to his methanol plant as part of the construction labor for this facility (AECB-1119, Table 4, p. 26). Construction labor for the coal, oil, natural gas, and nuclear facilities considered in the same table does not include the labor to mine the coal and uranium or to extract the oil and gas. Although a footnote and the accompanying text explain clearly enough what Inhaber has done, the casual peruser of the data is given the impression that methanol plants have huge construction-labor requirements compared to all the other technologies.

Even if AECB-1119 did not contain the gross inconsistencies just described, a more subtle one would remain. The data available on materials requirements and labor requirements for conventional energy technologies are for real commercial systems, for which it may be assumed that economic forces have been acting to minimize the amounts of materials and labor used, consistent with reasonable performance. The estimates of labor and materials requirements for unconventional energy technologies, by contrast, are for preliminary commercial designs, or for experimental systems, or for entirely hypothetical ones, for which there has been little to no economic optimization. From the array of estimates available in the literature, moreover, Inhaber has almost invariably chosen the highest ones; that is, he is looking at those versions of the unconventional technologies that are most demanding of materials and labor. (This is so even after correcting for his many misreadings and other outright mistakes.) Since costs of materials and especially of labor are major parts of the cost of creating energy facilities, it is almost certain that the extraordinarily labor-intensive and materials-intensive designs considered by Inhaber will never be built in significant numbers. They will be out-competed by the more frugal designs Inhaber ignores (or by still more frugal ones not yet invented).

III.B. CONCEPTUAL CONFUSIONS

III.B.1. Equivalence of Energy Forms

One of the most elementary errors in analysis of energy systems is to assume one Btu (or joule or kilowatt-hour) to be equivalent to any other,

irrespective of whether the energy is in the form of heat, chemical fuel, electricity, or something else. AECB-1119 is littered with instances of this mistake. Inhaber starts out, in fact, by announcing that he is embodying it in his ground-rules:

> "Most of the energy systems considered in this report can be used to generate electricity. Electricity is the form of energy with perhaps the widest variety of end uses. However, we are not concerned here with end uses, but energy production. As a result, all units of energy produced are deemed equivalent." (AECB-1119, p. 2)

The trouble with this notion is that the value of a unit of energy depends on its thermodynamic quality and other properties as they relate to the intended application and the available technology. If the application is operating a radio, it takes about three units of energy as chemical fuel to be the equivalent of one unit of electricity, because only electricity will do the job, and present technology can only get one kilowatt-hour of electricity out of each three kilowatt-hours of fuel. If the application is powering an airliner, on the other hand, a kilowatt-hour of liquid fuel is worth more than a kilowatt-hour of electricity. For this application, electricity will not work at all and, if one's primary energy sources produced only electricity, powering an airliner would require using the electricity to produce a portable fuel such as hydrogen at an energy loss--more than one kilowatt-hour of electricity would be needed to produce each kilowatt-hour of chemical energy in hydrogen. As a final example, if the intended application is space heating, a kilowatt-hour of low-grade thermal energy (say, from a flat-plate solar collector) is worth as much as a kilowatt-hour of electricity if the latter is to be converted to heat simply by running it through resistive wires, but the thermal energy is worth <u>less</u> than the electricity if the latter is to be used in a heat pump that can deliver two or three units of thermal energy per unit of electricity used.

It must be added that one must be careful to compare energy forms at the same <u>place</u>--that is, for example, electricity delivered to a room to be heated versus energy delivered to the room by solar-heated water. If one compares electricity at the power plant to heat delivered to a room, one misses the losses in transmission and distribution of the electricity before it reaches

the point of application.

These sorts of considerations made it clear to competent analysts long ago that the sensible approach is to compare the environmental costs of alternative pathways leading to the same energy form or, better still, to the same benefit (e.g., a warm room).

The statement quoted above and Inhaber's applications of it in AECB-1119 reveal no understanding of these points. The "megawatt-years net output" that serves as the common basis for all his tabulations of health effects refers to electricity at the power plant in the case of nine of his eleven technologies, to thermal energy as delivered to a room in the case of solar space heating, and to mechanical energy measured at the wheels of a methanol-burning automobile in the case of methanol.

Let us consider the case of methanol more closely. Inhaber assumes the methanol is made from wood obtained in conventional logging operations (in the treatment of which he makes many errors we discuss later), and that the product is used to drive automobiles at 12 percent efficiency (mechanical energy at the wheels divided by chemical energy in the methanol). Inhaber contends it is fair to consider a megawatt-year of electricity produced at a power plant to be equivalent to a megawatt-year of mechanical energy delivered to the wheels of automobiles because the electricity "could have been used to drive autos and buses" (AECB-1119, p. J-2). If Inhaber were following his stated dictum that "all units of energy produced are deemed equivalent" (emphasis added), of course, he would have to count a megawatt-year of chemical energy in methanol as equivalent to a megawatt-year of electricity. This would make the risks of methanol 8.33 times lower than those he tabulated, by getting rid of the 12 percent end-use efficiency factor (1/0.12 = 8.33). Aside from that, his defense of the equivalence of mechanical energy at the wheels and electricity at the power plant has three main defects: (a) the operating performance of electric automobiles for most purposes does not approach that of those powered by liquid fuel; (b) losses between the power plant and wheels of electric autos (transmission and distribution, battery charging and discharging, losses in the controller and in the electric motors themselves), completely ignored in Inhaber's comparison, are typically around 50%; (c) if electric vehicles really made more sense, one could easily burn

the wood directly to make electricity without suffering the significant energy loss in converting wood to methanol. The amount of wood that must be grown to make a megawatt-year of electricity--the rational comparison given the other technologies Inhaber considers--is about six times smaller than the amount needed to make a megawatt-year of energy measured at the wheels of a methanol-burning auto.

AECB-1119's discussion of the case of solar heating is equally illuminating as regards Inhaber's understanding of different energy forms and the efficiencies attached to them. He writes (AECB-1119, p. G-1):

> "All the previous systems considered produced electricity as their final product, while the present one produces thermal energy. We shall assume, to make data comparable, that the thermal energy corresponds to the electrical energy that would have been required to heat a building. This implies that there are no losses in heating water by electricity. The actual losses are about 5% - 10%, so the risk in the following section is probably underestimated by this ratio."

Inasmuch as "the following section" discusses the risk of solar-heating systems, Inhaber has stated the effects of the losses in the electric alternative exactly backwards; these losses mean he overestimates the risk of the solar approach relative to the electric one. (His figures credit the solar heater only with the heat it actually delivers to the rooms, while leaving out, as he says, the losses in the electric system between power plant and delivered heat.) That this is a real confusion, and not just a printing error, is confirmed by Inhaber's use of this example in his summary for Science (Inhaber 1979a, p. 718) to emphasize his generosity toward renewables:

> "For solar space heating, the thermal energy produced was taken to correspond to the electrical energy that would have been required to heat a building; such an assumption leads to an underestimation of risk from this system by a few percent.... The example of solar space heating is an illustration of my general tendency or policy to give nonconventional energy systems the benefit of the doubt, in terms of risk, wherever possible."

Had Inhaber introduced the possibility of an electrically driven heat pump, he could have argued that his comparison was being generous to solar heating. His use of "a few percent" to characterize the magnitude of the "underestimation" of the risk of the solar approach shows that he did not have a heat pump in mind.

A final example of Inhaber's views on the equivalence of energy forms is too amusing not to mention, although it is probably of little real consequence in the analysis. In AECB-1119's section on solar-thermal-electric conversion, Inhaber describes (and tallies up the construction risk for) a thermal-energy storage system consisting of rocks and oil (as the heat-transfer medium). Then, in the subsequent sections dealing with wind and photovoltaic electric power plants, Inhaber assumes that they use the same energy-storage system as the solar-thermal-electric plant. Of course, storage of heat is of no use at all to a wind or photovoltaic plant, since neither one can convert heat into electricity.

III.B.2. Storage, Back-Up, and Electricity-System Integration

By far the most important conceptual confusion in AECB-1119 from the standpoint of affecting the report's quantitative conclusions underlies Inhaber's treatment of energy-storage and back-up requirements for intermittent energy sources such as sunlight and wind. Based on reasoning that can only be described as intricately fallacious, Inhaber assigns to the intermittent renewables the health consequences of burning large amounts of dirty coal as "back-up" when the sun isn't shining or the wind isn't blowing. Leaving aside the errors imbedded in the calculation of the coal risks (discussed later), this procedure accounts for most of the upper-limit values of total health risks shown in AECB-1119 for wind, photovoltaic, and solar-thermal-electric systems.

In response to widespread criticism of early editions of AECB-1119 on this point, Inhaber added to the third edition an appendix that recomputes the "back-up" contributions on the assumption that nuclear power or natural gas is used as the back-up source in place of coal. On the bar charts that portray the relative risks of the various technologies in AECB-1119 and in the summary in *Science*, this alternative is not very conspicuous: the *tops* of the bars,

which give the casual reader his/her main impression of the ranking of the alternatives, still portray the case of coal back-up. Irrespective of the back-up source, moreover, Inhaber's procedure remains conceptually erroneous, as we now show.

Inhaber's stated intention in ascribing risks of back-up to his intermittent renewables is to put them on the same footing as the baseload coal and nuclear plants he supposes they would replace.(1) This supposition is itself an error, inasmuch as Inhaber has declared in his ground-rules that he is considering near-term technologies:

> "An important assumption in the calculations is that present-day technology, models and systems, with their corresponding risk, are used. Generally speaking, energy systems either in present use or likely to be used in the near future were analyzed." (AECB-1119, p. 5)

It is clear that the <u>near-term</u> applications of intermittent renewable electricity sources will not be as baseload plants but as "fuel savers" that reduce the use of fossil fuels by harnessing sun and wind whenever they are available (Bell 1975, Metz 1978). This mode of operation requires no storage and entails no back-up; it reduces the net risk of the electricity-generating system by an amount equal to the difference between the risks of building and operating the renewable technologies and the (higher) risks of getting and burning the fossil fuels that the renewables replace. (One variant of this mode uses windpower, as available, to pump water from below to above an existing hydro dam, increasing the energy the hydro facility can produce later on demand; the wind technology's effect on system health risk would still be computed as just described.)

Solar-thermal-electric plants probably also will be used in the regular intermediate-load role in the electric power grid as soon as it becomes economically feasible to equip such plants with enough heat-storage (perhaps 6

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(1) Baseload plants are those that an electric utility operates as steadily and as close to the plants' capacity as possible, in order to produce most economically the "base" level of electric output below which demand never falls. Demand above this base level is supplied by intermediate-load and peaking plants, which operate less of the time and often below their rated capacity, because they must "follow" the variations in the demand.

hours at 70 percent of rated capacity) to extend operation into the early evening demand peak typical of most utility grids. (An earlier, lower peak on most grids occurs during morning daylight hours.) For this role, too, the appropriate computation of risk would look at the net system effect--risk added by the solar plant minus the risk of the fossil fuels displaced.

Given, however, that Inhaber tries to force intermittent renewables into the baseload role, in violation of his own ground-rule about near-term systems, one may ask whether his procedure for calculating their risk in that role is correct. His only actual computation for the risk of back-up and energy storage for a renewable system in the baseload role is for the solar-thermal-electric system, and it is thoroughly misconceived. Later he states that the back-up and storage risk of the photovoltaic and wind systems in the baseload role can be assumed to be the same as that of the solar-thermal-electric system. This contention represents a separate confusion; whatever the approach to calculating back-up and storage risk, these three systems differ from each other so drastically in the relevant characteristics that their storage and back-up needs for a baseload role bear no resemblance to one another. But let us return to Inhaber's treatment of the solar-thermal-electric system.

For this system, Inhaber assumes (and tallies up the materials requirements and risks of) an energy-storage capability of 16.5 hours of operation at 70% of rated capacity, which his references indicate would permit an annual load factor of about 85%. Yet to the risk computed for each 1000 megawatt-years delivered by this system, he adds the risk for 19% as much energy--190 megawatt-years--from coal as "back-up". Here Inhaber has simply misunderstood his source on back-up requirements.[2] The solar plant described needs no net back-up energy at all to be the energy-producing equivalent of a conventional base-load plant with the same annual load factor.[3] Consider: A solar plant rated at 1000 megawatts and achieving an annual load factor of 85 percent

(2) Inhaber cites Manvi (1977), an internal report in the JPL solar study. A more accessible treatment of the JPL conclusions on storage and back-up requirements is the final report of the JPL project (Caputo 1977, pp. 4-26/4-29).
(3) The annual load factor is the annual electrical output divided by the output corresponding to steady operation at rated capacity for all 8760 hours in the year.

delivers 850 megawatt-years of electricity per year, exactly the same quantity as a 1000-megawatt-rated coal or nuclear plant that achieves the same load factor. The utility grid must maintain some back-up capacity for *all* its baseload plants, against the statistical certainty that part of the time these plants will be out of service--whether due to scheduled or unscheduled maintenance, accidents, regulatory shutdowns, or (in the case of solar) prolonged bad weather exceeding the storage capability of the plant. For all the baseload plants described--coal, nuclear, solar--the annual energy demand on that back-up capacity is exactly the same. If, for example, the system annual baseload demand were 1000 megawatt-years, the annual back-up energy for any one of our 1000 megawatt-rated plants, given its annual load factor of 85 percent, would be 150 megawatt-years (1000 minus 850).

There is absolutely no basis, then, for charging the solar plant described by Inhaber with the effects of back-up coal while ignoring the back-up needs of his nuclear and coal plants. Replacing a 1000-megawatt-rated, 85-percent-annual-load-factor coal or nuclear baseload plant with AECB-1119's 1000-megawatt-rated, 85-percent-annual-load-factor solar baseload plant would produce an extra back-up energy requirement in the utility system of exactly zero. Inhaber's addition of the risks of energy back-up from coal to the risks of the solar-thermal-electric plant described is completely gratuitous.

Actually, the distortion represented by AECB-1119's treatment of back-up is even worse than the foregoing suggests, because the nuclear and coal-fired baseload plants used as the standard of comparison are assumed in the report to have an annual load factor of only 70 percent. (This figure is consistent with recent operating experience for large coal and nuclear plants; 85 percent would be unrealistically high.) The solar plant described does not merely *match* the annual energy output of these plants without benefit of any back-up energy, it *exceeds* their output by the margin of 850 megawatt-years to 700. Either Inhaber should be charging the conventional plants with the "back-up" energy *they* need to match the annual output of the solar plant, or he should be tallying up the risks of a smaller and less materials-intensive energy storage system for the solar plant (so that it is just big enough to permit matching the 70 percent annual load factor of the conventional plants).

One important subtlety in the back-up issue needs to be mentioned: The amount of back-up capacity needed for baseload plants in a utility grid is a separate and more complicated question than the amount of back-up energy. The essence of the matter is as follows. In any utility grid, the difference between the peak demand (the maximum power in megawatts the utility is ever called upon to deliver) and the total installed capacity is called the margin. The reliability of the grid, measured in terms of the probability of not being able to meet the instantaneous demand at some moment, depends on the size of the margin--the bigger the margin, the greater the reliability. Determining the quantitative relation connecting reliability and the size of the margin, however, requires a rather complicated analysis, typically done on a computer, because the relation depends on the detailed pattern of demand (the load curve) in the utility's service area, on the reliability characteristics of each plant in the grid, and on other factors. The central question for our purposes here is: Does the size of the margin needed to produce a specified level of reliability change when coal or nuclear baseload plants are replaced with solar baseload plants of equal annual energy output? That is, might it be necessary to add back-up capacity when solar plants are introduced, even though their annual output is such that they require no net back-up energy?

According to the JPL study of the question, the answer is yes. The extra margin required because of the use of solar baseload plants is very small if the solar installed capacity is a small fraction of the total capacity on the grid. But if twenty percent of the grid's installed capacity were solar baseload plants, the JPL study shows that each 1000 megawatts of solar capacity would require almost 200 megawatts of extra margin--in effect, nonsolar back-up capacity--to produce the same grid reliability as when no baseload capacity is solar. The main reason is that simultaneous outages of solar baseload plants are much more likely than simultaneous outages of nonsolar baseload plants, because all the solar plants can be knocked out by a single and not terribly uncommon event--a prolonged spell of bad weather. Most unscheduled outages of nonsolar baseload plants are caused by random mechanical problems that are independent from one plant to the next. (At least, this is what the JPL analysis assumed; it is not clear whether recent experience with such "common mode" shutdown mechanisms as coal strikes and safety-related nuclear-plant shutdowns would change this verdict appreciably.)

In any case, this reason for solar's need for extra capacity margin should also make it clear how one can need back-up capacity without needing any net back-up energy. Consider an oversimplified grid consisting of 10 baseload plants and their back-up plants, and consider two alternatives for the baseload part: 10 solar plants of 1000 megawatts rated output and 70 percent annual load factor each, and 10 coal-fired plants with the same specifications. Assume, again for simplicity, that the system load curve is a perfectly constant demand for 8,000 megawatts, producing an annual electricity use of 8,000 megawatt-years. Both the solar and the coal baseload alternative deliver 7000 megawatt-years per year, given their common 70 percent load factors. Thus the additional ("back-up") energy requirement in each case is 1000 megawatt-years--no extra beyond this amount is required if solar baseload capacity is chosen instead of coal. But consider how this back-up energy requirement is likely to be distributed over the year. A substantial part of the back-up requirement for the coal plants is likely to be schedulable (e.g., scheduled maintenance in the form of cleaning out coal pulverizers, fuel injectors, sulfur scrubbers, etc.) and so can be spread uniformly through the year. If _all_ of the coal outage were schedulable, 3 out of the 10 plants would be out of service at any given time, so meeting the 8000 megawatts of demand would require 1000 megawatts of back-up capacity in operation in addition to the seven 1000-megawatt baseload plants in service at any given time. Allowing for the maintenance of the back-up capacity and for unscheduled breakdowns of both baseload and back-up plants, the total installed back-up capacity might be 2000 or 3000 megawatts. In the solar baseload case, by contrast, most of the outages that drop the annual load factor to 70 percent will probably be of the unscheduled variety imposed by bad weather. Such events are likely to affect several of the plants in the grid at once, perhaps even all of them. If all ten were reduced to 20 percent of rated output for even a short period by a prolonged spell of bad weather--an event completely consistent with an annual load factor of 70 percent--the back-up capacity needed on line would be 6000 megawatts. In this simplified example, then, one can see how the solar plants could require two or three times more back-up _capacity_ than the coal plants, without needing _any_ more back-up _energy_.

It only remains to explain exactly where Inhaber got the specific figure of 19 percent back-up energy he used for his solar plant. This is not hard to

do. This figure is indeed given in the JPL study; it is the fraction of an annual output of 864 megawatt-years (needed to match the annual output of a conventional 1000-megawatt baseload plant with an assumed annual load factor of 86.4 percent) that would have to be supplied by back-up for a 1000-megawatt baseload solar plant whose annual load factor was only 70 percent. That is:

output of JPL reference conventional plants =
1000 megawatts x 1 yr x 0.864 = 864 megawatt-yr;

output of JPL reference solar plant =
1000 megawatts x 1 yr x 0.70 = 700 megawatt-yr;

back-up needed to boost solar output to that of conventional plant =
864 megawatt-yr - 700 megawatt-yr = 164 megawatt-yr;

fraction of combined output attributable to back-up = 164/864 = 0.19.

Inhaber's error, then, can be summarized as follows:

(1) He found a number for the back-up energy requirement needed to make a 1000-megawatt solar plant with a load factor of 0.70 match the energy output of a conventional plant with a load factor of 0.864.

(2) He applied this number to essentially the opposite case, claiming in effect that this amount of back-up energy would be needed to make a 1000-megawatt solar plant with a load factor of 0.85 match the energy output of a conventional plant with a load factor of 0.70. (His solar plant has a higher load factor than JPL's because it has more storage; his conventional plant has a lower load factor than JPL's because he used a typical value for large conventional baseload plants rather than JPL's highly optimistic one.) For Inhaber's conditions, of course, the conventional plant needs the "back-up" to match the solar one.

(3) On his way to making the huge error described in (2), he made a smaller one in (1). He represents JPL's calculated back-up requirement as 19 percent of the solar contribution, when in fact it is 19 percent of the total delivered by solar and back-up combined. If this were the only error, he would have underestimated the risk of solar back-up. But his

bigger mistake swings the outcome wildly the other way.

(4) The amount of back-up energy Inhaber assigned to the solar plant--19 percent of the solar-generated output--is the amount a solar plant with an annual load factor of 0.59 would need to match the annual output of a conventional plant with load factor 0.70 (1.19 x 590 MWy = 700 MWy). Thus he has described a solar plant with a design load factor of 0.85, attributed to it a load factor of 0.70, and supplied it with back-up energy appropriate to a plant with load factor 0.59.

The risks of the unnecessary and irrationally computed coal back-up account for 85 percent of the upper-limit risk attributed to solar-thermal-electric power plants in AECB-1119. The application of the identical back-up "results" to the wholly dissimilar photovoltaic and wind systems accounts for 74 percent and 62 percent, respectively, of the upper-limit risks attributed to those technologies.

III.B.3. Aggregating Occupational and Public Risk

Inhaber adds deaths, injuries, and illnesses suffered by workers in energy industries to those suffered by members of the public, thus obtaining measures of "total" health risk from energy technologies. This addition of noncomparable entities is a trap that most other analysts of energy's risks have gingerly avoided. The noncomparability of occupational and public health effects has several dimensions.

First, the public effects are a net cost to society, while the occupational effects are a gross cost whose net magnitude depends on what the workers would have been doing if they had not been making or operating energy systems. To see the difficulty more clearly, consider energy technologies A and B, with the following characteristics:

	Technology A	Technology B
energy output	1 unit	1 unit
public effects	2 deaths	1 death
employment involved	1000 jobs	3000 jobs
occupational effects	1 death	2 deaths

Now, the public deaths are an unadulterated environmental cost attributable to the two technologies, and one can correctly say that Technology A is worse than technology B, with respect to this category of social cost, by the amount of the difference between the two values: 2 deaths/unit minus 1 death/unit equals one death/unit as the marginal cost in public deaths of choosing Technology A over Technology B to produce this unit of energy.

With respect to occupational deaths, however, things are not as simple. As we suggested above, the weight to be attached to the occupational deaths depends on what the workers would have been doing if they had not been employed in this particular way. Let us return to our example, and assume that a unit of energy <u>will</u> be produced with either Technology A or Technology B. (That is, for simplicity we exclude the option of doing without the energy.) Technology A creates 1000 jobs wherein the occupational death rate is 1 per 1000; Technology B creates 3000 jobs wherein the occupational death rate is lower--0.67 per 1000 (2 deaths divided by 3000 jobs). The total occupational deaths for B are greater than for A, but each job in Technology B is safer than in A. Let us consider the situation of the 3000 workers in Technology B in terms of two cohorts, as follows. The first thousand workers on Technology B can be compared with the thousand workers needed for Technology A; this cohort of 1000 is <u>safer</u> working on B than the same number of workers would be if working on A. The apparent excess occupational risk of Technology B compared to A can be thought of, then, as being entirely tied up in the additional cohort of 2000 workers who work on Technology B if it is chosen and who work outside the energy sector or are unemployed if Technology A is chosen. If the occupational death rate for Technology B, 0.67 per 1000 jobs, is lower than the occupational death rate in the nonenergy jobs available to these workers and lower than the accidental death rate (during working hours) to which unemployed persons are exposed (e.g., accidental death on the way to the unemployment office), then the 2000 extra workers are safer (and the total number of accidents in society smaller) if they work on Technology B than if they do anything else. On the other hand, if the 2000 extra workers were employed outside the energy sector, their output would represent an additional benefit offsetting the additional social cost of the (assumed) additional occupational risk. Clearly, what seemed, in the simple tabulation of occupational deaths shown above, to be an excess social cost of 1 death per unit of energy, attributable to choosing Technology B, is actually a much more

complicated matter. The key point is: One cannot compare the occupational effects of energy technologies of different labor intensities without accounting for the risks the "excess" workers in the more labor-intensive technology would experience if they were doing something else. To compare energy technologies fairly, it is necessary to compare them on the basis of the net change in risk for which each is actually responsible.

There are also reasons why society might weigh occupational risks differently than public risks in a total risk evaluation, which means, of course, that public and occupational risks should be tabulated separately and not lumped together. The major reason they might be evaluated differently is that, while the public risk is an almost purely external social cost, the occupational risk is partly internalized in the form of higher wages (translated into higher energy costs) for workers employed in the more hazardous of the energy-producing occupations. (An "external" cost is one that does not appear in the producer's cost to make a product or in the consumer's cost to buy it.) Insofar as occupational risks are at least partly internalized, counting them as fully equivalent to largely external public risks is a form of double counting. Of course, doing an analysis of differential wage rates among different occupations to determine the extent to which occupational risks have been internalized is very difficult, in part since many factors other than risk affect differences in wages. It is clear that there are also good reasons, such as imperfect worker perception of latent hazards, that internalization is likely to be incomplete. Nevertheless, available evidence is sufficient to indicate strongly that there is a significant degree of internalization in the form of wage differentials (see, e.g., Thaler and Rosen 1975).

A not entirely unrelated point is that occupational risks are largely assumed voluntarily by individual workers, who do so in exchange for the benefits of the job they choose. (Wages are only one of these benefits.) The public risks of energy production and use, by contrast, are imposed on the bearers of the risks by collective societal choices of how to get energy, not by individual choice. It is possible in principle and often the case in practice that the risks and benefits of such collective choices are not received by the same people. It is also possible that those bearing most of the risk had little voice in the societal choice. It is a well established principle

in risk assessment, moreover, that people deem much larger risks acceptable if these are assumed by individual choice than if the risks are imposed by societal decisions (Starr et al. 1976). These considerations simply reinforce the point already made here that occupational and public risks are not equivalent and additive, as Inhaber has implicitly assumed them to be.

All this is not to suggest that occupational risks are not cause for serious concern. We believe that most occupations are needlessly risky, that many occupational risks have not been internalized, and that continuing and increased attention to occupational health and safety by analysts, employers, and regulators is essential. The serious problem of occupational risks is a separate one from public risks, however, and the differences enumerated here generally have dissuaded sensible analysts of the comparative risks of energy technologies from aggregating the two.

Seemingly more innocuous forms of aggregation, which unlike the foregoing have been practiced by many analysts of energy risks before Inhaber, can also mislead in intertechnology comparisons. Specifically, we are ourselves among those who have succumbed (e.g., in Smith et al. 1975) to the temptation to assume implicitly that any death is the same as any other, permitting deaths from different causes to be added together. The fact is, however, that deaths from cancer, from air-pollution-aggravated respiratory and cardiovascular disease, and from occupational accidents are different both qualitatively and in the amount by which they shorten life.

On the qualitative side, most people would probably prefer to die of some cause other than cancer, since the generally drawn-out miseries of that complex of diseases have been well publicized and are (quite reasonably) feared. No way of quantifying this qualitative distinction seems to be at hand, but its existence may justify keeping separate track of different categories of public and occupational deaths in intertechnology comparisons.

The amount of life-shortening associated with different causes of death, by contrast, is amenable to quantification. Nevertheless, most attempts to aggregate deaths, injuries, and illnesses into "man-days" or "person-days" lost, including Inhaber's, have settled for using a single ratio for converting deaths into lost days of life. The usual value, used by Inhaber and many before him, is 6000 person-days lost per death. This figure (about 16 years)

may be a reasonable average for the loss of life expectancy associated with a cancer death; but it seems likely, on rather elementary grounds, to be too low for occupational accidents by a factor of perhaps two.(4) On the other hand, the figure of 6000 person-days lost per death is certainly much too high for the premature deaths associated with aggravation of pre-existing respiratory and cardiovascular disease by elevated air-pollution levels. The National Academy of Sciences report that is the source of the dose-response relations underlying Inhaber's (and most other recent) figures for public deaths from coal burning says (NAS 1975a, p. 611):

> "Most of the deaths occur among chronically ill elderly people, and the amount by which their lives are reduced may be only a matter of days or weeks."

Simply on the basis of these differentials in life-shortening, it would appear that Inhaber's comparisons--and the many others that have counted deaths associated with atmospheric sulfate levels as equivalent to cancers produced by nuclear-fuel-cycle operations--have probably substantially overstated the relative importance of the former. (The International Commission on Radiological Protection, for example, has addressed this problem in the case of radiation workers (ICRP 1977).)

We emphasize that we make this point here not because Inhaber is the only one to have thus aggregated various kinds of deaths, but because the readiness with which his results were widely seized upon as meaningful seems to call for renewed attention to <u>all</u> the reasons for viewing intertechnology risk comparisons with caution. It is the subtle problems such as the noncomparability of deaths that make it impossible to get the "right" answers simply by redoing Inhaber's analysis without the more obvious mistakes.

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(4) If workers in the most accident-prone jobs typically work at these jobs from age 20 to age 60, and if accident risk is uniform across this age span (experience offsetting declining physical capacities), then the age of the average victim will be about 40. Life expectancy at 40 for U.S. males is about 33 more years (U.S. Department of Commerce 1978, p. 70) or 12,000 days.

III.8.4. Comparisons at the Margin

Another conceptual problem that Inhaber propagates from some of his predecessors in risk comparisons is failure to compare the real alternatives relevant to current or future choices. Specifically, the environmental characteristics relevant to current choices are those of new facilities operated under present regulations and practices, not those of the older facilities and corresponding practices whose characteristics contribute strongly to available historical data. In a coal-versus-nuclear comparison, for example, one wants to know the consequences of building and operating a new coal plant versus those of building and operating a new nuclear plant--that is, as economists would say, the consequences at the margin. All too common in the literature, by contrast, are comparisons of new nuclear plants, assumed to meet all the relevant emissions regulations, with old coal plants, burning high-sulfur coal, mined under 1960s conditions, at emissions rates that would be forbidden in a new plant.

The reader of AECB-1119 or the published summaries of it is given the impression that Inhaber has avoided this pitfall, for the reader is assured repeatedly (as in the passage we quote above in connection with back-up and storage) that "present-day technology, models, and systems, with their corresponding risk, are used." Whereas the baseload role Inhaber assigns the intermittent renewables is too far in the future to be consistent with this ground-rule, the coal-system characteristics he uses are too far in the past. (Since, as we have seen, the largest part of Inhaber's risk figures for the intermittent renewables come from the assumed coal back-up, this combination of gaffes produces the remarkable situation that distant-future renewable technologies are deemed dangerous because coal used to be dangerous.)

Inhaber did not fall as completely into the trap of using outdated coal figures as he might have, but his occupational and public risks from the coal fuel cycle are based in significant part on practices that are either illegal in present U.S. operations (coal-dust levels in mines) or in new plants (SO_2 emissions). Present dust standards, enacted as part of the Coal Mine Health and Safety Act of 1969, imply occupational deaths from black-lung disease as much as 60 times lower than the figure used by Inhaber. (In a typical inconsistency, Inhaber uses data corresponding to present dust levels for black-

lung disabilities but data for pre-1969 dust levels for black-lung deaths; this is illogical, since the deaths are simply the end point of the severest disabilities and have the same cause.) Correction of the black-lung figures would lower Inhaber's upper limit of the occupational worker-days lost per megawatt-year of coal-generated electricity by a factor of 1.4. (More detail on these and the following points on coal is given in Chapter 2.)

The SO_2 emissions Inhaber says he considered (AECB-1119, p. A-1) fall within the New Source Performance Standards in force at the time the report was written; but the upper limit of the number of public deaths from SO_2-related disease (AECB-1119, Table A-2), which is used in the risk calculations, corresponds not to these emissions but to emissions five times higher, exceeding the New Source Performance Standards by a factor of 3.4.[5] In an inconsistency mirroring the one in Inhaber's black-lung data, his figures for incidence of public disease from coal-combustion correspond to lower emissions than do his figures for the public deaths caused by these same diseases. The net inflation of public person-days lost from the coal fuel cycle turns out to be a multiplicative factor of 1.4 compared to what would be obtained by consistent use of the New Source Performance Standards.

AECB-1119's figures for accidental deaths and injuries in coal mines are also either outdated or too high for other reasons. The report cites 1973 safety data (AECB-1119 reference 15: U.S. Department of Labor 1975) on anthracite mining, which it erroneously asserts is the source of the coal used in U.S. steelmaking (see section III.C.3, below). The data AECB-1119 should have used for both steel-making and electricity-producing coal are the safety figures for mid-1970s operations in mining bituminous and lignite coal, which account for about 99 percent of U.S. coal production. The bituminous and lignite data for 1973, presented in AECB-1119's reference 15 just below the anthracite data, imply a figure for worker-days lost due to nonfatal injuries more than 6 times lower than the figure used in AECB-1119 for mining coal for electricity generation. (Inhaber took an injury rate from Comar and Sagan

(5) To derive this result, we used the upper limit of the National Academy of Science dose-response relation referenced by Inhaber (NAS 1975a, Ch. 13), for the most unfavorable location that the Academy considered (a plant sited 60 km upwind from New York City), and worked backward from Inhaber's figure for public deaths to determine the emissions needed to produce these.

1976, which was consistent with recent experience, but he multiplied by far too high a figure for worker-days lost per injury. See Chapter 2.) AECB-1119's figure for accidental deaths in coal mining for electricity production is also from Comar and Sagan (1976), but it is outdated and about 2 times higher than the correct 1973 figure (National Academy of Sciences 1975b).

III.C. INAPPROPRIATE SELECTION OF SYSTEMS AND DATA

Beyond the inconsistencies and conceptual confusions described in the previous sections, several kinds of selectivity and poor judgment in the use of information in the literature have combined to inflate AECB-1119's figures for the risks of the renewable energy technologies. The main contributions are: (1) poor choices of technologies for the functions under consideration; (2) various combinations of high materials estimates and low performance estimates; (3) misapplications of occupational categories in ways that overstate both labor requirements for renewables and occupational risk per unit of labor needed. In what follows, we consider these three categories in turn. Chapter references in parentheses refer to the subsequent chapters in this report, where the individual errors are discussed at greater length.

III.C.1. Poor choices of technologies

One of the attractive features of the renewable energy options is that the range of possibilities contains something that works well in almost every part of the world: the mid-latitudes tend to be sunny, the far northern (or southern) latitudes windy, and the tropics have high potential for growing biomass. Of course, by placing a particular technology in a region where the corresponding form of solar energy is scarce or badly-matched to the need served, one can make that technology look unattractive indeed. This is just what Inhaber does in choosing one of AECB-1119's reference energy systems to be a solar-thermal-electric power plant for Canadian conditions. Every serious student of solar technologies knows that such plants have a chance of making economic sense only in very sunny climates. Not only does the capital-intensity of any sunlight-to-electricity conversion option argue for doing something else unless sunlight is abundant, but the argument is doubly powerful for the solar-thermal-electric scheme considered by Inhaber because (unlike photovoltaics and systems using solar ponds as collectors) this

technology's focusing collectors can only use the direct-beam component of sunlight.[6] Putting a focusing-collector solar-electric plant in the Canadian climate considered by Inhaber is akin to putting a hydroelectric plant in the Sahara desert; it takes a big investment of labor and materials to get a trifling output. (See also Chapter 5.)

No more sensible is Inhaber's choice of an active solar heating system for a leaky Toronto building as another of the reference energy systems in AECB-1119. This technology does not approach being an economically rational choice for Toronto conditions, and it would not become so even at much higher energy prices. The mains reasons are: (1) that plugging the heat leaks in the building and adding a few "passive" solar features can reduce the need for fuel or electric heating much more economically (in labor and materials as well as in dollars) than can active solar heating, and (2) that the residual heating demand after these obvious and economical reductions have been made occurs mostly during the prolonged spells of bad weather when active solar collectors would be working very inefficiently. Inhaber may argue, of course, that he had a perfectly respectable reference for his choice of this system (AECB-1119 ref. 83: Ontario Hydro 1975); our reply would be that it is just such studies as his reference that show the approach to be unattractive in comparison with available alternatives. We note in passing that the Ontario Hydro study tried to deal with the poor match between availability of sunlight and the need for heat by including a very large concrete tank for storage of thermal energy as hot water; Inhaber changes the tank to steel and thereby increases the risk associated with tank constuction (not to mention the cost) many-fold. (See also Chapter 9.)

Another strikingly inappropriate choice that greatly inflates the risk attributed by AECB-1119 to a renewable energy technology is the use of commercial logging as the source of biomass for methanol production. In reality, neither the "fuel plantation" approach nor harvesting from unmanaged forests will be the earliest choice for obtaining biomass for energy. Much cheaper,

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(6) Direct-beam sunlight refers to the parallel rays that arrive straight from the sun without first being deflected by interactions with dust, clouds, and air molecules. The sunlight that has been deflected in these ways arrives from all directions, is called diffuse, and cannot be focused. Much of the sunlight in cloudy climates is diffuse.

safer, and ecologically more attractive is the use of waste biomass in many forms: crop wastes, feedlot wastes, wastes from woodpulp and paper operations, and municipal wastes. Demonstration-scale and commercial operations for converting all these forms of waste biomass into fuel or electricity already exist in the U.S. and many other countries. Nowhere that we know of are there even plans to do commercial logging for the primary purpose of fuel production. Clearly, Inhaber's choice of logging as a biomass source, while ignoring the many waste-biomass operations that already exist, is yet another violation of his proclaimed intention to analyze energy sources "either in present use or likely to be used in the near future" (AECB-1119, p. 5).

Of course it is possible, in the more distant future when the attractive sources of waste biomass are all being exploited, that society will turn to harvesting unmanaged ecosystems (e.g., forests in Canada, chaparral in California) or to fuel plantations of some kind. These operations will surely bear little resemblance to commercial logging for lumber, whose considerable dangers per ton of product are largely related to the kind of harvesting required for this particular application. (This point is made very clearly in the report Inhaber cites as his source on trees for methanol production in Canada: Intergroup Consulting Economists 1976.) Harvesting for fuel permits the use of highly automated technologies resembling those used by the largest paper-manufacturing companies, whose safety record in these operations is much better than in logging for lumber. (See also Chapter 10.)

III.C.2. Massive materials, poor performance

Even after correcting for the many outright mistakes we discuss in subsequent sections, we find that Inhaber seems to have gone out of his way to locate the highest materials requirements in the literature for many of the renewable energy technologies he considers.

For his solar heating system, he uses one of the more materials-intensive flat-plate collectors in the literature and, as noted above, couples it to a massive steel storage tank. The performance he attributes to this system is much too low to be consistent with the large amount of storage provided, even accounting for the unfavorable climate in his Toronto location. (See Chapter 9.)

In AECB-1119's treatment of photovoltaics, Inhaber starts with materials requirements from an unpublished Jet Propulsion Laboratory interoffice memorandum dated May 1976 (McReynolds 1976), ignoring in so doing the somewhat lower numbers published in the final report of the same project (Caputo 1977), which report he also cites. Although the head of the JPL project, Richard Caputo, informed Inhaber in early 1978 that the memorandum contained errors and the figures in the final report should be used instead (Caputo 1979), Inhaber continued to use the earlier numbers in the subsequent revisions of AECB-1119. (See Chapter 6).

The materials requirements Inhaber uses for his solar-thermal-electric plant are correctly described in AECB-1119's reference 21 (Herrera 1977) as being at the high end of the range found in the literature. Neither for this nor for any other technology does Inhaber discuss systematically or use in his calculations the range of material requirements actually to be found in the sources he has cited. He simply makes the general assertion (AECB-1119, p. 21): "In each of the appendices, only what seemed to be the most reasonable values were used."

Beyond the selection of high values of materials required per megawatt of capacity, there are opportunities for inflation of requirements per megawatt-year of output by two other obvious means: the choice of annual load factor and the choice of system lifetime. The lower the load factor, the more megawatts of rated capacity (and hence more materials) are needed to generate a megawatt-year. For fixed capacity and load factor, materials required per megawatt-year can be further inflated by assuming a short facility lifetime. (One calculates materials needed per megawatt-year by dividing the tonnages used to build the facility by its lifetime output.) Inhaber took considerable advantage of these opportunities for inflation.

Consider first the issue of plant lifetime. All of Inhaber's conventional systems are assigned lifetimes of 30 years, except hydropower, which is assumed to last 50. Solar collectors for space heating, windmills, and wood-to-methanol plants, by contrast, are assigned lifetimes of 20 years. Of course, until contemporary commercial versions of all these technologies have operated for the requisite length of time, one cannot be absolutely sure that Inhaber's pessimism is unjustified. There is, however, no a priori reason to

suppose that these unconventional technologies will be shorter lived than the conventional ones. Flat-plate collectors are rather simple devices made of durable materials, and versions used for water-heating have operated for about 30 years in Israel. Wind turbines run at relatively low rotational speeds and stresses (compared, for example, to low-performance aircraft that routinely have lasted 30 years or more), and many small wind turbines have run for more than three decades. (See Chapter 7.) Inhaber's basis for supposing that methanol plants last only two-thirds as long as oil refineries is a methanol reference that uses a 20-year depreciation period having nothing to do with physical lifetime. (See Chapter 10.) Of course, in all cases the use of 20 years in place of 30 increases the risk attributable to creation of facilities, per megawatt-year of output, by 50 percent. We note finally that Inhaber's pessimism about plant lifetime should for consistency's sake have been applied as well to commercial nuclear plants, which he assigned a lifetime of 30 years. No light-water reactor as much as two-thirds as large as the contemporary commercial reactors considered in AECB-1119 has yet operated for as long as a decade (Nuclear News 1979).

Inhaber's manipulation of load factors from the literature is even more obvious. As noted earlier, he charges his solar-thermal-electric plant with the materials and labor requirements of an amount of storage corresponding (according to the references he cites) to an annual load factor of about 0.85, but he claims this plant's load factor is only 0.70 and charges it for back-up energy as if its load factor were 0.59. (See Section III.B.2 above.) The reference from which Inhaber gets all his data on ocean thermal energy conversion specifies a load factor of 0.90 for the system described, but he again uses a figure of 0.70. (See Chapter 8.) An annual load factor of 0.34 is specified for wind plants in the Project Independence report that is the source of Inhaber's materials requirements for wind systems, and the same load factor is given in an ERDA report he cites as the source of the figure in the first edition of AECB-1119. In the second and third editions, however, Inhaber uses a load factor for wind plants of only 0.06 (referenced to a private communication), and the ERDA reference has disappeared altogether. This extremely low load factor implies a windmill site nearly bereft of wind, an unlikely choice for a sensible investor in wind energy.

In the case of wind, a plausible explanation for Inhaber's reduction of

the load factor is not hard to find. A number of critics called to Inhaber's attention that the first edition's materials requirements per megawatt-year of electricity from wind were 50 to 100 times too high. Apparently unable to determine where this inflation actually came from, Inhaber chose to try to make it seem that the high materials figures were caused by a very low load factor. When he finally grasped the real cause of the inflation (an initial mistake by Inhaber of a factor of 50, temporarily increased to 1000 and then reduced to 50 again by consecutive, offsetting confusions between lifetime and annual output), Inhaber wrote a new wind appendix for inclusion in a possible fourth edition of AECB-1119. The load factor in this new wind appendix is 0.14. The only basis we can find for this number is that, being almost exactly 2.5 times smaller than the figure of 0.34 in Inhaber's reference for the materials requirements, its use permits him to misrepresent his initial mistake of a factor of 50 as a mere factor of 20 caused by "a misunderstanding about these requirements on an annual and a lifetime basis" (Inhaber 1979b). The respectable contemporary literature is unanimous in the view that sensibly sited modern wind machines will achieve annual load factors between 0.20 and 0.50. (See Chapter 7.)

III.C.3. <u>Category mistakes</u>

The method used in AECB-1119 to estimate the health risks of creating energy facilities is highly susceptible to mistakes related to the categories of materials and labor involved. The possibilities include: wrong definitions of terms as used in the literature of materials requirements; double-counting of materials-processing steps through misinterpretation of what is included in the Standard Industrial Classification (SIC) categories used for data on labor requirements and occupational health and safety; and assignment of energy workers to the wrong SIC categories. AECB-1119 contains all these mistakes and more. We consider them here under several subheadings.

The concrete-cement confusion

AECB-1119 consistently treats concrete and cement as one and the same. In its calculations of the materials-acquisition risks associated with concrete requirements, it includes the risks of acquiring the full weight of the concrete requirement under both "non-metal mining" and "cement". Concrete is

actually made mostly of aggregate (sand, gravel, and rock), which is bound together by cement. The proportions of the mixture vary depending upon the application, but the cement is generally only one-sixth to one-quarter of the total weight (Brady 1973, pp. 173, 223, 272, 687). Furthermore, cement-making operations include the mining of the required limestone, so no separate step of "non-metal mining" is appropriate for the computation of risks of cement "acquisition". Consequently, Inhaber's figures for "acquisition" risks related to concrete are inflated in two ways: the "non-metal mining" requirement (for the aggregate) should be only three-fourths to five-sixths as large as Inhaber's and the cement requirement should be four to six times smaller. Inhaber's concrete-cement confusion has the effect of inflating the occupational risks from "acquisition" of concrete by about a factor of two.

A second result of this confusion is the exaggeration of transportation risks. Inhaber computed these risks by multiplying the sum of the weights of all intermediate and final materials times the risk per ton of transporting coal by rail. Since he erroneously counted the weight of the concrete requirement twice, he overstated the weights to be moved and the risks that resulted. In addition, almost all the aggregate for making concrete is actually shipped by truck, not by rail, and for distances only about one-tenth as far as U.S.-average coal is shipped by rail (Kearney 1972, Hayes 1976). Transport by truck is more hazardous, however, than transport by rail, so no simple adjustment of Inhaber's procedure is possible. A correct accounting of transportation risks, for all materials, would require analysis of the modes and distances of transportation for each material, and of the different risks associated with various modes. Such an accounting is beyond the scope of this critique. It should be clear, however, that Inhaber's procedure for computing the risks associated with the transportation of non-fuel materials and components provides only the crudest approximation of these risks.

Hard coal versus soft coal

AECB-1119's estimates of occupational risk in acquisition of coal for steel-making are based on data for hard (anthracite) coal. This choice is unreasonable: Inhaber's ratio of 1.5 tons of coal to 1 ton of steel corresponds to a use of about 180 million metric tons of coal for steel-making each year in the U.S. in the early 1970s, but only one percent of the

country's 600 million metric ton annual coal production is anthracite (U.S. Department of Commerce 1978, p. 763). The significance of this unreasonable choice of data is that it inflates the risk: anthracite mining is about 1.5 times more dangerous per worker-hour than mining the bituminous coal and lignite that make up 99 percent of the production. The productivity (i.e. worker-hours per ton) of anthracite miners, moreover, is only about half that of the rest of coal mining, so the use of anthracite figures for coal mining produce an inflation of risk from this activity by a factor of three.

Fabricated metal products

For all of the unconventional renewable sources, the principal contributor to AECB-1119's estimated risks from materials and equipment acquisition is the fabricated-metal-products category (SIC 34). The importance of this category to Inhaber's totals has two origins: first, it is labor intensive (Inhaber's number is 149 worker-hours per metric ton of product, versus, for example, 10 worker-hours per metric ton in steel manufacturing); and, second, Inhaber erroneously assumes, for all the unconventional energy technologies but two, that 100 percent of all the metals used passes through this labor-intensive category. The situation is summarized in Table 1.1.

TABLE 1.1
FABRICATED METAL PRODUCTS (FMP) AND AECB-1119 RISK ESTIMATES

	Percent of Total Metals Assigned to FMP by AECB-1119	Percent of AECB-1119's Materials & Equipment Risk Due to FMP Sector
Solar Thermal	20	51.7
Photovoltaics	22	67.1
Solar Heating	100	82.5
Wind (REV-2)	100	79.7
OTEC	100	79.4

The U.S. Census of Manufacturers indicates, however, that the fabricated-metal-products sector specifically excludes all electrical generating equipment (SIC 36), turbines and generators (SIC 351), and photovoltaics (SIC 3674). Some of these sectors are less labor intensive or safer than fabricated metal products. More importantly, the fabricated-metal-product category does not include piping, basic steel products, and reinforcing steel

and iron, the manufacture of which is already counted in the much less labor intensive primary-metals category (SIC 33); and it does not include field and on-site assembly of fabricated items (accounted for in the contract construction sector, Division C). When Inhaber multiplies fabricated-metal risks by the full metals tonnages for his unconventional energy technologies, therefore, he double counts both with respect to metal that needs no fabrication work beyond what it got in primary-metal operations and with respect to metal whose fabrication beyond the primary-metals stage is counted under construction. Inhaber himself inadvertently provides a very rough indication of the size of this error when he presents data, in AECB-1119's Table M-1, showing that 120 million metric tons of steel were produced in the U.S. in 1973, while only 20 million tons of fabricated metal products were produced.

The ubiquitous roofers

SIC category 176, roofing and sheet metal work, includes some of the most dangerous occupations in U.S. industry. The figure for worker-days lost by employees in this category due to accidents was about 170 per 100 worker-years in the early 1970s, compared to 200 for hard-coal mining, 130 for miscellaneous contracting, and 55 for electrical construction work. Inhaber evidently was much impressed by the versatility of this accident-plagued category of workers, because in AECB-1119 he assigns to them 84 percent of the worker-hours devoted to operation and maintenance of solar-thermal-electric plants, photovoltaic systems, wind power plants, and solar space heating systems. The remaining 16 percent are taken to be supervisory personnel, whom Inhaber implicitly assumes are free of occupational risk. Thus, in practice, he is taking the risk per employee occupied in operation and maintenance to be 84 percent of that in the roofing and sheet metal industry, still a very high number. In the case of the solar heating systems, Inhaber throws in a further factor of four by assuming that the homeowners doing the repairs are effectively amateur "roofers", taking twice as long to do each job, and killing and injuring themselves twice as often per hour.

In the construction of energy facilities, too, AECB-1119 has the roofing and sheet metal workers heavily employed. Inhaber assigns these workers 36 percent of the construction work for solar-thermal-electric plants, 59 percent for photovoltaics plants, and 50 percent for solar heating systems and

windmills. (Only for the heating systems is construction of energy storage included in these figures; for the others, risk of construction of energy storage was computed by an improper procedure, described later, that does not involve assignment of labor to occupational categories.) None of these figures make sense.

Accidental deaths: synthetic statistics

The occupational health and safety data mainly relied upon by Inhaber, and by the JPL work on which he drew so heavily (McReynolds 1976), do not include figures for accidental deaths disaggregated by detailed occupational categories. Instead, these data (U.S. Department of Labor 1975) give accident fatalities only for large classes of industries (e.g., manufacturing, construction, mining). The apparent reason is that there is only a rather small number of accidental fatalities each year in each occupational category, so the random year-to-year fluctuations can be very large. This means that any single year's figure will be of little statistical significance.

Since AECB-1119's approach requires fatality figures for the individual occupations to which labor for construction and operation and maintenance are assigned, Inhaber tries to construct an approximation to these data by multiplying the working-days lost (other than from fatalities) in each occupation of interest times the ratio of fatalities to nonfatal working-days lost in the broad class of industries to which the occupation belongs (and for which the actual fatality data are at hand). Thus, for example, deaths in "plumbing" are estimated by multiplying nonfatal working-days lost in plumbing times the ratio of deaths to nonfatal working-days lost in all construction occupations. (A bizarre variation of this approach is mentioned below under "improper calculational procedures".)

This approach to synthesizing accidental death statistics for individual occupations is perhaps defensible as the roughest of first approximations, and it was indeed used in the earlier JPL work (McReynolds 1976). But, since Inhaber was working two years later, with the JPL results to build on, one might have thought he would take the more sensible approach of using, say, five-year averages of actual fatality data in individual occupations (which would greatly reduce the problem of year-to-year fluctuations). This approach

would have required some real digging into the literature of occupational safety, of course. His long list of references notwithstanding, Inhaber evidently was not prepared to dig.

The problem with the synthetic approach he borrowed from McReynolds is that it misses large differences in the ratio of deaths to working-days lost in the individual industries making up the large sectors for which fatality data were used. This shortcoming might be considered of minor importance in the overall context of AECB-1119's comparisons, except for a further aggregation that permits the contrived accidental-death figures to dominate and submerge the other forms of occupational risk. We refer to the assignment of 6000 working-days lost to each occupational fatality, and the addition of these lost working-days to those from nonfatal occupational injuries and illnesses, in order to obtain a single figure for occupational risk. The result of this procedure is that, for all of the unconventional renewables considered in AECB-1119, 65 to 68 percent of the occupational risk ascribed to operation and maintenance and 45 to 65 percent of the occupational risk ascribed to creation of energy facilities come from the synthetically constructed death statistics.

We can summarize AECB-1119's problems with occupational risk, then, as follows:

(1) A large part of the risk calculation is based on inappropriate choices of occupational categories for the tasks performed.

(2) The part of the results that could have contributed to a meaningful comparison among unconventional renewables, _if_ the correct occupational categories had been used, is the nonfatal working-days lost, for which data disaggregated to the level of these categories were in hand. But by combining these data with death statistics synthesized from death figures for much more highly aggregated categories, Inhaber botched even this modest opportunity to inform.

III.D. MISREADINGS AND MISREPRESENTATIONS

Checking Inhaber's references against the statements in AECB-1119 reveals many discrepancies between what he says the references contain and what is actually found there. Whether those discrepancies represent misreadings or deliberate misrepresentations is usually hard to determine, so we have classified these two kinds of problems under one heading.

The most dramatic of these errors is the inflation of the materials requirements of windmills by a factor of 50 over those in the Project Independence report that is Inhaber's reference on the subject. Making the error required taking materials requirements and energy output from a column of figures in the reference that evidently referred to the start-up phase of a wind program--much construction and little output--while ignoring the several adjacent columns in the same table that give the correct figures for calculating annual output per ton of materials invested. (The same table clearly shows the load factor of 0.34 that Inhaber chose to ignore after his first edition, as noted above.) At the same time, Inhaber apparently misread annual output as lifetime output, which made the 50-fold error into a 1000-fold one, given his assumed 20-year plant lifetime. Later he confused annual and lifetime output a second time and got this factor of 20 back. The net error of a factor of 50 persisted through all three editions of AECB-1119. (See Chapter 7.)

Inhaber's OTEC calculations rest on a jumble of misstatements of what is in the literature he cites. His materials requirements come from a Lockheed report (not actually cited in AECB-1119) via a survey article (Pollard 1976) whose author seems to have misread the Lockheed figures himself. The Lockheed report indicates that a plant of 160 megawatts net electrical output would contain four power modules, each generating 60 electrical megawatts gross and 40 electrical megawatts net after subtracting the requirements to operate the plant. Inhaber's materials figures duplicate Pollard's in erroneously including only one power module rather than four. Since the power modules make up only a small part of the total materials requirement, however, this understatement of risk is modest. It is more than compensated by Inhaber's subsequent errors in misrepresenting the whole plant's output as 96 net electrical megawatts instead of 160, and in using a load factor of 0.70 rather than the 0.90 given both by Pollard and in the Lockheed report. Failing to consult the

primary reference also permits Inhaber to think data given in metric tons are in short tons. Inhaber goes on to overstate OTEC's construction-labor requirements by a factor of 10 to 50, again based on misstatement of what is in a reference. (See Chapter 8.)

AECB-1119's entire section on "material acquisition and construction" for methanol facilities (AECB-1119, pp. J-1/J-2) consists of the application of various (erroneous) "correction factors" to figures Inhaber indicates are for the construction risks of oil refineries. His reference for these figures is Comar and Sagan (1976), who make clear that the numbers are for operation and maintenance, not for construction. Irrespective of Inhaber's erroneous manipulation of the numbers, then, his misreading of operation and maintenance for construction invalidates this part of his calculation completely. (See Chapter 10.)

What is more clearly a case of misrepresentation rather than mere misreading is Inhaber's treatment of what he found in the literature of nuclear risks. Inhaber contends in AECB-1119 and in the various published summaries of it that: (a) many of his data on nuclear risks came from the work of a "well-known nuclear critic" (Holdren); (b) that these data include the risk of "disposition" of nuclear wastes; and (c) that he used the highest values of nuclear risks from the sources he surveyed. The first two of these contentions are misleading and the third is false. The evidence is unambiguous, moreover, that Inhaber knew them to be misleading and false. (See Chapter 3 and Section V.D, below.)

The work by Holdren that Inhaber cites as being the source of many of his data on nuclear risks (Smith, Weyant, and Holdren 1975) is a literature survey. The sources of the numbers Inhaber actually took from Smith et al. are plainly indicated there: they are mostly work done by or sponsored by the U.S. Atomic Energy Commission; none is the work of "nuclear critics". The term "disposition of nuclear wastes" would doubtless be interpreted by most readers to include long-term management or disposal, but the figures from Smith et al. offered by Inhaber under this heading are limited to spent-fuel shipment and reprocessing and are so labeled in that report. Inhaber acknowledges this in a footnote to a table in his nuclear appendix to AECB-1119; but, in the main text of AECB-1119 and in the published summaries, he boasts of including risks

of "waste disposition" with no clue as to the novel definition he is using for this term. (See Chapter 3 and Section V.D.)

The figures Inhaber uses for risk of nuclear accidents are far from the highest given by his references: his "upper limit" is about two times smaller than the upper limit given in the Rasmussen report (Nuclear Regulatory Commission 1975), more than 200 times smaller than the upper limit stated in the Ford/MITRE study (Nuclear Energy Policy Study Group 1977), and some 40 times smaller than the upper limit in Smith *et al*. that Inhaber explicitly claims to have used. (He in fact misrepresents Smith *et al*.'s figures for routine emissions as including accidents, ignoring in so doing the plainly labeled figures for accidents a few lines away in the same table.) Had Inhaber actually used the Smith *et al*. upper limit as he claimed, his upper limit on public person-days lost per megawatt-year of nuclear-generated electricity would have been about 60 rather than the 1.5 shown in AECB-1119's Figure 12, and the upper limit on nuclear's total person-days lost per megawatt-year in Figure 14 would be about 70 rather than the 10 shown. (See Chapter 3.)

III.E. IMPROPER CALCULATIONAL PROCEDURES

Doing a decent job of comparing environmental effects of energy technologies is a formidable task both because there is not enough information and because there is so much. Whole research groups keep busy combing the growing literature to maintain the major data bases--for example, the Brookhaven Energy Model Data Base and the Strategic Environmental Assessment System (SEAS)--familiar to serious workers in the field. Many needed quantities nevertheless remain elusive, and a diligent analyst confronted with a gap must decide whether to dig deeper in the literature, or to try to construct his/her own estimate of the missing quantity, or simply to call attention to the gap and leave it to future work to fill.

Inhaber, by contrast, found it easy to make his "pioneering" contribution--he is said to have spent only a few man-months on the project (Lemberg 1979)--because he did not let himself be troubled by any of these difficulties. His search of the literature was superficial and spotty: the 190 "references" in AECB-1119/REV-2 are actually 190 footnotes that cite a considerably smaller number of documents, and it is clear that Inhaber did not

read most of these. Most of what he did read he evidently did not understand (as we have already shown). And when he needed a number that his superficial search had not turned up (or when he did not like the numbers he found), he lost little time constructing his own estimate--little time because he evidently did whatever first came to mind, whether or not it made sense. This section summarizes some of the more remarkable calculational procedures that resulted.

Inhaber decided to calculate the risk of 16.5 hours of energy storage for his solar-thermal-electric plant by a procedure best described in his own words (AECB-1119/REV-2, pp. E-7/E-8):

> "An indirect approach can be taken. Table 1(a) of Reference 16 indicates the risk due to an average production of 1000 MW(e), without storage. Reference 68 shows the same risk, but modified in two respects: it applies to a load factor of 0.54, and has 6 hours storage. If we multiply Table 1(a) of Reference 16 by 0.54 and subtract it from the table of Reference 68, we obtain the risk incurred to produce 6 hours' worth of storage. Results for this and 16.5 hours, the average recommended by Reference 66, are shown in Table E-3."

Reference 16 is the JPL internal memorandum (McReynolds 1976) that Inhaber had been informed contained errors. Reference 68 is a later report of the JPL project (Herrera 1977), minus the errors and quite possibly with other changes (besides the amount of storage) having been made in the detailed specifications of the plant. The procedure described by Inhaber is hopeless. It leads to a result so ridiculous it troubled even Inhaber, but rather than suspecting his procedure he produces a further silly argument to try to show that the result is right. (See Chapter 5.)

Since, as we noted earlier, Inhaber insists on siting his solar-thermal-electric plant in a Canadian climate to which it is unsuited, he must modify the data in his references that are applicable to the sunny southwestern United States. He does so by multiplying all the risks calculated for operation in the U.S. by 1/0.76 or 1.32, on the basis that insolation at his Canadian site is only 76 percent of that in the Southwest. For this type of plant, however, the appropriate scaling factor is the ratio of direct-beam radiation at the two sites, not the ratio of total insolation used by Inhaber.

(The two ratios can be very different. In this case, Inhaber's error underestimates the disadvantage of the Canadian site.) More seriously, to a first approximation only the risks that scale with collector size should be increased by this factor; once bigger collectors have compensated for lower direct-beam insolation, the rest of the plant and its operation (turbines, generators, transformers, energy storage, back-up) are no different than in Arizona. (See Chapter 5.) Finally, having taken an irrationally computed set of storage risks and multiplied them by an unwarranted factor of 1.32, Inhaber uses the result to represent the risk of storage not only for his solar-thermal-electric plant but for photovoltaics and wind as well.

An equally irrational procedure is to be found in AECB-1119's treatment of photovoltaic power plants. There Inhaber asserts, ostensibly on the basis of a report in a 1971 conference proceedings, that the land requirement for photovoltaics is 34,500 square meters per megawatt-year of electrical output rather than the 3800 square meters per megawatt-year used by JPL, and that it follows that the JPL materials requirements per megawatt-year must be multiplied by a correction factor of 2.27 (AECB-1119, p. F-1). Inhaber's basis for making the inflation factor 2.27 instead of 34,500/3800 = 9.08 is his supposition that the 34,500 square meters per megawatt-year "refers to peak power" and should therefore be divided by 4 to correspond to the average output. This is an astonishing bit of reasoning. First, peak power is measured in megawatts, not megawatt-years. Second, it takes about 4 times <u>less</u> area to make a peak megawatt than to make an average megawatt, not 4 times more. Third, it is an elementary calculation to verify that the JPL land requirement was correct in the first place, so no "correction" to the JPL materials requirements on this basis is warranted at all. Removal of this error alone reduces the non-back-up part of Inhaber's upper limit estimate of risk from photovoltaics by a factor of 1.7. (See Chapter 6.)

Inhaber's treatment of emissions in the acquisition of materials for all his renewable energy technologies is based on numbers for the solar-thermal-electric system, which in turn are taken directly from the JPL study (Caputo 1977). That study attributed a range of 0.5 to 1.5 public person-days lost per megawatt-year to emissions of oxides of sulfur in the production of the materials for a solar-thermal-electric plant. Inhaber scales these values to apply to the other renewables with the aid of the following chain of

approximations and assumptions: (a) the emissions in materials acquisition come entirely from the combustion of coal in the materials industries; (b) most of the coal used in the materials industries is used to make steel; (c) one can therefore obtain person-days lost due to emissions in materials acquisition for any energy technology by multiplying the JPL figures for solar-thermal-electric plants (0.5 to 1.5 person-days lost per megawatt-year) by a ratio consisting of the technology's steel requirements divided by the solar-thermal-electric plant's steel requirements. To say that these are heroic assumptions is an understatement. On the presumption that the original JPL numbers were themselves derived by a more reliable procedure, one could have confidence in Inhaber's extrapolation from that beginning <u>only</u> if the relative proportions of steel and other emissions-producing materials were roughly constant for the various energy systems examined. This is so far from the case that AECB-1119's figures for person-days lost due to emissions in materials acquisition are practically useless.

Inhaber's computation of the labor required to install a flat-plate solar heating system uses an erroneous procedure to extract the maximum inflation from an erroneous piece of data. The starting point is a clearly wrong estimate in the JPL internal memorandum (McReynolds 1976) that it takes 1174 worker-hours of plumbers' time--more than half a working year--to install a ton of pipe. Inhaber multiplies this number by the weight of the copper tubing in his solar collector to get an enormously inflated estimate of the "plumbing" part of the installation labor. (Even if Inhaber had the right ratio for installation labor per ton of pipe, this quantity of copper is not the right basis for the calculation; the copper described is embodied in the collector, and Inhaber has already counted the labor to do so under "fabricated metals".) Finally, he uses a statement from another reference, to the effect that the labor for installing a solar heating system is half plumbing and half roofing, to assign the same inflated estimate to the roofing part of the labor. The effect is to overstate the installation labor requirement per megawatt-year by a factor of about 10 if one accepts Inhaber's value for the system's output. The factor is about 20 if one uses the correct output. (See Chapter 9.)

Elsewhere in his treatment of solar heating Inhaber inflates <u>all</u> the risks of this technology by a further factor of 1/0.70 = 1.43, by applying a

"load factor" of 0.70 to his system's annual output. This is a completely gratuitous reduction of output, since any figure for annual output already has the load factor embodied in it, by definition: capacity (rated megawatts) times annual load factor (average output over rated output) equals annual output (megawatt-years per year).

Many other procedures devised by Inhaber, when he couldn't find the data he wanted or didn't like the data he found, are equally baffling. We mention only two more here.

Apparently unable to find materials and labor requirements for natural-gas technology, Inhaber decided to represent values for natural gas by values for fuel oil or coal gasification, whichever were lower! (See Chapter 2.) As noted earlier, Inhaber did not convert materials and labor requirements into risks for any of the conventional technologies, so the irrational procedure by which he obtained these requirements for natural gas is of little significance; it merely renders meaningless yet another part of AECB-1119's Table 4 (Summary of Material Acquisition and Construction Data, p. 26).

Finally, we take an example from Inhaber's methanol section as representative of his manipulations of occupational health and safety data. Having a figure for worker-days lost in logging but no figure for accidental deaths in that industry, Inhaber purports to estimate the latter by multiplying the worker-days lost in logging by a ratio consisting of the number of deaths in the SIC industry group "contract construction" divided by the number of worker-days lost in the three-digit SIC category of "roofing and sheet metal work". (See Chapter 10.)

III.F. UNTENABLE ASSUMPTIONS AND CONTENTIONS

The errors we discuss under this heading are mostly of small importance to AECB-1119's overall conclusions, but they are worth presenting for the picture they provide of the extraordinary breadth of the incompetence that permeates AECB-1119.

In his discussion of methanol, Inhaber contends that, since methanol contains only about half as much energy per unit weight as gasoline, a methanol factory will require twice as much labor and materials per unit of energy

output as an oil refinery. Obviously, the energy density of the product does not by itself determine the size and complexity of a fuel-producing facility. The fact is that methanol-from-biomass plants perform fewer and less complicated operations than oil refineries, and might well require less construction labor and material, not more.

Later in his discussion of methanol, Inhaber contends that the risk of methanol production per unit of output may be infinite, because "more energy is expended on the process than is gained from the resulting product" (AECB-1119, p. J-2). Consulting his reference on the point reveals that it is not describing a conventional wood-to-methanol plant, but a hypothetical plant making use of electricity from hydropower to boost the amount of methanol derivable from each ton of wood. This approach would make sense, the reference's authors point out, only if hydropower (or another electricity source) were relatively cheap and abundant and liquid fuel relatively scarce and expensive. Even so, it would only be an energy loser if the electricity source were not hydropower but thermal electricity generation. In that case, one would be using three units of heat--say, from nuclear fission--to get one unit of electricity, which would be used to make 1.5 units of energy as methanol from 2 units of energy in wood.(7) (See Chapter 10). Such a procedure is unlikely to be attractive from the standpoint of total cost (economic and environmental): but, if it were, there would be no more reason to worry about its net energy loss than to worry about that of the nuclear plant by itself--three units of nuclear heat to get only one unit of electricity.

Another complete misunderstanding of the technological world is revealed by one of Inhaber's statements about scaling of energy systems. He writes (AECB-1119, p. G-1):

(7) An ordinary methanol plant produces about 1 unit of energy as methanol for every 2 units of energy in the wood used as feedstock. This is an energy loser only in the same sense as are oil-refineries, coal-burning power plants, and all other converters of less-desirable to more-desirable energy forms.

"Solar space heating is the first technology considered in this report to involve scaling-up in size. The previous systems were all assumed to have one plant with a 1000-megawatt rating. This is clearly not feasible for space heating. We have assumed that the materials required for large-scale use of space heating are proportional to that required for small-scale installations, per unit energy output. Only further research and experience can validate this assumption."

It is hard to believe one's eyes, but Inhaber does indeed have this point exactly backwards. One knows for sure that the materials requirements of 1000 megawatts of solar heating in the form of 400,000 2.5-kilowatt installations can be obtained exactly by multiplying the materials requirements of one such installation by 400,000. (This is precisely what Inhaber's "large-scale use of space heating" entails, as described in Appendix G of AECB-1119.) No "further research and experience" are needed to verify that this procedure is accurate. It is not true, moreover, that the "previous systems" considered in AECB-1119 were assumed to have "one plant with a 1000-megawatt rating". The solar-thermal-electric plant from which Inhaber's data were scaled was a 100-megawatt plant. The coal, oil, and nuclear plants were scaled to a hypothetical 1000-megawatt size from data based on a range of actual plant sizes. It is in these scalings that uncertainties enter (as Inhaber's references for these data make clear), not in the straightforward multiplication of the requirements per solar-heating installation.

Our final example in the category of untenable assumptions and contentions is perhaps not as transparently silly a blunder as the foregoing ones, but it is the most important in terms of affecting Inhaber's quantitative results. We refer to his assumption that all the materials for construction of the renewable energy systems must be transported as far as average coal is transported in the U.S., about 300 miles, and thus pose the same transportation hazard per ton. This assumption seems not unreasonable for some materials used in construction of energy facilities, such as structural steel and fabricated components; indeed, it might even produce an underestimate for those categories, insofar as some of these materials may be moved mostly by truck, which is more dangerous per ton-mile than rail. Inhaber has applied the assumption, however, to all the intermediate categories of materials as well as final ones (e.g., to 1.67 tons of iron ore per ton of steel used and

to 4 tons of bauxite per ton of aluminum). He also applied it to such common materials as the aggregate (sand and gravel) for concrete[(8)], rock for the thermal energy storage, and earth for earth-fill dams. In reality, materials such as earth, rock, aggregate, and sandstone are transported at most an average of a few tens of miles. (See, e.g., Hayes 1976.) Some approximate fractions of Inhaber's estimates of transportation risks that are due to the greatly inflated distance he moves earth, rock, sand, and gravel are: hydropower, 98%; solar-thermal-electric, 50% (another 35% is coal transportation for the superfluous "back-up"); OTEC, 50%; and photovoltaics, 35% (another 25% is coal transportation for back-up, inappropriate to the "fuel-saver" mode in which photovoltaics would first be used).

III.G. TYPOGRAPHICAL ERRORS, INTERNAL DISCREPANCIES, AND ARITHMETIC MISTAKES

In any long report, some typographical errors are inevitable. In work that has been produced with reasonable care, however, and certainly in work that has been reviewed by someone other than the author before distribution, it is rare to find typographical errors of the sort that renders crucial points obscure or impenetrable. Internal discrepancies (e.g., lack of agreement between text and tables or between descriptions of calculations and what was actually done) and mistakes in arithmetic should also be a rarity by the time a report has survived internal review, whether in an academic institution or in a government agency. Yet the first edition of AECB-1119, printed in March 1978 after extensive (according to the AECB) internal review, contained gross discrepancies, typographical errors[(9)], and arithmetic mistakes in such profusion as to make following what had been done almost impossible for anyone not intimately familiar with the literature. It is not unlikely, in fact, that the almost impenetrable screen provided by these mistakes at many places in AECB-1119 is part of the explanation for the failure of many early readers of the report to perceive the more fundamental blunders that lay underneath.

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(8) As we noted earlier, Inhaber does not seem to know that concrete is only a sixth to a fourth cement and the rest aggregate and water. Thus he takes tonnages listed in his references as "concrete" and, calling them "cement", multiplies them by the transportation risks of moving a ton of coal 300 miles.

(9) We use a rather generous definition of "typographical" here, in order to include the many instances where Inhaber seems to have written something other than what he meant.

III.G.1. Some assorted examples

An early review of AECB-1119 performed at the International Atomic Energy Agency (Black *et al*. 1978) pointed out more than 30 jarring mistakes in the typos/discrepancies/arithmetic category in AECB-1119 and AECB-1119/REV-1 (May), and we found many more, some persisting into the third edition (AECB-1119/REV-2, November 1978). The flavor of these is imparted by the following examples. (The edition given in parentheses with each mistake is the latest one in which the mistake persists.)

"For example, coal would have a maximum of 10/10,000 = 0.010 deaths per megawatt output per year over the 30-year system life." (AECB-1119, p. 33) The quotient is wrong by a factor of 10.

"Another source (59) indicates a requirement of 38.2 x 10^6 cubic metres (50 x 10^6 cubic yards) of concrete for a system producing 430 megawatts electricity per year." (AECB-1119/REV-2, p. E-3) Megawatts per year is a senseless unit except in the context of the rate of installation of new capacity.

"Original data was for a 1000 megawatt plant. Since a lifetime of 30 years was assumed, data was divided by 30,000." (AECB-1119/REV-1, p. E-6) The division actually performed was by 21,000 (30,000 x 0.70), to take into account the load factor.

"The system specified has a storage volume of 9 cubic metres." (AECB-1119/REV-1, p. G-2) The intended figure is 91 cubic meters.

"Table H-2 shows that material acquisition produces a total number of man-days lost of 2.6." (AECB-1119/REV-1, p. H-2) The figure shown in the table is 88.9 worker-days lost.

"Table H-1 shows that material requirements are less than for solar systems." (AECB-1119/REV-1, p. H-2) Inhaber has written "material" where he means "concrete", since all the other materials requirements stated in the table are higher than his corresponding figures for solar technologies.

"The total requirement for sand is then 45.5 + 11 - 4 = 5.25 million metric tons." (AECB-1119/REV-1, p. F-3) The correct relation is 4.55 + 1.1 - 0.4 = 5.25 million metric tons. Inhaber's attempt to correct this error in REV-2 reads "4.55 + 1.1 - 4 = 5.25 million metric tons."

"Reference 63 indicates that about 2.8 man-hours per kilowatt of capacity is required. Allowing 2000 man-hours per man-year, this is 14 man-years per megawatt of capacity." (AECB-1119/REV-2, pp. E-4/E-7) The figures given are for operation and maintenance, so the appropriate units are worker-hours per year per kilowatt or worker-years per year per megawatt. Straightforward arithmetic starting with the figure given in Reference 63 (67,000 maintenance workers employed per 100 electrical gigawatts of capacity) gives 1.34 worker-hours annually per kilowatt, not Inhaber's 2.8. Starting from the 2.8 figure, Inhaber's second sentence also represents a typo or an arithmetic mistake: 2.8 worker-hours per kilowatt is 1.4 worker-years per megawatt, not 14.

Other examples include:

-- The first column of figures in Table E-2 (REV-2) doesn't add up to the total given there.

-- Two columns of figures on page E-4 (REV-1) are labeled "man-hours" when they should be "man-days".

-- Every single number in Table E-4 (REV-2) is too low by a factor of 10.

-- There are significant discrepancies between accompanying text and Tables E-5, F-2, F-4, G-2, H-2, and I-1 (all REV-1, and some REV-2), among others.

These examples are not isolated, they are representative. There is scarcely a page in the technical appendices of AECB-1119 (where all the report's calculations are found) that does not contain several such mistakes. Many others are described in our later chapters, which take up the contents of these appendices in more detail.

What is as remarkable as the initial number of mistakes of this sort is Inhaber's failure to remove many of them over the eight months between his

first and third editions. As some of the foregoing examples indicate, even when Inhaber detected an error, there was a good chance his "correction" would be wrong, too. And the third and so far final edition introduced some entirely new errors that make the reader's task still more difficult. The most damaging of these is the omission of the two-thirds of Table D-2, Light Water Nuclear Electrical Production System Risk, that previously occupied page D-5. (Page D-5 now contains text, some of it duplicating what appears on page D-7. This section could not have been proofread at all.) The missing portion of the table contains the entirety of Inhaber's estimates of occupational risk from the nuclear fuel cycle plus all the public risk from accidents. Understanding his nuclear section without it is impossible.

The next two subsections describe two especially elaborate sets of blunders in the detail necessary to unravel them for the interested reader. Those who have no particular desire to appreciate the intricate tangles that often lie beneath a mere sentence or two in AECB-1119 can skip these unusually detailed subsections without loss of continuity.

III.G.2. *The methanol mix-up*

A striking example of a passage becoming more opaque as Inhaber struggled with its errors is provided by the treatment of efficiency of the methanol fuel-cycle through successive editions of AECB-1119. (The conceptual confusion underlying this section was discussed above in Section III.B.) In the first and second editions one finds (AECB-1119 and AECB-1119/REV-1, p. J-2):

> "For methanol, the end use will probably be in transportation, a system whose efficiency is low. Reference 118 [National Academy of Sciences. *Criteria for Energy Storage R&D*, 1976] notes that the efficiency of the 'federal driving cycle', combining highway and city conditions, is about 0.12. The third multiplicative factor which must be applied to the data is then 1/0.12 = 8.33."

The "data" referred to here (from Smith *et al.* 1975 and Comar and Sagan 1976) gave risks per megawatt-year of oil-generated electricity, rather than per megawatt-year of chemical energy in oil as Inhaber seems to have thought. Since the fuel-to-electric efficiency assumed in deriving those data was 0.36, performing even the irrational comparison Inhaber had in mind (electricity

derived from fuel oil, measured at the power plant, versus mechanical energy derived from methanol, measured at the wheels of an automobile) would dictate that his "multiplicative factor" be the ratio of the efficiencies of the oil-to-electricity process (0.36) and the methanol-to-mechanical-energy process (0.12). Making the ratio 1/0.12 was equivalent to assuming that electricity could be made from oil at an efficiency of 100 percent. This mistake evidently came to Inhaber's attention (after persisting through two editions), and he attempted to correct it. In the third edition, the passage quoted above became (REV-2, p. J-2):

> "For methanol, the end-use will probably be in transportation, a system whose efficiency is low. The efficiency of the "federal driving cycle", combining highway and city conditions, is about 0.36(33). Avoiding double-counting of conversion losses, the third multiplicative factor which must be applied to the original data is then 0.36/0.12 = 3.0."

This revised passage is so garbled by added errors that no one who had not unraveled the original mistake in the earlier editions could possibly understand what is going on. Specifically, as we noted above, 0.36 is the oil-to-electricity conversion efficiency; its appearance in the REV-2 passage as the driving-cycle efficiency is an error, as is the failure to identify it <u>anywhere in the methanol section</u> as the oil-to-electricity efficiency. The result of these errors is that the reader of REV-2 has no way of knowing where the 0.12 in the ratio 0.36/0.12 comes from, has a false impression of where the 0.36 comes from, and thus can have no idea of what the ratio is all about. The opacity of the new passage is rendered complete by the fact that Inhaber's new reference number (33) for the driving-cycle efficiency is also a mistake: reference 33 is to page 140 of Smith <u>et al</u>. (1975), which is a table of costs and resource requirements for the oil-to-electricity fuel cycle; it is the source of the fuel-to-electricity efficiency of 0.36.

Quite possibly the proximate cause of all this confusion in REV-2 was drop-out of a line when the revised passage was typed or printed. That is, the middle sentence of the quoted passage probably was intended to be two sentences, as follows: "The efficiency of the 'federal driving cycle', combining highway and city conditions is about [0.12 (118). The efficiency of converting oil to electricity is about] 0.36 (33)." We have bracketed the words we

suppose to have been left out. It is indicative of the dimensions of Inhaber's carelessness that this error went unnoticed, even though it completely garbles the third-edition revision of a passage he was trying to correct upon discovery of a major error in the earlier versions.

Even more shocking is the unambiguous evidence that Inhaber has now *fooled himself* with what almost certainly started as a mere typing or printing error. In his response to a draft of our letter to *Science*, Inhaber writes (Inhaber 1979c, p. 12):

> "Since the main use proposed for methanol is in vehicles, it seemed logical to assume that it would be used that way. However, it was pointed out after the first two editions were printed that this assumption, while reasonable, might be unfair to methanol in terms of risk calculation because the efficiency of turning a fuel into electricity (usually around 35%) was greater than the 12% efficiency of turning fuel into mechanical energy in a vehicle.
>
> "In the interest of giving non-conventional energy systems the benefit of the doubt, the efficiency was assumed to be about 0.33. Due to a typographical error in the text, this assumption was not made as clear as it could have been, although the calculations show the assumption being used. I regret that Prof. Holdren was unable to phone or write me to clarify this point, as a number of others did."
>
> "I then was assuming electricity from biomass (or methanol) and comparing it to electricity from petroleum or coal."

In the foregoing passage, Inhaber has confused a reference number (33) with an efficiency (0.33) he never used. The efficiency "correction factor" used in the calculations in the third and last edition (REV-2) is the ratio of 0.36 to 0.12 given at the end of the passage from page J-2 quoted above. *Inhaber's statement that he compared electricity from biomass to electricity from petroleum or coal is false*. To see that it is false, one has merely to read the paragraphs immediately following the passage in AECB-1119 we quoted above (REV-2, p. J-2):

> "As in the case of solar space heating, we have here a technology

which does not directly produce electricity. However, methanol generates something which could also be brought about by electricity - mechanical motion. In effect, we are assuming that the methanol used is equivalent in terms of mechanical energy to the electricity that could have been used to drive autos and buses.

"The number of occupational deaths, injuries and man-days lost per unit energy for oil processing are $0.057\text{-}1.43 \times 10^{-3}$, $4.3\text{-}88.6 \times 10^{-3}$, and 0.58-13.4, respectively (23). Disease-related risk is assumed to be zero. Taking the above methanol-to-oil factors into account, the number of occupational deaths, injuries and man-days lost per unit energy for methanol refining would be $0.51\text{-}12.9 \times 10^{-3}$, 0.039-0.80, and 5.2-121, respectively."

The second set of figures, all of which are reproduced in Inhaber's Table J-1, are exactly 9.0 times the first set. The factor of 9.0 consists of the factor of 3.0 (0.36/0.12) we have been discussing here, times 2.0 (the gasoline-vs-methanol energy density "correction" discussed earlier), times 1.5 (the ratio of an oil refinery's assumed 30-year lifetime to the 20 years Inhaber assumes for a methanol factory). If Inhaber had been comparing electricity from biomass to electricity from petroleum or coal, as he later claimed, the ratio 0.36/0.12 would be absent and the overall "correction factor" would be 3.0 rather than 9.0.

III.G.3. <u>The "materials handling factor"</u>

The methanol mix-up just described is not the only instance in AECB-1119 where Inhaber initially makes errors of the typo/discrepancy/arithmetic variety and then builds these into major confusions and misrepresentations. Another remarkable example of this syndrome is his discussion of the intermediate and final materials requirements for building a solar-thermal-electric power plant. The relevant passage, as it appears in the first and second editions of AECB-1119, reads in full (AECB-1119/REV-1, p. F-3):

"The time spent in fabrication and construction should be approximately proportional to the weight of materials being used. Excluding concrete, Table E-2 shows that a total of 10,900 man-hours is required to construct material weighing 47.3 metric tons per unit energy. Table E-1

shows that a total of 1.03 million metric tons, excluding concrete, were used as final products in the solar thermal plant. Using the 30 year lifetime and the 1000 MW(e) average production, this corresponds to 34.3 metric tons per unit energy output averaged over the plant lifetime. We can deduce that each ton of raw material corresponds to 47.3/34.3 = 1.38 tons in the final construction."

This passage is a hodge-podge of errors, some of them interactive with errors in Table E-2. We can more easily clarify the matter by reproducing here, as our Table 1.2, the relevant column from AECB-1119's Table E-2 (all editions, p. E-5).

TABLE 1.2

THE MATERIALS COLUMN FROM AECB-1119'S TABLE E-2

Material & Equipment Acquisition	Materials (metric tons) [per megawatt-year]
Iron Ore Mining	63
Bauxite Mining	87*
Sandstone	180
Hard Coal Mining	56
Flat Glass	6.3
Cement	174
Steel	39
Fabricated Metal Products	8.3
Aluminum	2.2
Total	616

* This erroneous figure was corrected to 8.7 in AECB-1119/REV-2, but the total was not adjusted accordingly.

The main errors relating to the passage and the table are as follows:

(a) The bauxite figure in Table E-2 was ten times too high in the first and second editions. When Inhaber corrected this error in the third edition, he failed to adjust the total accordingly. (All three editions show 616 metric tons.) With the bauxite figure corrected as in the third edition, the total should be 537.5 metric tons, or 363.5 metric tons without the "cement" (which really is concrete). The mass to which Inhaber first refers in the passage on page F-3, 47.3 metric tons, is evidently the sum of the 39 tons of steel and the 8.3 tons of fabricated metal products. He evidently means to exclude intermediate materials such as iron ore, bauxite, and sandstone, but the figure he gives is neither fish nor fowl: some of the 39 tons of steel is intermediate material that goes into the 8.3 tons of fabricated metal products. If he wanted to get a total for finished materials used in fabrication and construction, without double-counting, he should simply have summed the steel, the aluminum, and the glass: 39 + 2.2 + 6.3 = 47.5 metric tons. If the 47.3 was a typo where 47.5 was meant, one must note that it propagated through the calculation and beyond. Our point here is not that the difference between 47.3 and 47.5 is significant, of course, but rather that Inhaber, as usual, did not know what he was doing. The bigger blunders follow.

(b) The materials numbers in Table E-1 do not add to 1.03 million metric tons, as stated in the passage, but to 999,400 metric tons or, rounding to the same number of significant figures as Inhaber, 1.00 million.

(c) Inhaber divided his 1.03 million tons by 30,000 megawatt-years lifetime plant output to get his 34.3 metric tons per "unit energy" output, where his "unit energy" is a megawatt-year. He has forgotten the annual capacity factor of 0.7 he assumes for this plant (stated in the notes to Table E-1 and the text). He should have divided by 30,000 x 0.70 = 21,000. The correct calculation, 1.00×10^6 metric tons divided by 21,000 megawatt-years, gives 47.6 metric tons per megawatt-year. That, as it should be, is virtually identical to the finished-materials total of 47.5 metric tons, which Inhaber <u>should</u> have gotten from Table E-2.

(d) Using his wrong figure of 47.3 metric tons per megawatt-year for finished materials and his wrong figure of 34.3 metric tons per megawatt-year in the constructed plant, Inhaber then decides that "each ton of raw material corresponds to 47.3/34.3 = 1.38 tons in the final construction". He has this backwards. If the 47.3 and 34.3 figures were not themselves erroneous, the correct conclusion would be that each ton in the constructed plant (presumably what he means by "the final construction") corresponds to 1.38 tons of finished materials entering the fabrication and construction process.

Inhaber's misstatement of this last relation caused some alert readers of his report to marvel at the apparent violation of conservation of mass. The IAEA reviewers wrote (Black <u>et al</u>. 1978): "Appears illogical to assume 1 t of raw material produces 1.38 t product." In apparent response to this observation, Inhaber in his third edition left entirely unaltered the paragraphs we quoted in full above, except that he added one new sentence at the end. The paragraph now closes (REV-2, p. F-3):

> "We can deduce that each ton of raw material corresponds to 47.3/34.3 = 1.38 tons in the final construction. Of course, this implies that each of the final construction products may be handled more than once."

This comedy of errors illustrates, perhaps more clearly than any of the other of Inhaber's blunders we have elucidated here, how intricately confused he was and remains. The finished materials numbers in Table E-2 came from exactly the same source (AECB-1119 Reference 16: McReynolds 1976, Table 2a) as the numbers in Table E-1 for materials embodied in the constructed plant. Inhaber manipulated this one set of numbers from his reference to get tonnages per 1000-megawatt plant in Table E-1 and tonnages per megawatt-year in Table E-2. Then he seems to have forgotten that these were basically all the same numbers, and, by making addition errors and dropping a load factor, made them appear to differ by a factor of 1.38. This he took to represent a ratio of general significance, to be used elsewhere in the report. The paragraph following the one quoted earlier applies the result to photovoltaics (AECB-1119/REV-2, p. F-4):

> "What does this imply for Table F-2? Table F-1 shows that there is a total of 6.24 million metric tons of final products in the photovoltaic

system, excluding cement. Using the assumptions of the previous paragraph, this implies 207.8 metric tons per unit energy, lifetime-averaged. The 'handling factor' of 1.38 then produces 287 metric tons used in construction."

The "handling factor" is purely "handling" by Inhaber. It is inconceivable to us that any competent individual who actually <u>followed</u> such calculations could have recommended distribution of AECB-1119 or publication of summaries of it. We elaborate on this issue in Section V.

IV. <u>AECB-1119'S RESULTS</u>

We have shown that AECB-1119's risk calculations are based on a host of mistakes of all kinds. We now turn to a consideration of the figures for occupational and public risk generated by these mistakes. Our discussion will proceed in two parts. First is an analysis of the evolution of Inhaber's figures, as they have appeared in the three versions of AECB-1119 and in various articles summarizing the report. Then we will show the effects of removing some of the largest and most easily corrected errors from Inhaber's results.

We cannot emphasize too strongly that our revisions of Inhaber's figures are in no sense "correct" values for the risks of the energy technologies considered. As discussed in Sections III.A. and III.B, AECB-1119's entire system of measurement is thoroughly flawed, in that:

(a) the things it fails to measure may well be more important than those it tries to measure;

(b) its evaluation of risks is not done at the margin (that is, comparing risks of alternative choices for the next facility to be built) but rather mixes risks of distant-future configurations of some technologies with risks of old plants and outmoded practices for others;

(c) its measurement of occupational risks confuses gross risks with net risks, and its aggregation of occupational and public risks confuses partly internalized costs with essentially external ones;

(d) the unconventional energy systems are often worst-case examples (or nearly so) in terms of materials and labor requirements, energy outputs, and, hence, economics.

So pervasive a set of defects could hardly be remedied with the limited resources available for this critique. Indeed, comparing carefully and properly the environmental liabilities of conventional and unconventional energy technologies--the task that Inhaber, with much audacity and little insight, attempted to shortcut--will occupy a sizable community of researchers for many years to come. We have ventured to provide here some partial corrections of AECB-1119's risk figures only to dispose of the assertion, made repeatedly by Inhaber (Inhaber 1978d, 1979b, 1979c), that even correction of AECB-1119's largest errors does not significantly change the "overall conclusions". As we show below, this claim is completely without merit.

IV.A. EVOLUTION OF AECB-1119'S RESULTS

The first column of Tables 1.3 and 1.4 shows the risks calculated in the initial version of AECB-1119 (dated March 1978). These numbers were computed by adding up the figures presented in the detailed tables in the Appendices to AECB-1119. An obvious typographical error in Table G-3, corrected in the third edition, was ignored. Nowhere in AECB-1119 are such compilations of its numerical results provided. AECB-1119's only comparisons of its quantitative results for the different technologies are the report's Figures 9 through 12, which are bar charts drawn on logarithmic scales.

In Tables 1.3 and 1.4 we have ordered the technologies according to Inhaber's "overall ranking" as of the first edition of AECB-1119. This "ranking" evidently refers to the sum of the upper limits of occupational and public risks. As discussed in Section III.B.3, these two categories are so fundamentally different that they simply should not be mixed. If this index is used, however, the results are dominated by the risks of coal combustion.

Coal and oil are shown in Inhaber's results to be far "riskier" than any of the other energy technologies considered, because of the large uncertainty about the effects of sulfur oxides on human health. Of the 20-2012 public person-days lost from coal, 7-1920 of these are from sulfur oxide emissions; thus these emissions are 95 percent of coal's risk, using Inhaber's upper-

TABLE 1.3
EVOLUTION OF INHABER'S RISK FIGURES
(Person-days lost per megawatt-year)

	(*)	----- AECB-1119 ----- (3/78)	REV-1 (5/78)	REV-2 (11/78)	Executive Summary (2/78) New Sci. (5/78) Nuc.News (7/78) Wash.Post (1/28/79)	Science (2/79)[a]	New Wind Appendix (2/79)
Coal	O:	18-73	same	same	73	~18-73	--
	P:	20-2012	same	same	2010	~20-2000	--
Oil	O:	2-18	same	same	18	~2-18	--
	P:	9-1920	same	same	1920	~9-1900	--
Meth-anol	O:	920-1270	268-617	222-348	1270	~220-350	--
	P:	0.1-0.4	same	0.05-0.14	0.4	~0.12	--
Wind	O:	214-283	same	217-287	282	~100-130	104-129
	P:	22-539	same	same	539	~8-540	4.6-130
Solar PV	O:	141-188	same	same	188	~140-190	--
	P:	10-512	same	same	511	~10-510	--
Solar Thrml	O:	60-101	same	62.6-103	101	~60-100	--
	P:	8.9-510	same	9.4-515	510	~10-510	--
Solar Heat	O:	91-103	same	same	103	~90-100	--
	P:	4.6-9.5	same	same	9.5	~5.0-10	--
Hydro	O:	22.8-34.5	same	same	--	~20-35	--
	P:	7.6-12.4	same	same	--	~7.6-12	--
Ocean Thrml	O:	23-30	same	same	30	~25-30	--
	P:	0.9-1.4	same	same	1.4	~0.9-1.4	--
Nu-clear	O:	1.7-8.7	same	--[b]	8.7	~1.8-8.7	--
	P:	0.3-1.5	same	--[b]	1.4	~0.3-1.5	--
Nat. Gas	O:	1.1-5.9	same	same	5.9	~1.1-5.9	--
	P:	0	same	same	0	0	--

* O = occupational worker-days lost; P = public person-days lost.
a Estimated from logarithmic bar chart.
b Two-thirds of Table D-2, which contains the figures for nuclear risks, was omitted from AECB-1119/REV-2.

TABLE 1.4
EVOLUTION OF INHABER'S DEATH FIGURES
(Deaths per thousand megawatt-years)

		----- AECB-1119 (3/78)	REV-1 (5/78)	REV-2 (11/78) -----	Lord Rothschild (11/78)	*Wall Street Journal* (4/24/79)	New Wind Appendix (2/79)
	(*)						
Coal	O:	2.3-10.1					
	P:	2.8-152	same	same	5-160	5-160	--
Oil	O:	0.19-1.9					
	P:	1.4-140	same	same	2-140	2-140	--
Meth-anol	O:	101	29-66	25-38			
	P:	0.004	0.004	0.002	--	--	--
Wind	O:	21-30					11-13
	P:	2.2-39.8	same	same	23-70	12-23	0.67-10.3
Solar PV	O:	15.8-21.2					
	P:	0.7-38.7	same	same	--	--	--
Solar Thrml	O:	6.9-11.4		7.3-11.8			
	P:	1.1-38.5	same	1.2-39.4	--	--	--
Solar Heat	O:	9-10					
	P:	0.4-0.44	same	same	9-10	8-9	--
Hydro	O:	1.9-3.6					
	P:	1.3-2.0	same	same	--	--	--
Ocean Thrml	O:	2.2-3.3					
	P:	0.1	same	same	--	--	--
Nu-clear	O:	0.23-1.32					
	P:	0.04-0.24	same	same	0.25-1.5	0.25-1.5	--
Nat. Gas	O:	0.08-0.4					
	P:	0	same	same	0.1-0.4	0.1-0.4	--

* O = occupational deaths; P = public deaths

limit-based index.

Most of the risks of the renewable technologies are derived from the risks for coal and oil. One-quarter of the total risk shown for coal is embodied in the risks of wind, photovoltaics, and solar-thermal-electric systems, as the misconceived "energy back-up". (The figure of 25 percent comes from the 19 percent energy "back-up" Inhaber decided would be needed in the U.S.,

multiplied by the factor of 1.32 insolation "correction factor" for Canada.) These coal risks dominate the risks attributed to these three technologies.

Risks from coal mining, transportation, and combustion are included in another way in the risks for all the renewable technologies except methanol (for which much of the risk is scaled from the risks of oil): coal mining for steel manufacture is generally the second-largest component of the materials acquisition risk (after the misapplied risks of fabricated metal products); and risks from coal combustion are included as "emissions" from that same coal when it used to reduce iron ore. Finally, the hazards of coal transportation are the standard Inhaber uses for computing the risks of transportation of materials and components for the renewable energy technologies. Nuclear power and natural gas rank low on Inhaber's risk scale in part because he did not attribute any of these "secondary" coal risks to them as he did to the renewables.

It will be noted that Inhaber's "overall ranking" of the technologies is the same whether they are ranked by lost person-days or by deaths. There are two simple reasons for this. First, the figures for lost person-days <u>include</u> the deaths, counted at the rate of 6000 lost person-days per death. As a result, the lost person-days attributable to deaths actually dominate the totals for lost person-days. (See Section III.C.3, above.) A second reason is that the occupational deaths are actually <u>derived from</u> the occupational worker-days lost. It should not be surprising, therefore, that the rankings are identical.

The second and third columns of Tables 1.3 and 1.4 show the changes in the risk figures in the second and third editions of AECB-1119 (AECB-1119/REV-1, dated May 1978; and AECB-1119/REV-2, dated November 1978). What is most remarkable, considering the number of errors that cry for correction, is the tiny number of changes Inhaber made. The two revisions only show changes in the risk figures for three technologies, and for only one of these--methanol--were the changes significant.

The only change in the risk figures for the second edition was correction of a major mathematical error in the computation of operation and maintenance risks for methanol. In the first edition Inhaber had overstated these risks by a factor of four,[10] adding an extra 652 lost worker-days to methanol's

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(10) Inhaber had concluded that production of one megawatt-year of energy required 24.7 worker-years of logging, but he copied un-

risks. The second edition removed this error, although no mention was made that any of the risk figures had changed, or that methanol's "overall ranking" had changed from third worst to fifth worst.

The third edition made another major correction in methanol's risks, again without any mention of the change. This time Inhaber removed part of his spurious correction factor relating to methanol's end-use efficiency. (This is explained fully under "The methanol mix-up", Section III.G.2, above.) Again, this change had a major effect on the values for methanol's risk, dropping it one more place on the "overall ranking" of riskiness.

The other changes Inhaber made in the third edition were minor. Two of them appear to have been stimulated by the IAEA review (Black *et al*. 1978). That review pointed out disagreements between the text and tables that affected risk in construction of windmills and transportation of materials for solar-thermal-electric plants. In addition, Inhaber corrected a mathematical error in the number of deaths of roofers constructing the solar-thermal-electric plant. All of these minor changes produced slight increases in the risk figures.

The last revision of AECB-1119's risk figures (of which we are aware) is contained in the draft of the new wind appendix sent to us in February 1979. In this draft Inhaber has changed the materials requirements and the load factor, reducing the occupational risks for wind by about a factor of two. He has also incorporated a new method of computing "energy back-up" risks, apparently using a "mix" of conventional technologies to provide the back-up energy, instead of coal. Of course, this fails to fix the back-up problem, since inclusion of any risks from this source is simply incorrect in the context of the near-future applications of wind (see Section III.B.2, above).

The published summaries of AECB-1119 provide four different sets of risk figures. The most commonly used set is that given in the Executive Summary, which simply presents the upper limits of the ranges of public and occupational risk for each technology as computed in the first edition of AECB-1119.

The second set of published figures was Lord Rothschild's sample of death

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changed the statistics he cited for risks per 100 worker-years.

statistics. Only two of the renewable energy technologies were mentioned in this sample, and the figures given correspond to the three editions of AECB-1119.

The third set of data to appear was that published in Science on 23 February 1979. In his summary article there, Inhaber gave no numbers for risks, but he did provide bar charts similar to those in AECB-1119. Reading exact figures from these logarithmic charts is impossible, but the only clear difference between these charts and the figures in the third edition of AECB-1119 is in the wind risks. The occupational risks of wind appear to have been adjusted to correspond to the draft wind appendix, but the public risks have not been similarly altered.

The last compilation of risk figures appears in a Wall Street Journal essay on the controversy surrounding the Inhaber affair (Weaver 1979). There Inhaber presents death figures for wind and solar space heating. The wind risks correspond to those given in the draft appendix. The solar heating risks have been lowered slightly from those in AECB-1119, but do not correspond to any document known to us.

IV.B. EFFECTS OF ERRORS ON AECB-1119'S CONCLUSIONS

This subsection presents the results of partial correction of the errors identified in the preceding parts of this chapter and in Chapters 2 through 10. It must be emphasized that we have not been able to correct all of AECB-1119's errors by any means. As we noted earlier, some of the defects of that report as a guide to relative risk of energy technologies stem from the limited subset of categories of harm considered at all (risk to occupational and public health from accidents and a small selection of pollutants). We cannot blame Inhaber for this shortcoming (except for lack of attention to the implications of the omissions), since it is clear that the restricted task he undertook was already far beyond the resources he was prepared or able to devote to the job. Neither did we try to expand the coverage of the study here, except to call attention to the possible significance of what was omitted. Trying to unravel the sins of commission was more than chore enough. (But for extended discussion of effects of renewable technologies in the categories not considered by Inhaber, see Holdren 1978 and Holdren, Morris,

and Tanenbaum 1979).

Among the conceptual problems in AECB-1119, some are easily enough corrected quantitatively that we did so. Many of the conceptual and methodological mistakes, however, could not be corrected without doing the entire study again from scratch. The conceptual problems with the treatment of occupational risks as gross rather than net effects, and the uniform use of 6000 person-days lost per death, fall in this category.

Inhaber's decision, inconsistent with the stated groundrules of his study, to force wind and photovoltaics into a baseload role by the addition of large (not to mention irrationally computed) quantities of back-up and storage, we have corrected simply by omitting back-up and storage risk for these technologies (as appropriate for their actual near-term application in a "fuel-saver" mode). Back-up risks for the baseload solar-thermal-electric plant we also omitted, since the energy storage provided for this plant is so large as to make it more than a match for the reference coal and nuclear plants as an annual energy producer.

Intricate combinations of category mistakes--for example, the use of inappropriate Standard Industrial Classification categories for the jobs associated with creating and running energy facilities--we have generally not tried to correct in detail. Doing so properly would require a good deal more work than went into the original report. And, although it is clear from our discussions above and in Chapters 2 - 10 that most such mistakes in AECB-1119 have the effect of overstating the risks of unconventional technologies, getting really plausible estimates of the "right" numbers probably would require extensive use of input-output models of the U.S. economy--a task well beyond the scope of this critique. Similarly, we have not tried to replace AECB-1119's treatment of transportation (moving all materials 300 miles by train) with a more sensible procedure distinguishing different distances for different materials and different risks per ton-mile in different transport modes. Nor have we tried to construct a defensible treatment of public risk from emissions during materials acquisition.

The mistakes we have corrected quantitatively, then, are for the most part the misreadings and misrepresentations of literature, the straightforward internal inconsistencies and improper calculational procedures, and the

arithmetic mistakes. The results of these partial corrections are in no sense "right" answers about comparative health risks of alternative energy technologies, but they do show how drastically even the relatively elementary errors in AECB-1119 influenced the results.

Table 1.5 compares the occupational and public risks given in AECB-1119/REV-2 (the third and so far final edition) with the figures that result from our partial corrections. The derivations of the "corrected" figures are given in Chapters 2 through 10. Table 1.5 does not include natural gas or hydropower, because AECB-1119's treatment of these technologies is so deficient in usable data that even partial corrections would require more effort than we thought warranted. The table lists the technologies in order from best to worst, ranked according to the sum of the upper limits of worker-days lost and public person-days lost per megawatt-year of output on the basis of the figures in AECB-1119/REV-2. (As we have emphasized, the addition of occupational and public risks is conceptually unsatisfactory. We have done the addition implicitly only to help the reader trace how our partial corrections change the results as Inhaber presented them.) The third column in Table 1.5 shows the ratio of AECB-1119's upper limits to the upper limits after our partial corrections.

The conclusions that emerge from Table 1.5 need emphasis. Correcting only the misreadings and misrepresentations of literature, straightforward internal inconsistencies, improper calculational procedures, and arithmetic mistakes we have found in AECB-1119 reduces the upper-limit estimates of risk given in the third edition for unconventional energy sources by factors of 6 to 50 in the occupational category and by factors of 1.6 to almost 1000 in the public category. The upper-limit estimate of nuclear risk in AECB-1119, by contrast, is increased by these corrections by a factor of 47 in the public category. The (conceptually flawed) ranking of energy sources on the basis of total (occupational plus public) person-days lost per megawatt-year is thoroughly scrambled by these corrections, with nuclear falling from best of the sources listed in the table to third from worst if the ranking is based on the upper limits. If the ranges of the partly corrected values are considered, rather than just the upper limits, all the renewables look comparable to or better than nuclear in terms of both public and occupational risk, with the possible exception of solar heating (which remains a silly choice of

TABLE 1.5
EFFECTS OF PARTIAL CORRECTIONS ON AECB-1119'S CONCLUSIONS
All figures in worker-days lost (WDL) or public person-days lost (PDL) per megawatt-year of output, rounded to two significant figures.

	AECB-1119[a]	Our partial corrections	Ratio of upper limits[b]
Nuclear			
WDL	1.7 - 8.7	3.1 - 12	0.73
PDL	0.3 - 1.5	0.3 - 70	0.021
Ocean Thermal			
WDL	23 - 30	2.2 - 4.6	6.5
PDL	0.8 - 1.4	0.40 - 0.90	1.6
Solar Heating			
WDL	91 - 100	11 - 17	5.9
PDL	4.6 - 9.5	2.1 - 5.5	1.7
Methanol			
WDL	220 - 350	6.3 - 6.6[c]	53
PDL	0.05 - 0.14	0.005- 0.015	9.3
Solar-Thermal-Electric			
WDL	62 - 100	7.4 - 15	6.7
PDL	9.4- 520	1.0 - 2.7	190
Photovoltaic			
WDL	140 - 190	5.0 - 14	14
PDL	10 - 510	0.9 - 2.2	230
Wind			
WDL	220 - 290[d]	9.7 - 10	29
PDL	22 - 540	0.21 - 0.58	930
Oil			
WDL	2 - 18	3 - 19	0.95
PDL	9 - 1900	9 - 1000	1.9
Coal			
WDL	18 - 73	19 - 43	1.7
PDL	20 - 2000	20 - 1500	1.3

a These figures are based on the totals presented in the Appendices of AECB-1119/REV-2; where Inhaber made errors in summing the components of these risks, his incorrect sums are shown here.

b AECB-1119 figure divided by our partially corrected figure.

c Excludes risks of creating methanol facility, since no data actually relevant to this category were provided in AECB-1119 (See Chapter 10).

d Figures in new typescript wind appendix are 100-130 WDL and 4.6-130 PDL.

technology for the environment in which Inhaber placed it).

One may wonder, of course, since our corrections do not repair all the problems in AECB-1119, whether additional corrections might cancel some of the effects of those we have made and thereby worsen the relative position of the renewables compared to nuclear power. This is conceivable but not likely, since most of the other errors we have identified in AECB-1119 (in the earlier parts of this chapter and in Chapters 2 - 10), but were not able to correct quantitatively within the constraints of this critique, can be seen qualitatively to inflate the relative risks of the renewables.

Inhaber's repeated assertion that correction of the errors in AECB-1119 leaves the overall rankings and general conclusions essentially unaltered (Inhaber 1978d, 1979b, 1979c) is therefore seen to be completely without merit. The only conclusion of AECB-1119 that survives correction of its errors is the unexceptionable one that renewable energy sources are not risk-free. That has been said before and better by many others (see, e.g., Holdren and Herrera 1971, von Hippel and Williams 1975; Ehrlich, Ehrlich, and Holdren 1977), and responsible contributions to the literature exploring the quantitative dimensions of these risks are appearing with increasing frequency. Inhaber's report is not one of them.

V. THE PROPAGATION OF MISINFORMATION: HISTORY AND IMPLICATIONS

We have shown in the foregoing sections that AECB-1119 suffers from such massive errors of both omission and commission that most of it is unusable even as input to a sensible study of the hazards of energy alternatives. The report's coverage is not merely incomplete, but nonuniformly so from one technology to the next. The data sources drawn upon are far fewer than claimed, and most of those actually used have been misunderstood or misrepresented. The heart of the author's approach is not original but was borrowed wholesale from an internal memorandum from an earlier study by others, errors and all. The material added by Inhaber in trying to adapt and extend earlier comparisons to include additional technologies--namely, wind, hydropower, OTEC, methanol, and flat-plate solar heating--is so riddled with errors that his contribution to the field must be considered not merely small but negative.

The entire report, in all its editions, is shot through with conceptual confusions, inappropriate selection of systems and data, improper calculational procedures, and untenable assumptions and contentions, many of which are obscured beneath a layer of gross printing errors and arithmetic mistakes.

Yet this document was circulated all over the world by the sponsoring agency, cited approvingly in lectures and articles by prominent figures in the intellectual community, and published in summary form in three internationally circulated technical journals. How did it happen? What can be learned from this sequence of events, as regards (for example) the reliability of the peer-review process and the circumstances under which nonsense can propagate and become entrenched? In this section, we consider the history of the Inhaber affair in the hope of shedding some light on these questions.

V.A. IN THE BEGINNING WAS THE EXECUTIVE SUMMARY

AECB-1119 made its public debut on February 22, 1978, in the form of a press release, AECB Information Bulletin 78-1. The material released at that time consisted of an Executive Summary of the report, written by Inhaber, preceded by a "Preface to the Full Report", signed by AECB Chairman Dr. A.T. Prince. The actual release of the full report did not follow until April, although the first edition is dated March 1978. Thus it appears that the Atomic Energy Control Board had learned nothing from the earlier unfortunate experience of its sister agency, the U.S. Atomic Energy Commission (later split into the Energy Research and Development Administration and the Nuclear Regulatory Commission): the AEC was rightly excoriated in 1974 for announcing the conclusions of its Reactor Safety Study (the Rasmussen Report) before the document containing the calculations was available for scrutiny by the technical community and the public.

Dr. Prince's preface was enthusiastic in summarizing what had been accomplished by Inhaber's study. We reproduce his first two paragraphs here in full:

> "The risk to the health and safety of workers involved in the production of energy by various systems, as well as to members of the public, is an important consideration in choosing one system over another. At the Atomic Energy Control Board, we are focussing not only on ensuring

that the risk from the nuclear system is as low as reasonably achievable, but also on what might constitute an acceptable level of risk.

"One approach to the problem is to compare estimated risk for other energy systems in a systematic and consistent fashion. The results of such an exercise are reported in the following pages. Based on data compiled from almost 150 references, the author has shown that electricity provided by natural gas involves the lowest risk, and nuclear energy has a fairly low risk compared to other sources."

Later he adds:

"The author has developed a new methodology for processing and interpreting available data."

As we have seen, Inhaber's approach was neither systematic nor consistent, and his methodology was not new. Recipients of the press release, however, had no way to know this, and it is hard to doubt that the endorsement of the President of the AECB was taken by many at face value.

Inhaber's accompanying Executive Summary is undiluted by any of the qualifications and caveats that found their way into later editions. Nor is he bashful in his own claims of a pioneering contribution. He writes in the first paragraph:

"For the first time, a comparison is made between non-conventional technologies - like solar, wind, and methanol - and conventional technologies - like coal, oil and nuclear power."

He must have known this claim to be false, because AECB-1119 cites repeatedly and uses extensively the Jet Propulsion Laboratory study, published in March 1977, which compared electricity generation using coal, oil, nuclear power, and three kinds of solar technologies (solar-thermal-electric, terrestrial photovoltaic, and orbiting photovoltaic).

The Executive Summary contains one figure and one table, each showing only the upper limits of Inhaber's risk estimates for the various technologies, with no breakdown of the contributions to these risks by materials acquisition, construction, operation and maintenance, and so on. (Use of the

upper limits maximizes the impression of nuclear power's advantage over the renewables, not only because the upper limits Inhaber found for the latter are inflated by multiple errors, but also because the nuclear "upper limit" is not really that at all.) Nowhere in the 6-page Executive Summary is there any mention that the bulk of the upper-limit risk of wind, photovoltaics, and solar-thermal-electric power comes from the assumed coal back-up; indeed, back-up is not mentioned at all. Inhaber claims instead that the public risk from wind, solar-thermal-electric and solar photovoltaic "is mainly due to air pollution caused by smelting steel, a required metal for these technologies". This claim is false, and, if the author did not know it to be so, it could only have been because he wrote the Executive Summary without looking at (or recalling the contents of) his report.

Inhaber had no trouble remembering the conclusion he wished to convey in the Executive Summary, however, and one waits only until the second paragraph (reproduced here in full) to get it:

> "Natural gas used to generate electricity had the lowest overall risk of the technologies considered, followed by nuclear power and ocean thermal. Somewhat surprisingly, the seven other technologies had significantly higher risk. In some cases, they are over 100 times more dangerous in terms of accidents, disease or death. In other words, far more risk is caused if a system like solar space heating is chosen, instead of safer systems."

The Executive Summary of the Rasmussen Report, subsequently withdrawn by the U.S. Nuclear Regulatory Commission because it did not accurately represent the underlying report, was by comparison a model of care and restraint.

V.B. EARLY CIRCULATION AND RESPONSES

Inhaber and the AECB lost no time seeking wider circulation of his results, evidently even before the report was actually made available to the public. The April 1978 issue of Nuclear Engineering International carried a news item summarizing the report's conclusions, and the 18 May 1978 issue of New Scientist carried a three-page article by Inhaber himself (Inhaber 1978b) entitled "Is solar power more dangerous than nuclear?" (Given the printing and publication schedules of journals such as these, it is reasonable to suppose

that they received the materials no later than March.) Nuclear Engineering International carried a larger, favorable piece on Inhaber's results in its May issue (Knox 1978). Inhaber, meanwhile, had presented his conclusions at the Organization for Economic Cooperation and Development's Seminar on Environmental Assessment of Energy Supply Scenarios in Paris in March, and he submitted the Executive Summary quoted above as his paper for the Proceedings. The U.S. journal Nuclear News favorably summarized the conclusions of AECB-1119 in the news section of its July issue (Nuclear News 1978).

The first objections of substance (of which we are aware) were raised in Canada. The June issue of the French-language monthly Quebec Science carried a long article by journalist Gilles Provost (Provost 1978), the title of which translates "Camouflaging Nuclear Risk: Dissection of an Atomic Energy Control Board Report on the Risks of Different Forms of Energy". Provost clearly had undertaken quite a detailed review of AECB-1119, and he found many of its major errors. His article says, for example (in translation):

> "[For methanol] the author of the study calculates the dangers of factory operation twice without noticing it and he compares, nobody knows exactly why, electric power plants with methanol used as car fuel. He also assumes rather arbitrarily that a methanol factory would be as dangerous as a petroleum refinery but that its useful life would be 10 years shorter (20 years instead of 30)."

> "[I]t is strange that, according to this study, for equal amounts of energy, the maintenance of a hydroelectric power station is 10 to 15 times more dangerous than the maintenance of gas, petroleum, or nuclear power stations, while the latter are considerably more complicated and vulnerable to breakdowns."

> "Of course, the back-up system chosen is based on coal, and, with the most pessimistic assumptions about its dangers, the erroneous conclusion is that 85 percent of the purported dangers of an electric solar power station really are related to the coal back-up system. In the case of wind energy, two-thirds of the dangers attributed to it come also from the coal back-up system."

Provost also pointed out that AECB-1119 had compared contemporary nuclear technology with out-of-date coal technology, that the coverage of risks was incomplete and uneven among technologies, and that the treatment of occupational risks posed conceptual problems. He concluded that

> "all in all, the results of this study do not come close to achieving the initial objectives and the above-cited conclusions remain eminently debatable."

> "The study technique is biased due to the fact that it favors in an incredible way the systems which require a minimum of labor force and, as a result, which create very few jobs."

We cite Provost so extensively because a clear understanding of the technical detail and the correctness of this early critique is essential to place Inhaber's response in context. That response displayed a pattern that was to become very familiar to observers of the Inhaber affair. The pattern has four components: (a) indicate that the differences with the critic are of modest consequence and in any case not surprising, given the complexity of the issues; (b) attack the critic's qualifications, competence, and motivation; (c) misrepresent the specific criticisms and/or the parts of AECB-1119 that stimulated them; and (d) leave most of the identified errors uncorrected. Often elements (a) and (b) appear in different forums, and this was the case in the Provost exchange. For his response in the November Quebec Science, Inhaber wrote (Inhaber 1978e):

> "I would like to thank Mr. Provost (and his collaborator, Pierre Sormany) for having spent the time to study a large number of calculations and tables included in the report, a study very few people could do due to its length (about 150 pages).... It would have been surprising should Mr. Provost, in the course of his analysis, have agreed with all that is written in the report. The facts that we have at our disposal are not yet understood enough to expect a complete agreement."

The tone in Inhaber's letter to the Toronto Globe and Mail (November 17, 1978), responding to an article about the controversy by Jake Brooks, is rather different:

"What Mr. Brooks says about the report is, quite simply, untrue or misleading. He says that Gilles Provost of the journal Quebec Science 'thoroughly criticized' the report. This statement would tend to make readers think that the report had been discredited. True, Mr. Provost, a journalist, not a scientist, did criticize the report, but could make his points only by falsifying and distorting my data. His paper could never have appeared in any reputable scientific journal, based as it was on misrepresentation. My rebuttal will appear in the November issue of Quebec Science."

One has the impression that Inhaber thinks readers of The Globe and Mail will not read Quebec Science, and vice versa. More evidence is supplied below of Inhaber's tendency to dash off statements in one forum without regard to what is already in print under his name elsewhere.

V.C. THE AECB'S REVIEW PROCESS

Notwithstanding Inhaber's protestations, the suggestions that AECB-1119 might contain gross errors led to inquiries about the extent of the scrutiny the report had received before being released by the AECB. Journalist John Marshall of the Toronto Globe and Mail looked into the matter and wrote in the 14 November 1978 issue that at least two reviews solicited by the AECB had been negative (Marshall 1978; see also Brooks 1978). Specifically, he reported that Dr. Ken Tupper of Canada's National Research Council recommended "that the report as he saw it in draft form should not be published. NRC colleagues say his criticisms still apply to the published version." (Tupper had since retired.) Marshall also described as "critical" an assessment commissioned from an outside consulting firm, Lemberg Consultants Limited, and indicated that "Its recommendations were not accepted."

We have learned that researchers in the National Research Council informed Inhaber as early as September 1977 that his draft contained serious exaggerations of the materials requirements of solar heating systems. We have also learned that energy researchers not only in the National Research Council but also in the Department of Energy, Mines, and Resources and Atomic Energy of Canada, Limited called many other major errors to the attention of the AECB in the months following the report's initial publication in April; they were

(and some still are) reluctant to speak out publicly, not only because the AECB is a "sister" agency, but because of Inhaber's tendency to lash out wildly at his critics.

The review by Lemberg Consultants, interestingly enough, was commissioned after AECB-1119 had been sent to the printer, but before distribution. When Lemberg inquired what use his critique could be under these circumstances, he was told that his comments would be taken into account in preparing subsequent editions. Lemberg's 17-page report, dated 27 March 1978, was subsequently stapled together with Inhaber's 8-page response and given an AECB cover and report number (AECB-1131). Its distribution must have been very limited indeed, for Lemberg himself says he never received a copy. Lemberg's critique addressed itself almost entirely to the conceptual problems in AECB-1119, including the dominant role of risk of back-up coal in the risk of renewables assumed by Inhaber to require back-up, the noncomparability of occupational and public risk, the aggregation of death and injury statistics, and the failure to distinguish between gross and net risk (see our section III.B.3, above). He also indicated that assumptions at least as defensible as Inhaber's on crucial points would produce the opposite conclusions about relative risks of conventional and unconventional technologies.

Inhaber's responses to Lemberg fit the pattern. In the essentially non-circulated AECB-1131, he writes:

> "Lemberg Consultants is to be congratulated for a forthright and incisive analysis of AECB-1119 (to be termed "the report"). It would be impossible to expect perfect agreement with the many details of the report."

And the Acknowledgements to the third edition of AECB-1119 contains the line (REV-2, p. 43):

> "Rein Lemberg, of Lemberg Consultants Limited, Oakville, Ont., made a penetrating analysis of an earlier draft."

But in a letter to the Globe and Mail dated 20 November 1978, responding to Marshall's article, Inhaber has this to say about Lemberg (Inhaber 1978d):

> "Mr. R. Lemberg, who did a study of the report for the Board, is incorrectly described as a 'risks measurement expert'. He is an economist who has specialized in economic risk, not health risk. He makes an obscure point that the labour used to build energy systems could have been used to build something else. The formulas I used in terms of occupational risk were standard ones which have been used by risk experts in hundreds of scientific papers. Frankly, nobody here could figure out Mr. Lemberg's comments when he made them a year ago, and I still can't."

One wonders, on reading this, what happened to the claims of originality made earlier about Inhaber's methodology. On the other hand, the last few words in the passage quoted are entirely plausible.

As for his other "inside" critics, Inhaber implies in a letter to the *Probe Post* (Inhaber 1979d) that the official reviewers, one from the National Research Council and one from the Department of Energy, Mines, and Resources, were satisfied with the changes he made before publication. They were not. In the first of his two letters to the *Globe and Mail* he describes one critic at Energy, Mines, and Resources as a "junior official" who "has never made a written public analysis of the report in question." Apparently, the *private* written analyses communicated to Inhaber and the AECB do not count, although elsewhere Inhaber indicates he believes this is the way responsible scientific critics do business (Inhaber 1979e).

V.D. THE SPREADING CONTROVERSY

In late 1978 and early 1979, increased attention was drawn to AECB-1119 and its conclusions by several circumstances. Great Britain's Lord Rothschild made risk the topic of his Dimbleby lecture, nationally broadcast on the BBC, and summarized Inhaber's quantitative risk comparisons. The lecture was reprinted in *The Listener* (Rothschild 1978) and later, partially, in the *Wall Street Journal* (*Wall Street Journal* 1979). The exposure in the United Kingdom stirred up responses pro and con in *New Scientist* (*New Scientist* 1978, Inhaber 1978f), and *Nature* (*Nature* 1978a, 1978b). Meanwhile, Inhaber presented a summary of AECB-1119 at the International Forum on an Acceptable World Energy Future in Miami in November (Inhaber 1978g) and published another version (focusing on hydropower but summarizing the rest of his results) in the

December issue of Energy, The International Journal (Inhaber 1978c). And in late January 1979, journalist William Greider of The Washington Post wrote a long, editorial-page piece entitled "The Threat of the Killer Windmills", acknowledging the controversy over Inhaber's conclusions but presenting them nonetheless as worthy of serious consideration (Greider 1979). At this point, one would have had to have concluded that, controversy notwithstanding, Inhaber's work was becoming entrenched as a force to be reckoned with in the worldwide debate over energy choices.

Although two of us had concluded in September, after spending a few hours with AECB-1119, that it was unworthy of the attention of serious analysts and would surely sink without a trace (we were unaware at that time of the controversy in Canada, nor had we noticed the article in New Scientist or the items in Nuclear News and Nuclear Engineering International), by January it was clear to us that our optimism had been premature. Inhaber's nonsense was propagating like wildfire, and among the many claims he was making for his work to boost its credibility was that its analysis of nuclear risk was based substantially on the work of two of us. In the New Scientist, for example, Inhaber had written (Inhaber 1978b, p. 446):

> "... a survey was taken of the major papers in the scientific literature which had estimated aspects of nuclear risk, including a monograph written by a well-known nuclear critic, John Holdren of the University of California at Berkeley. For each component of risk, the highest value from the group of scientific sources was used."

And in the Executive Summary, one found (AECB 1978, p. 4):

> "In the case of nuclear energy, the low public risk value arises despite the conservative approach of taking estimates of its accident risk from a well-known opponent of nuclear power."

Since these and similar statements in AECB-1119 and virtually all summaries of it are false, and since they they were perhaps contributing to the increasingly widespread acceptance of this thoroughly incompetent and misleading work, we felt some obligation to try to set the record straight. On January 27, Holdren wrote to Nuclear News and on 5 February to the President of the Atomic Energy Control Board (with a copy to Inhaber) pointing out that AECB-

1119 had misrepresented Holdren's work and that of others. Both letters called attention to specific massive errors in the wind and methanol sections of AECB-1119 and indicated the types of additional pervasive errors that render the report's conclusions completely unusable. The letter to the President of the AECB concluded:

> "If your Board does not publicly repudiate the Inhaber report, I suspect your credibility as a technical body will be greatly damaged. I respectfully suggest you subject the report to a (belated) careful internal review, and draw your own conclusions."

Holdren's letter to Nuclear News was published in the March issue (Holdren 1979a) together with Inhaber's reply. Inhaber's letter was surprising, even when the nature of his earlier responses to critics is taken into account. He evidently decided that so direct an assault on his report could best be countered by a strategy that at least would confuse the majority of readers of Nuclear News: brazen, vigorous denial of wrongdoing. After all, few readers could be expected to take the time to dig up and check the relevant documents, and, as long as they did not do so, Holdren's assertions and Inhaber's denials would simply leave the impression of unresolved controversy--hardly unusual in the energy debate. Discussions with many colleagues inside and outside the nuclear community indicate that this strategy of Inhaber's has been substantially successful. We therefore will take the necessary space here--an amount which Nuclear News felt it could not make available--to expose some of Inhaber's deceptions by direct comparison of the relevant passages.

The piece in the July 1978 Nuclear News to which Holdren's letter was responding is a short summary of Inhaber's conclusions in the "International" section, apparently written by the journal's staff. It contains the following passage (Nuclear News 1978, pp. 77-78):

> "... the risk connected with waste disposal was calculated only in the case of nuclear power, and this was conservatively estimated, using pessimistic, and therefore relatively high, data supplied by a nuclear critic."

Holdren's response o this point, as published in Nuclear News, reads as follows (Holdren 1979a):

> "As the 'nuclear critic' the article says supplied the 'pessimistic, and therefore relatively high' data for Dr. Inhaber's calculation of the risk of nuclear-waste disposal, I feel obliged to set the record straight on a number of points.... Although Inhaber does indeed reference some 30 times a report I coauthored (K. Smith, J. Weyant, and J. Holdren, "Evaluation of Conventional Power Systems", 1975), we offered no quantitative estimates in that report--or elsewhere--on the health impacts of long-term storage or disposal of radioactive wastes. Our estimates of occupational and public health effects of waste management were for spent-fuel transport and reprocessing only, came directly from WASH-1224, and were clearly referenced as such."

Inhaber's reply in the same issue dealt with the point this way (Inhaber 1979e):

> "With respect to radwastes, the data in my report was taken directly from Holdren, who, as he notes, took it from report WASH-1224. Footnote (d) of my table D-1 indicates that I copied Holdren's original footnote on the risk from radwaste almost verbatim, noting that it included only shipping and reprocessing. I do not know what more could have been done.
>
> "With respect to the question of long-term storage of high-level wastes, no explicit statement was made in my report to the effect that it was included."

The first two sentences of this reply are damning enough, for they show that Inhaber knew waste disposal was not included, but chose to tell his readers this only in a footnote to a table in an appendix, while misleading them in the text of the appendix, the main body of the report, and in all published summaries of it (see Chapter 3, below). The last sentence is simply false; the initial wording used by Nuclear News came virtually verbatim from the Executive Summary that appeared under Inhaber's name in February 1978 (AECB 1978, p. 6):

"Risk from waste disposal was calculated only in the case of nuclear power. It was conservatively estimated using pessimistic and therefore relatively high data from a nuclear critic."

As shown in Chapter 3, the only noticeable difference between this passage and those appearing in later editions and journal summaries of AECB-1119 is that the words "waste disposition" and "waste management" replace "waste disposal".

Inhaber's reply to Holdren in Nuclear News also employs a pea-and-shell game--Which edition of AECB-1119 has the error?--that has become a bastion of his subsequent defenses. Holdren wrote (Holdren 1979a):

"Inhaber's biomass figures are uniformly inflated by a factor of 8.33 (on top of various other mistakes), because he multiplied by that factor (1/0.12) to 'correct' for an assumed 12 percent efficiency of conversion of chemical energy to mechanical work delivered to the wheels of methanol-burning vehicles."

Inhaber's reply on this point reads (Inhaber 1979e):

"With respect to methanol (or biomass), the value of 8.33 which Holdren quotes is not to be found in section J-i of the third and latest edition. Perhaps he is using an earlier version. An error relating to possible double-counting of conversion losses was noted some months ago, and was corrected in the third edition. This change lowered methanol risk, but did not change its over-all ranking."

In reality, the factor of 1/0.12 = 8.33 is in section J-i of the third edition, although its role is somewhat obscured by gross printing errors. (See Section III.G.2 above, where the passage is quoted in full.) Inhaber also knew perfectly well that the factor was in the first two editions; there is no "perhaps" about it. Inhaber had indeed managed to remove in the third edition his earlier gross mistake confusing chemical energy in oil with electricity made from oil, an error he now disingenuously describes as "relating to possible double counting of conversion losses". The net inflation associated with this particular point then became 0.36 x 8.33 = 3.0, a reduction Holdren did not credit to Inhaber in his letter for the March Nuclear News because he had not yet seen a copy of the third edition at the time the letter was

written.[11]

Inhaber's claim, in the third sentence of the passage quoted above, that his partial correction did not change the relative ranking of methanol, introduces another theme repeated in his later defenses. His rankings are based, as we have seen, on the compounding of dozens of errors. Few of these individually are large enough that correcting one will alter a ranking, and most of those that are (e.g., the nuclear-accident risk "upper limit", the treatment of coal "back-up") Inhaber has refused to correct. As shown in our Section IV, however, correcting very many of his errors together rearranges the rankings completely.

Yet another of the countermeasures Inhaber trots out again and again in his attempts to neutralize his critics is his isolate-the-critic technique. In the March Nuclear News one finds (Inhaber 1979e):

> "The document has been publicly available for almost a year, and I am told has been carefully read by many people, especially those who are not happy with its conclusions. Professor Holdren is the first, to my knowledge, to make such sweeping public statements about the mathematics. Can he be the only one to discover such a series of errors?"

This statement misleads, while narrowly skirting the edges of outright prevarication. As we have seen, there were damaging criticisms of AECB-1119's approach and data selection by researchers in sister agencies and by a commissioned consultant, both before and after the report was distributed. An August 1978 review at the International Atomic Energy Agency (Black et al.

(11) After Holdren pointed this out in a follow-up letter in the April 1978 Nuclear News, Inhaber triumphantly responded in the May issue (Inhaber 1979b): "He admits that he was using the first edition of the report for his claims." Of course, it was the first edition that was summarized in the news item that stimulated this exchange. If Inhaber was unwilling to have the first edition criticized by those to whom he had not sent later ones, moreover, one wonders why none of the summary articles published under his name over the period from May 1978 to February 1979 inform the reader that there are "wrong" versions of AECB-1119 in addition to the "right" one presumably being summarized. (His December 1978 article in Energy identifies AECB-1119 as a March 1978 document--the date of the first edition; the February 1979 article for Science gives simply 1978 as the date of AECB-1119 and does not say REV-1 or REV-2.)

1978), cited extensively in Section III.G., above, and in Chapter 3, pointed out dozens of errors in AECB-1119, many of them in the mathematics; these criticisms, however, were somewhat hidden behind two introductory paragraphs of ritualistic congratulation of Dr. Inhaber on his brave attempt, and they were not made publicly. Neither were those of Amory Lovins, who had written to the Atomic Energy Control Board in August 1978, informing them that AECB-1119 "is the most unimpressive technical paper I have read in many years notable for exploration of the murkier backwaters of the literature" (Lovins 1978), and suggesting that the AECB withdraw the report. The commissioned review by Lemberg Consultants was highly critical of the method but did not attack the mathematics. The critique by Provost in *Quebec Science*, discussed above, complained of "gross errors" and "careless mistakes", but also did not specify that these included mathematical errors. Rereading Inhaber's denial in this context reveals an attention to detail in wording ("... sweeping *public* statements about the *mathematics*...", emphasis added) that would have been better spent on the original report.

Of course, one must at least marvel at the audacity of an individual who, after the performance recounted in the preceding sections and in the midst of his response in *Nuclear News* to Holdren's strongly worded critique, could write (Inhaber 1979e):

> "I would like to eliminate any errors or misstatements in the report, but it should be done in a rational and even-tempered manner. The scientific method of analysis, critique and re-analysis produces the most accurate work."

V.E. THE SCIENCE ARTICLE AND SEQUELAE

If it was frustrating for those knowledgeable in risk assessment to watch Inhaber's nonsense propagating in nuclear trade-association journals and the popular press and by policy makers, it was nothing less than shocking to see it slip through the peer-review barrier into (presumably) refereed scientific journals: *New Scientist*, *Energy*, and finally, in late February 1979, *Science*. The last is the pre-eminent general scientific journal in the United States; it is relied upon not only by the technical community but by the popular press, nationwide and beyond, for reliable information on scientific and

technical issues. For work linking science and public policy, publication in Science provides a stamp of respectability unequaled save perhaps by publication in the United Kingdom counterpart, Nature. This fact was not lost on the Atomic Industrial Forum, whose February 1979 newsletter crowed (Info 1979):

> "A study by the Atomic Energy Control Board of Canada, showing that nuclear is safer than solar and most other alternative energy systems, is scheduled to be published in the Feb. 23 or March 2 issue of Science magazine.... Publication in Science indicates that the Inhaber report has undergone still another peer review process."

What happened to the peer review process, that it permitted this debacle? Dr. Philip Abelson, editor of Science, indicated in a letter (23 March 1979) to one of us that Inhaber's article "went through the usual peer review and received the highest marks." We understand there were two referees, although of course we have no way of knowing who they were. We will, however, venture the following opinions about what happened:

(1) The referees either were not knowledgeable in the field of environmental comparisons of energy technologies, or they were not paying attention. Even though the article contains not a single example of the calculations on which its conclusions purportedly rest, it does contain enough misstatements and confusions (not to mention serious anomalies in the conclusions) to boggle the mind of any reader both careful and experienced in the field.

(2) The referees did not obtain--or if they obtained did not scrutinize--the report to which readers of the article are referred for all the details, namely AECB-1119. Whether experienced in energy technology and environmental science or not, no one with a decent technical education could possibly read, say, the methanol calculations in AECB-1119 without realizing that their author did not know what he was doing.[12] It is, of course, most unusual for referees to recommend publication of an article

(12) A number of undergraduate and masters-level students at Berkeley independently discovered many of Inhaber's bigger blunders, and a Stanford masters candidate in engineering teamed with an undergraduate economist to write a devastating critique, sent to Science as a letter to the editor (Hernandez and Shuman 1979) before ever hearing of our own work.

that simply asserts its conclusions without showing any calculations, even if the author is a recognized authority in the field under discussion. To our knowledge, Inhaber had not been heard of outside the AECB, in the field of comparative risk of energy technologies, before the appearance of AECB-1119.

We reluctantly conclude that the referees did not do their job. Presumably they simply never imagined they would be sent an article that so thoroughly misrepresented how its conclusions were derived, nor that the underlying report might contain dozens of serious quantitative blunders that would vitiate its conclusions even if methodological mistakes had not. Their faith was, alas, misplaced. As for giving the article the "highest marks", they must have found its conclusion--that nuclear power is safer than renewables--appealing enough to make up for the lack of evidence.

Not surprisingly, *Science* received a number of letters from outraged readers, some with prior knowledge of Inhaber's work. Richard Caputo, director of the Jet Propulsion Laboratory project from which Inhaber borrowed so liberally, indicated in a letter published in the 4 May 1979 issue of *Science* that he had discussed with Inhaber in 1978, after receiving a copy of AECB-1119, the "enormous differences" between Inhaber's results and those of the JPL work on which they were based. He wrote (Caputo 1979):

> "About half of [Inhaber's] source material and the methodology he claimed as his own is taken from work I technically directed or had contracted at the Jet Propulsion Laboratory.... He indicated that he had added a few things that were left out of the JPL analysis but did not identify even in a general way what these left-out factors might be. Since I had spent 3 years developing the data and had had the assistance of about 20 professionals, I expressed skepticism and advised him not to publish any further without checking his analysis."

Obviously, that advice was not taken. The same issue of *Science* contains a letter from Rein Lemberg, recounting the methodological objections in his commissioned review of AECB-1119, and noting that none of Inhaber's revisions have been responsive to those objections (Lemberg 1979). Our own letter to *Science*, summarizing some of the errors we have elucidated at greater length here, appeared in the 11 May 1979 issue (Holdren, Smith, and Morris 1979).

Science received a number of other letters, which were not published, from researchers in the fields of renewable energy technology and environmental comparisons. These letters suggest that our dismay over the quality of Inhaber's work and its publication in Science is widely shared. Drs. D.W.O. Rogers and R.J. Templin of the Energy Project and Low Speed Aerodynamics Laboratory of Canada's National Research Council began their letter to Science (Rogers and Templin 1979a):

> "We wish to draw your readers' attention to some of the distortions, biases and errors contained in a study of risk from energy sources recently reported by H. Inhaber (Science, 23 Feb, 1979). Although we see problems throughout the report, we will restrict our comments to two areas in which our institution has special expertize, viz the design of solar space heating and wind power systems. Correcting errors in these areas leads to order of magnitude changes in Inhaber's 'risk'." (Emphasis in original.)

Dr. Michael Yokell of the Solar Energy Research Institute (the U.S. national solar laboratory) is well known in the field of environmental assessment of energy technologies for his ongoing studies of environmental costs of solar energy using the Strategic Environmental Assessment System (SEAS)--a large computer model and data base designed for exploring energy/environment/economy interactions. Yokell wrote to Science (Yokell 1979):

> "I am both surprised and deeply disturbed that a journal of Science's caliber would publish 'Risk with Energy from Conventional and Nonconventional Sources' by Herbert Inhaber, which appeared in the issue for February 23.... The work reported in this article is deeply flawed methodologically.... Inhaber's report seriously biases the comparison between conventional and nonconventional sources of energy against the nonconventional sources."

Dr. Jerome Weingart, former Visiting Scholar in solar energy at the International Institute for Applied Systems Analysis in Vienna and now Participating Guest at the Energy and Environment Division of the Lawrence Berkeley Laboratory, described the results of his own review of Inhaber's work in the context of its references as follows (Weingart 1979a):

"In this review I have encountered literally dozens of serious errors in procedure, methodology, computations and data.... these examples should serve to alert anyone with an interest in these matters that Inhaber's numbers cannot be used reliably in any serious discussion of the risks associated with nonconventional energy sources."

Nor were years of experience in the field of energy and environment necessary to reach the same conclusion. The long critique sent to Science by two Stanford students ended as follows (Hernandez and Shuman 1979):

"It is a pity that our effort, and that of others whose time is much more valuable, has and will have to be expended to undo the damage Inhaber has done to the objective analysis of the risk of energy technologies. We urge Science to more carefully screen unsolicited manuscripts before granting their authors the privilege of credibility."

The only response by Inhaber to the letters actually published in Science (those by Lemberg, Caputo, and Holdren et al.) that we have seen so far is a "Memo to File" replying to the first draft of the Holdren letter, which was sent to him in March. This 17-page memo (Inhaber 1979c, hereafter denoted the Memo) contains all the ingredients of Inhaber's earlier responses to criticism, including:

(a) Misrepresentation of the criticisms themselves, accomplished with the aid of "paraphrasing". ("I will take the liberty of paraphrasing Holdren's statements so they are briefer.")

(b) Misrepresentation of what was done in AECB-1119. (See our discussion of methanol end-use in subsection III.G.2, above, which quotes in full such a misrepresentation in the Memo.)

(c) Misrepresentation of what is in the references. (See our discussion of nuclear accident risk in Chapter 3, which quotes such misrepresentations in the Memo.)

(d) Misrepresentation of the nature of acknowledged errors. (The use of risks scaled from oil-refinery operation and maintenance to represent construction of a methanol plant is explained in the Memo as "mislabelling" of the section in question; but the text of that section refers

repeatedly to construction. See also our discussion of the wind materials requirements in Chapter 7, which quotes another such misrepresentation in the Memo.)

(e) Misrepresentation of the persistence of errors. (Regarding the cement-concrete confusion, the Memo reads "In some early editions of the report, there was some mislabelling of the two materials." In all three extant editions of AECB-1119, concrete and cement are treated as identical.)

(f) Misrepresentation of the size of the changes in AECB-1119's conclusions that would result from the correction of its errors. ("While the absolute values of the risk per unit energy have changed as a result of errors being eliminated, the relative rankings have changed only a minimal amount." As shown in Section IV, above, this statement depends on Inhaber's not having removed the biggest errors.)

(g) Bizarre analogies intended to belittle the critic. (On the Holdren letter's complaint that Inhaber should have used the wind load factor given in the reference he used for the materials requirements, the Memo says "The course he proposes is similar to that used in science before the 17th century, when matters were decided, not on the basis of evidence, but on what ancient texts said.")

These same tactics are also employed in Inhaber's final letter to Nuclear News, published in the May 1979 issue (Inhaber 1979b).

V.F. REACTIONS OF THE ATOMIC ENERGY CONTROL BOARD

Richard Caputo reported in Science (Caputo 1979) that, when he saw Inhaber's summary in New Scientist after having told Inhaber himself of gross discrepancies between AECB-1119 and its references, he wrote to each member of the Atomic Energy Control Board to complain about this evident propagation of error. The Board, wrote Caputo in Science, continued to "support" Inhaber.

More details of the nature of this support are provided by correspondences between Amory Lovins and Board President A.T. Prince and between Prof. Paul Ehrlich and Prince's successor, J.H. Jennekens, as well as by our own correspondence with the Board. Lovins, Ehrlich, and Holdren all called on the

Board to withdraw or repudiate AECB-1119, noting that the credibility of that agency could only be damaged by its continuing sponsorship of this incompetent and misleading document in so sensitive an area of public policy (Lovins 1978a, Ehrlich 1979, Holdren 1979b). The responses by Prince and Jennekens to these letters contained a curious mixture of: (a) praise for the originality, quality, and usefulness of AECB-1119, with emphasis on the favorable reviews received from others; (b) refusal to take full responsibility for the report's contents; (c) scolding of the writers (Lovins, Holdren, Ehrlich) for intemperate language or lack of substance in their criticisms; and (d) excuses for Inhaber's errors, emphasizing his difficulty in understanding the literature. Thus one found in Prince's response to Lovins of 15 September 1978 (Prince 1978):

> "I am only sorry that you were not disposed to make any substantive comments on [the report].... [Dr. Inhaber] tells me, however, that the corrections and amendments he has made or will make do not strongly alter his overall conclusions. These conclusions are, of course, his to defend, not mine.... The report was reviewed extensively within the Board, by two outside federal government energy scientists, and by a private consultant.... we have been gratified by the favorable opinions of experts highly respected in their fields, such as F. Niehaus of the International Atomic Energy Agency, and Sir Edward Pochin of the British National Radiological Protection Board."

As we have noted earlier, Niehaus' review (presuming this to be Black, Niehaus, and Simpson 1978) was devastating in its detailed comments, although (evidently for political reasons) it began with some rather ritualistic and nonspecific praise. Jennekens' response to Holdren of 27 February 1979 says:

> "Your only detailed comments are in your third and fourth paragraphs [about the wind and methanol errors], and I will relay Dr. Inhaber's replies. They are, of course, his to defend.... By way of defense, Dr. Inhaber mentions the difficulty of interpreting data relating to non-conventional energy systems.... With respect to the question of withdrawal, I see no purpose to be gained. An objective of the report, as Dr. Prince stated, was to stimulate debate on the relative risk of <u>all</u> energy systems. This, I feel it has done. However, this debate can be

productive to society only if it is kept unemotional and on a scientific level."

Concerning Inhaber's indisputable difficulties in understanding the literature of the risks of renewable energy sources, one wonders why he nevertheless chose to write and disseminate what purported to be a sweeping synthesis of the field. As for the Board's intention "to stimulate debate", there surely are better ways for a nation's nuclear regulatory body to do this than by circulating as an official document a morass of mistakes that greatly exaggerates the liabilities of competing energy sources.

In his letter of March 14 to Ehrlich, Jennekens declares that AECB-1119 made a pioneering contribution by comparing energy systems on "a common-denominator and total-impact basis" (a contention we show in Section III, above, to be completely without merit); argues that "in authorizing the publication of the Inhaber study, the Board did not endorse its conclusions or methods" (a disingenuous view, considering the push provided to the propagation of those conclusions by the AECB imprimatur); insists that AECB-1119 should be viewed merely as "food for thought" (a questionable diet, promoted as it was by a national regulatory agency); and complains that "the most vocal criticisms have been mostly not substantive". While the last statement is certainly untrue if meant to include, for example, the earliest of Holdren's criticisms (which stressed the massive methanol and wind errors), it does call attention to the issue of early critics who were substantive but evidently not vocal enough. Had they been more vocal, Inhaber might have been forced to make many more corrections than he actually did, and the larger community would have been alerted not to swallow his results as uncritically as most people actually did.

As to whether or not the Board should take responsibility for what it publishes, one wonders whether its members suppose they build confidence in their competence as a technical regulatory body when they disseminate between glossy AECB covers a report littered with elementary errors discoverable by any careful undergraduate. Certainly there can be little doubt that the AECB imprimatur deterred potential critics in agencies maintaining official relations with the Board from calling attention to the report's errors sooner, less equivocally, and more publicly.

The Atomic Energy Control Board, it is clear from these excerpts, is unrepentant--at least publicly. One can only marvel at the agency's willingness to squander its credibility in so unworthy a cause.

V.G. WHAT HAS BEEN LEARNED?

Has this exhausting (although still not exhaustive) dissection of the Inhaber affair been worth the trouble? We thought it necessary to undertake it for two reasons.

First, the evidently wide appeal of Inhaber's conclusions among proponents of "conventional" energy technologies has given these conclusions a life of their own. People seem unwilling to relinquish them without the most thorough demonstration that almost everything about the underlying calculations is wrong. This is an extraordinary burden of proof to place on a critique; ordinarily, elucidation of only a few errors, if they are gross enough, is considered sufficient to disqualify a technical document from further serious attention. We accepted the burden nonetheless, because the subject matter sits so squarely at the heart of present national and international debates about energy choices that persistent misinformation could do great harm.

Second, we believe the Inhaber affair is one of the more compelling demonstrations of this decade of the surprising vulnerability of the intellectual community to the propagation of misinformation. As a much more extreme case than most, the Inhaber affair provides an unusually good vehicle for illuminating the extent of this vulnerability and the reasons for it.

We trust that the preceding material in this chapter and the additional detail in Chapters 2 - 10 will suffice to meet the burden of proof that was our first motivation. In the remainder of this section, we discuss the issue of vulnerability and what we think the Inhaber affair reveals about it.

In many of the most important issues facing contemporary society, accurate information about the technical characteristics of the alternatives is necessary for rational decisions but not sufficient. Making sensible choices about such issues as nuclear weapons, energy, and public health requires not only technical information (in which we would include inputs from physical science, technology, and economics), but also a balancing of interests,

preferences, and values of the affected groups and individuals. This characteristic makes the political process the proper place for deciding these matters, notwithstanding the importance of their technical dimensions (see, e.g., Primack and von Hippel 1974, Holdren 1976). Just as war is too important to be left to the generals, so are nuclear weapons too important to be left to the physicists, energy too important to be left to the engineers, and public health too important to be left to the physicians.

While the technical community, then, cannot and should not determine public policy on such issues, it nevertheless bears a heavy responsibility for illuminating the technical side of these issues as accurately and clearly as the state of knowledge permits. Those who do make the choices, and the members of the public to whom they are accountable, cannot be expected to have the detailed knowledge of specialists in all the technical fields that apply. They must in some sense trust the specialists--if not individually then collectively--to tell it straight. Since, in fact, it would be even sillier to suppose that individual specialists are infallible than to suppose that groups of specialists are, it is in the collective mechanisms for certifying technical information that the greatest trust is placed. And, accordingly, it is the possible failure of these safeguards of collective certification that poses the greatest potential for harm. Put more bluntly, once a substantial part of the technical community has been fooled by an erroneous report or article, one can hardly expect that the popular press, the public, and the policy makers will not be fooled as well.

We believe the Inhaber affair illuminates rather dramatically the conditions under which the technical community's safeguards--and hence the public's--can be defeated. These conditions include both some general characteristics of technical life today and also some specific circumstances present in the case of the Inhaber report. The general characteristics are:

(a) A high degree of specialization in technical education and professional work, meaning not simply that only a small fraction of the technical community is really well informed in each sub-field but, more importantly, that all too many scientists and technologists will not even try to follow a quantitative calculation in a paper outside their own speciality to see if it makes simple sense.

(b) Heavy pressure to publish and a resulting excess of reports and articles, many done carelessly, incompletely, or without a real contribution to make at all.

(c) Overcommitment. Almost everyone in technical life is overcommitted--too busy--and often the most competent are the most overcommitted. This fact combines with the flood of reports and articles to assure that many never receive proper peer review, even if formally passed through the hands of ostensible reviewers.

In the case of the Inhaber report, the salient additional circumstances included:

(a) A field of endeavor with an unusually small number of practitioners--namely, comparative assessment of environmental effects of energy technologies.

(b) A conclusion with instant appeal for a substantial part of the technical community. Many technologists are fed up with the erroneous assertions of some environmentalists that renewable energy sources are free of environmental costs.

(c) A superficially plausible approach, coupled with the appearance of reliance on a large body of technical literature.

(d) The relative inaccessibility of most of the references actually relied upon, making the checking of references very difficult for most potential reviewers.

(e) Almost impenetrable prose in the report's technical appendices, which, coupled with atrocious proofreading, encouraged the reader to stick to the somewhat more readable (but calculation-free) main report. Readers of all the journal publications of the work were spared the appendices altogether, and with them much of the evidence of error.

These circumstances all contributed to what happened, but they are not the whole story. There is a degree of internal trust in the technical community, without which it would be very difficult to operate. The internal trust includes the suppositions: that individuals with advanced degrees in technical

fields will do their arithmetic correctly and will get their ratios right side up (at least most of the time); that authors will read the references they cite and will use the data they say they use; that researchers will correct errors pointed out to them by colleagues, and will not disseminate results known to be based on error; and that authors will not try to avoid the appearance of error by denying they wrote what they wrote. Gross abuses of this trust are fortunately rare. But the rarity means that, when an example does present itself and is pointed out, the reaction of those not close to the subject matter is likely to be disbelief. The further the abuse has propagated before it is pointed out plainly, moreover, the more intense the disbelief becomes ("How could so many have been fooled?"), and the more likely it becomes that people will consider the matter to be a mere technical controversy rather than an abuse of professional norms.

Herein lies the major liability of the scientist's almost reflexive restraint, politeness, and understatement in criticizing the work of others. This professional decorum is appropriate when the issue under dispute is indeed a matter over which responsible analysts might disagree. But when the sheerest incompetence and misrepresentation are the issues, it does no service to science or to the public to paper it over with understatement and euphemisms. Indeed, the price of such unwarranted "professional niceties" is likely to be the further propagation of misinformation. It should be added that the tradition of restraint in scientific criticism developed in an era when science was not so crucial to public decisions as it is today; if errors propagated because initial criticism was so understated as to be missed, they would nonetheless be caught in the literature in due time, having caused little damage. When bad science has the potential to cause bad decisions on behalf of entire nations, which is more than occasionally the case today, one cannot be so complacent.

Some of us have been taken to task by Inhaber, by the AECB, and even by journalists for the directness and harshness (one writer said "incivility") of our language in this affair. As the foregoing paragraph suggests, we do not think these criticisms warranted. By the time we entered the fray, Inhaber and the Atomic Energy Control Board had already demonstrated their unresponsiveness to correctives applied with the usual professional courtesies; they were busily engaged in an energetic and successful campaign to distribute

worldwide the conclusions of a report whose pervasive incompetence had already been called to their attention (in the usual restrained language) by a variety of reviewers. We believe the integrity of the whole process of intellectual inquiry and rational debate is too fragile and too precious--and the costs of misinformation too high--to dismiss so blatant and persistent an abuse with a shrug or with another restrained commentary. Such drastic instances of the propagation of outright technical nonsense, whether deliberate or out of ignorance, have been relatively rare even in the emotional and intellectual turmoil of the energy debate. When they do occur, there should be no taboo on the accurate use of words like "incompetence" and "nonsense" to warn the technical community and the public about the nature of what they are being asked to believe. It would have been better had such accurate descriptions been applied to AECB-1119 by earlier reviewers than ourselves, for significant damage to the credibility of the AECB, several scientific journals, and the technical community itself might thereby have been avoided.

CHAPTER 2: FOSSIL-FUELED ELECTRICITY GENERATION

I. INTRODUCTION

The principal function of AECB-1119's estimates of the occupational and public risks of fossil-fuel use would seem at first glance to be to serve as yardsticks against which to compare the risks of renewables, the calculation of which receives considerably more attention. The estimated coal risks, however, have a much more important role: they account for the bulk of the risk AECB-1119 attributes to wind, photovoltaic, and solar-thermal-electric systems, all of which are assumed to use coal as back-up when sun or wind is absent. Accordingly, we will devote more attention here to coal (treated in AECB-1119's Appendix A) than to oil and gas (Appendices B and C in AECB-1119).

Notwithstanding the attention that Inhaber gives in the main body of AECB-1119 and in the published summaries to his method of calculating occupational risks from materials and labor requirements, Appendices A, B, and C reveal that he has not applied this method to coal, oil, and gas at all. He has simply taken ready-made risk figures from some previous literature surveys (relying almost entirely on Smith _et al_. 1975, Comar and Sagan 1976, and U.S. Atomic Energy Commission 1974), evidently with little understanding of how these figures were derived, to what they refer, and how they may and may not be combined. The most remarkable feature of all this is that the risk figures that Inhaber has garnered from the literature contain no contribution at all from materials acquisition, component manufacture, or construction--despite the repeated indications in the main report and the summaries that he has treated all energy sources on a consistent basis. Although this is a striking inconsistency, its effect on the results is modest compared to those of many other errors in AECB-1119, because, for coal and oil at least, the risk attributed to air pollution overwhelms the contribution that the risks of creating the facilities would have made if included. Details are supplied fuel by fuel in what follows.

II. THE COAL-ELECTRIC SYSTEM

The coal-electric system treated in AECB-1119 is entirely conventional. The coal is assumed to be transported an average of 300 miles between mine and power plant, where it is burned in a conventional steam-electric cycle at about 37 percent efficiency.

II.A. APPENDIX A'S DISCUSSION OF WHAT WAS DONE

Inhaber took most of his data on the risks of this system from work by two of us (Smith *et al*. 1975), but he misrepresents what those data include. One finds at the beginning of the coal appendix (AECB-1119/REV-2, p. A-1):

> "Table A-1 (9) shows that [sic] labor and material requirements per megawatt-year net electrical output."

This table for coal, as well as those for oil and gas (Tables B-1 and C-1) does not include the labor requirements for materials acquisition (except for supplying the coal itself) and component manufacture. The data come directly from Smith *et al*., which includes labor for fuel acquisition and on-site construction but not for acquisition of nonfuel materials or for component manufacture. If Inhaber had applied the same method to the coal system as he did to the unconventional systems, he easily could have found these additional labor requirements from the list of materials requirements he had available in Table A-1. He also could have used the risks for materials acquisition and construction given in Caputo (1977, p. 6-36), whom he cited on other points. Caputo's figure for these risks is 1.1 worker-days lost per megawatt-year.

AECB-1119's Appendix A contains the following statement (REV-2, p. A-1):

> "Results of risk calculations are shown in Table A-2 (22). Occupational risk is derived mainly from construction and coal mining, and public risk is derived primarily from air pollution."

These risks do not include any contribution from materials acquisition and component manufacture, because the labor requirements were never calculated for these categories for conventional systems. Nor, in spite of the statement quoted, do they even include any contribution from construction, for which the report *does* list labor requirements. The occupational risks have been taken

almost without change from Smith et al. (p. 122). These risks are for operation and maintenance only. In some instances, data from Comar and Sagan (1976) are substituted for or added to the values from Smith et al., but their study also neglected construction risks.

One gets the impression from the main text of AECB-1119 that Inhaber was aware that the occupational-risk figures given in his main reference on the conventional technologies were for operation and maintenance, and not for the sum of creation of energy facilities plus operation and maintenance, for he writes (AECB-1119, p. 16):

> "Operation and maintenance risk has been previously estimated for the conventional technologies (7) [Smith et al. 1975] and for certain non-conventional systems like solar thermal electricity."

Perhaps this awareness had slipped away by the time the appendices containing the calculations were written.

Inhaber apparently felt obliged to assure the reader that no systematic violence was being done to the results of Smith et al. by the addition of data from other sources. He wrote (AECB-1119/REV-2, p. A-1):

> "Generally speaking, as many data points were adjusted down as were adjusted up."

The statement may be true, but it is misleading: the magnitudes of the upward changes were much greater. As a result, AECB-1119's upper limit for total occupational risk is 1.3 times higher and the upper limit for total public risk almost 1.5 times higher than the corresponding figures in Smith et al.

It is not unusual in AECB-1119 to find quantitative information that seems to have been thrown in for effect, with no determinable relevance to the actual estimates of risk. Thus one finds (AECB-1119/REV-2, p. A-1):

> "It was assumed that about 80% of sulfur was removed from coal (185), or a release of 21,800 metric tons of sulfur dioxide per year from a potential value of 109,000 metric tons."

This sentence and those that follow it make little sense in the context of the rest of AECB-1119. The report has framed its discussions in terms of megawatt-years electrical output. Suddenly it begins to refer to an entire coal plant's outputs and inputs without first stating what this plant's characteristics are--size, efficiency, sulfur content of coal used, and so on. To a reader who only reads AECB-1119, this coal plant would remain a mystery throughout. Checking all the references from which this plant might have come reveals it is the one described in WASH-1224 (U.S. Atomic Energy Commission 1974). Why this particular plant is mentioned at all is unclear, since most of the risk estimates derive from Smith *et al.*, whose reference coal plant has somewhat different characteristics.

What the text of Appendix A implies about pollution control for the coal-burning plant differs, moreover, from what is indicated in the main body of the report. The statement quoted above about 80-percent sulfur removal would lead most readers to believe that flue-gas scrubbing was assumed, notwithstanding the phrasing that the sulfur was "removed from coal", because this level of desulfurization is not generally attainable in commercial precombustion coal-processing techniques. (Readers who managed to discover the source of the numbers quoted would have been able to confirm that flue-gas desulfurization was specified.) In a footnote in the main body of the report discussing the assumptions about coal, however, one finds (p. 44, footnote 8):

> "The system described [in Smith *et al.*] also includes lime scrubber flue gas desulfurization. However, the report admits that this is only 'under development', and we are considering only operational systems. We therefore assume a coal plant without this desulfurization process, yet meeting present U.S. standards by judicious choice of coals."

Of course, between the time Smith *et al.* were writing and the time Inhaber wrote, lime scrubbers went into full commercial operation on many large coal-burning power plants. It is in any case an odd comparison indeed that would consider lime scrubbers an unproven technology for coal plants being considered as alternatives to solar-thermal-electric and photovoltaic electricity generation.

On this as on many other points, the reader simply cannot tell what Inhaber did by reading the words in AECB-1119; there are too many

contradictions and ambiguities. Only by comparing the figures in detail with what one finds in the sources Inhaber used is it possible to discover what the numbers mean. The remainder of our discussion of AECB-1119's treatment of coal, therefore, works from that report's Table A-2, where all the risk estimates for coal are given.

II.B. AECB-1119'S TABLE OF COAL RISKS

AECB-1119's Table A-2, Coal-Fired Electrical Production System Risk, is reproduced here in full on pages 101 - 102. The boxes and circled numbers have been added by us as keys to the following discussion.

(1.) These values were taken directly from Smith *et al*. (1975) and, as noted above, do not include risk from construction, materials acquisition and component manufacture. Although the rest of the risk numbers in the table do not come from one single source, none of the other sources include these categories of risk either.

(2.) The high end of this range represents the risk of death from black lung disease (coal workers' pneumoconiosis (CWP)). Footnote (b) indicates that this risk is based on Hamilton (1974) although the risk listed in that report was actually 2.7 times higher(1). Hamilton (p. 60) indicates that the risk was estimated by looking at the risk "in a cohort of coal miners whose mortality experience was followed over 37 years" (p. 60). Thus, the risk that AECB-1119 incorrectly cited was based on CWP that developed as a result of the high but largely unknown coal-dust levels common in mines before the enactment of the Federal Coal Mine Health and Safety Act (1969, P.L. 91-173). A National Academy of Sciences' study has described the situation as follows (NAS 1976a):

> "No useful information on dust levels to which miners were exposed was available prior to 1968. At that time, the U.S. Bureau of Mines initiated an environmental respirable dust survey program in a representative number of mines. The significant findings from the study were that

(1) 7 deaths per plant-year divided by 750 megawatt-years per plant-year equals 9.3×10^{-3} deaths megawatt-year (Hamilton 1974, p 53).

Coal-Fired Electrical Production System Risk
(per megawatt-year net electrical output) (22)

	Gathering & Handling Fuels (j)	Transportation	Electricity Production	Total
	(4)	(1)		
Occupational				
Accidental				
Death (h)	$0.7 - 1.5 \times 10^{-3}$ (a)	$1.6 - 5.0 \times 10^{-3}$	$1.3 - 9.0 \times 10^{-5}$	$2.3 - 6.6 \times 10^{-3}$
Injury (i)	0.04 - 0.07 (e)	$1.3 - 4.8 \times 10^{-2}$	$1.6 - 8.5 \times 10^{-3}$	0.056 - 0.083
Man-days lost	8 - 16	10 - 35	0.23 - 1.3	18 - 52
Disease				
(2) Death (h)	$0 - 3.5 \times 10^{-3}$ (b)	--	--	$0 - 3.5 \times 10^{-3}$
(3) Disability (k)	$5.5 - 8.2 \times 10^{-4}$ (f)	--	--	$5.5 - 8.2 \times 10^{-4}$
Man-days lost	0.03 - 21	--	--	0.03 - 21
Public				
Accidental				
Death (h)	--	$0.8 - 1.9 \times 10^{-3}$ (c)	--	$0.8 - 1.9 \times 10^{-3}$
Injury (i)	--	1.6×10^{-3}	--	1.6×10^{-3}
Man-days lost	--	5 - 11.6	--	5 - 11.6

	(5) Gathering & Handling Fuels (j)	Transportation	(6) Electricity Production	Total
Disease				
Death (h)	$1.4 - 14 \times 10^{-3}$ (b) (m)	--	$0.1 - 140 \times 10^{-3}$ (d) (g)	0.002 - 0.15
Disability (l)	--	--	1 - 216 (g)	1 - 216
Man-days lost	8.4 - 84	--	7 - 1920 (g)	15 - 2000

Notes:

(a) In original table, only underground coal mining was considered. To take account of the fact that strip mining is also used to gather coal, the data of Comar and Sagan (23) was used to calculate accidental death rates for gathering and handling fuels. Death rates from strip mining are considerably lower. Ref. 23 showed rates of a 1000-MW(e) station operation for one year. The actual energy produced per year divided by this rated energy capacity is defined as the load factor, assumed to be 0.7 for all systems unless otherwise specified. The total energy produced is then 700 megawatt-years. The risk data of Ref.23 is then divided by this value. The range in this data is the minimum and maximum from Refs. 14 and 25 - 28.

(b) From Ref. 26.

(c) From Refs. 14, 25, 26 and 28.

(d) From Refs. 26 and 28.

(e) Allowance was made for varying types of coal, as noted in footnote (a). From Refs. 14 and 25 - 27.

(f) Assumes that (i) present dust levels are enforced, (ii) there are 11,600 - 12,500 BTU per lb of coal, (iii) 10 - 14 metric tons per day are produced by a miner, and (iv) the dose-response curve is as described in Ref. 29.

(g) Oxides of sulfur only, from Ref. 30. Other pollutants have an effect, but are not included, for simplicity.

(h) 6000 man-days lost per death, from Ref. 31.

(i) 93 man-days lost per injury, from Ref. 31.

(j) Only 2/3 of coal is processed, from Ref. 26, 27 and 31.

(k) Implies 55 man-days lost per disability.

(l) Implies 5 man-days lost per disability.

(m) Ref. 131 states: "This wide range reflects the variations in plant location and uncertainties in dose-response relations for health effects of sulfur-related pollutants. In the course of new plants meeting new source standards for sulfur emission by burning low-sulfur coal, the estimated range would be (a factor of four lower)." As noted in the Introduction, we are concerned here only with conditions for energy production as they presently exist, not as they might be in the future. The data used does not preclude future improvements.

a large proportion of underground coal miners had respirable coal dust exposures in excess of 3.0 mg per cubic meter (average 6.5 mg per cubic meter). It was not unusual during the first two years of the survey to find full-shift respirable dust levels of 20 to 30 mg per cubic meter, with peak elevations of 50 mg per cubic meter. But by December 1973, following implementation of the mine dust standards in the 1969 law, approximately 94 percent of all operating sections in the mines were at or below 2.0 mg per cubic meter, and about 60 per cent were at or below 1.0 mg per cubic meter."

British experience would suggest that exposures at the present dust standard (2 mg/m^3--the average U.S. level seems to be already well below this figure; NAS 1976a, Parobeck 1975) will lead to an incidence of CWP very much lower than that experienced in the past (about 1/60th, Jacobsen 1971, NAS 1975a). The most severe forms of CWP, which can lead to death, might even be absent but, at most, would not exceed their present fraction of all CWP (NAS 1975a, NAS 1976a). This information was the basis for the figures in Smith _et al_. (1975), which are intended to be estimates not of past problems with coal (an interesting but separate issue) but of the problems that can be expected if more coal facilities are built. It is based on the logic that the best dose-response information is gleaned from studies of the past exposures and incidences but that this information must be coupled to present exposure levels in order to determine the impact of present systems.

(3.) As just discussed, AECB-1119 modified the death risk listed in Smith _et al_. so that it represented the risks of past dust levels, and it did this modification incorrectly. AECB-1119 retained, however, the risk of disability given in Smith _et al_., which represents the risk of present dust levels. This inconsistency makes no sense since the deaths are simply the end points of the severest cases of CWP and, most evidence suggests, have the same cause. Rationalizing the black-lung figures in AECB-1119 reduces the upper-limit risk from disease in coal mining from 21 worker-days lost per megawatt-year to 0.4 worker-days lost per megawatt-year.

(4.) Most of the figures for the risk of accidents and disease in coal mines are out of date. They reflect the average experience in all U.S. mines, much of which occurred previous to the changes wrought by the implementation of new

range would be (a factor of four lower)." As noted in the Introduction, we are concerned here only with conditions for energy production as they presently exist, not as they might be in the future. The data used does not preclude future improvements.

laws and technologies. Inhaber cited a source of more recent data on U.S. coal mining experience as his reference on risks of anthracite mining, which he assumed to be the coal used for steel making (see Chapter 1, Section III.C.3). The reference (U.S. Department of Labor 1975) also contains data for working days lost to nonfatal injuries and illnesses in mining of bituminous coal and lignite (which together account for 99 percent of U.S. coal production). The figure for bituminous coal and lignite for 1973 (the year covered by this reference) is 139 worker-days lost per 100 worker-years; this translates to 1.1 worker-days lost per megawatt-year, given the productivity of miners (2.1 metric tons of coal per worker-hour, U.S. Department of Commerce 1974) and the coal-to-electricity conversion ratio of 3.33×10^3 metric tons per megawatt-year. These figures are for the combination of underground and surface mining that actually prevailed in 1973; Inhaber indicates in the first footnote to his table on coal risks (AECB-1119, p. A-4) that he intends his mining risks to represent the combination of surface and underground mining. The upper-limit nonfatal injury rate used in AECB-1119 (0.07 injuries per megawatt-year or 21 injuries per million metric tons of coal) is about the same as that given by the Department of Labor for 1973, but Inhaber uses a figure of 100 working-days lost per injury whereas the Department of Labor's figure is 18. Inhaber's upper-limit risk estimate for nonfatal injuries in coal mining thus turns out to be 7 worker-days lost per megawatt-year, whereas the authoritative source (U.S. Department of Labor 1975) he cites on another point gives 1.1.

Inhaber's upper-limit figure for deaths in coal mining, which he took from Comar and Sagan, is simply outdated. It is 1.5×10^{-3} deaths per megawatt-year, or 0.45 deaths per million metric tons of coal, or (at 6000 days lost per death) 9 worker-days lost per megawatt-year. More recent data would not have been hard to find. Widely reprinted figures from the U.S. Mining Enforcement and Safety Administration (see, e.g., National Academy of Sciences 1975b, pp. 205-212) give the average accidental death rate for all U.S. coal mines in 1973 as 0.23 deaths per million metric tons, or 0.77×10^{-3} deaths per megawatt-year, or 4.6 worker-days lost per megawatt-year.

Even these data are not entirely appropriate for Inhaber's purpose, however, since they stem from statistics that come from all existing coal mines, both old and new. Even better would be data on new coal mines only; those,

for example, that have been put into operation since 1969. These would most closely represent the conditions in the next mine to be built--the one that is to be compared with other energy-producing alternatives. There is some evidence, however, that the accident rates are higher for younger and more inexperienced miners. The newest mines, therefore, might initially have higher risks to the extent that they hire new miners (Argonne National Laboratory 1977). In any case, when current data are available, as they were in this case (U.S. Department of Labor 1975, AECB-1119's reference 15) it is best to use them, especially in an activity like coal mining where conditions are much different now than they were in the recent past.

(5.) Not enough information is given in AECB-1119 to determine how these risks were included. One reference (Table A-2, footnote b, ref. 26) states that "No estimates (have been) made to date" (Hamilton 1974, p. 64) of the public risk due to coal processing. If these risks are supposed to represent the risk of the air pollution from the burning of tailings banks, then there should be disabilities associated with them. In Hamilton (1974) such disabilities are listed along with the death risks in the summary table (p. 52) although not in the detailed table on coal (p. 54).

Footnote (j) of Table A-2 states that "[o]nly 2/3 of coal is processed", which is a condition taken directly from Smith et al. (page 114), but Smith et al. includes no impacts in this category since it assumes that mine regulations will be enforced to prohibit the build-up and spontaneous combustion of tailings piles around mines and processing plants. This view is supported by AECB-1119's reference 27 (Council on Environmental Quality 1973) which also includes no public impacts in this category. Although there is no direct reference, it seems that AECB-1119 has actually taken this risk from Comar and Sagan (1976), which cites Hamilton (1974) as its reference. Comar and Sagan leave out the disability risk found in Hamilton. Hamilton gives an estimate equivalent to the high end of the range of risk indicated (p. 54):

$$\frac{\text{10 deaths per plant-year}}{\text{750 megawatt-yrs per plant-yr}} = 13 \times 10^{-3} \text{deaths per megawatt-year}$$

Apparently Comar and Sagan added the low estimate on their own since it is not found in Hamilton, the only reference in Comar and Sagan for this category (p. 588).

To summarize: Although the cause of the risks in this category is not completely clear, it seems that they are based on Hamilton's estimate of the air pollution deaths for burning coal tailings in the past. The low end of the range seems to have been added, without reference, by Comar and Sagan, which, in turn, has not been directly cited by AECB-1119 in this category. However, if the simple assumption is made that present regulations will be met by new facilities, then the health impact in this category should be nearly absent (at least for short-term air pollution). If this assumption is not made, then risk analyses degenerate into a series of arbitrary judgments on the part of the analysts. In some cases there may be solid evidence that regulations are not being met by new facilities, but no such evidence has been presented in this case.

By arbitrarily (and nonuniformly) choosing between impacts based on past experience and those based on present practice, AECB-1119 is not following its own dictum (AECB-1119, p. 5):

"An important assumption in the calculations is that present-day technology, models and systems, with their corresponding risk, are used."

This example deals with a relatively minor part of the public impacts from coal but does illustrate some of the problems endemic to risk analyses (not, unfortunately, only Inhaber's). One study quotes the last, often altering the numbers slightly without explanation and failing to carry forth the assumptions of the previous study. Inconsistencies (in this case, deaths but not disabilities from air pollution) are perpetuated, along with enough vagueness to frustrate attempts to trace the original reference. It often turns out that the original references for any particular risk are few and are based on studies of the average of all systems, old and new, and not on the characteristics of the marginal system which is the concern of most risk analysis. Apparent consistency among current analyses, therefore, often is due merely to the fact that each, through a different route, has based its numbers on the same few original references of questionable relevance to the question at hand.

6. In Appendix A (p. A-1), AECB-1119 states that these risks have been based on emissions that are lower than the EPA standards, and implies that the reference system achieves this performance through application of flue-gas

desulfurization technology. In footnote 8 (p. 44), however, it claims that the emissions meet the requirements through use of low-sulfur coals. Neither turns out to have been the basis of the actual calculations in AECB-1119, however. Disability risks are taken directly from Smith et al., which calculated a range of emissions from 25% to 85% of those allowed according to the type of coal and level of pollution control used. Death risks in AECB-1119, however, have been estimated using an example drawn from a National Academy of Science's study (NAS 1975a: reference 30 of AECB-1119, cited in footnote g of Table A-2). The example used in this reference (page 631) was based on emissions 4.2 times greater than allowed under the New Source Performance Standards (NSPS):

$$\frac{96.5 \times 10^6 \text{ lb S}}{4.28 \times 10^9 \text{ kWh}} \times \frac{2 \text{ lb } SO_2}{1 \text{ lb S}} \times \frac{111.3 \text{ kWh}}{10^6 \text{ Btu input}} = \frac{5 \text{ lb } SO_2}{10^6 \text{ Btu input}}$$

The NSPS for SO_2 is 1.2 lb/10^6 Btu (National Academy of Sciences 1975a, p. XLIV). The National Academy of Sciences study (1975a) is a good source for dose-response information on SO_2 emissions, but the sample plant it considered emitted substantially more than what is allowed by law for new plants. Its risks, consequently, should be modified to be consistent with these standards. The actual risk of death at the high end of the range in the NAS example is 0.17 (84 deaths per plant-year divided by 488 megawatt-years per plant-year) while AECB-1119 quotes 0.14. Apparently the number again actually comes from Comar and Sagan (AECB-1119's reference 23; 100 deaths per plant-year divided by 700 megawatt-years per plant-year equals 0.14) even though that reference hasn't been directly cited. This number, in turn, was based on Hamilton (1974) and Sagan (1974) (AECB-1119's reference 28), which deal with emissions prevalent in the past but illegal in new coal plants. Using NSPS emissions, and the dose-response relationship in the National Academy of Sciences' study, yields a risk of death, at the high end of the range of uncertainty, of 0.17/4.2 = 0.04, or about a factor of 3.5 lower than actually indicated in AECB-1119. To be consistent, the risk of disability should be raised from the value in Smith et al. (which, at the high end of the range, was 85% of NSPS) to a value equivalent to the NSPS emissions, an increase of a factor of 1.2 (1/0.85). Making these corrections and removing the inconsistencies results in the set of risks shown in Table 2.1. No correction has been attempted here

for the low end of the range of uncertainty.

TABLE 2.1

PUBLIC RISK FROM ELECTRICITY PRODUCTION WITH COAL

(Effects per megawatt-year)

	AECB-1119	"Corrected"
Deaths:	$0.1 - 140 \times 10^{-3}$	$0.1 - 40 \times 10^{-3}$
Disability:	1 - 216	1 - 250
Person-days lost:	7 - 1920	7 - 1500

To summarize: In two different parts of the text, AECB-1119 states that different and contradictory levels of SO_2 emissions were used as the basis of its risk estimates. In the table where the risks are presented, two other, and also contradictory, levels of emissions were actually used--one for the risk of death and the other for disability. Changing these two levels to be consistent with one another and to reflect AECB-1119's stated intention to consider present-day technologies ("... we are concerned here only with conditions for energy production as they presently exist ..." AECB-1119, p. A-4, footnote m) lowers the death risk substantially and raises disability risk slightly. This results in a total public risk about 1.3 times lower than what is listed in AECB-1119.

II.C. SUMMARY FOR COAL

The risks of generating electricity by coal have been calculated incorrectly in AECB-1119, and most of the errors can be classified into two categories:

-- AECB-1119 left out the risks of constructing the coal power plant and fuel-cycle facilities, fabricating the components used in these facilities, and acquiring the material used to make these components. This is so in spite of statements to the contrary in the text and in spite of the fact that these risks were a significant part of the total risks calculated for the unconventional systems. Using the estimates of Caputo (1977) for coal-plant materials acquisition and construction would add

for the low end of the range of uncertainty.

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II.C. SUMMARY FOR COAL

The risks of generating electricity by coal have been calculated incorrectly in AECB-1119, and most of the errors can be classified into two categories:

-- AECB-1119 left out the risks of constructing the coal power plant and fuel-cycle facilities, fabricating the components used in these facilities, and acquiring the material used to make these components. This is so in spite of statements to the contrary in the text and in spite of the fact that these risks were a significant part of the total risks calculated for the unconventional systems. Using the estimates of Caputo (1977) for coal-plant materials acquisition and construction would add

1.1 worker-days lost per megawatt-year.

-- The risks from air pollution, black-lung disease, and mining accidents are all too high because AECB-1119 has relied on information that reflects the past performance of coal technologies. No one builds or operates coal mines and power plants the way they used to, and, in fact, it is illegal to do so. Great strides have been made in controlling the risks from these sources, although there is considerable room left for improvement. To be valuable as an assessment tool, risk analysis must address itself, as much as possible, to the next plant to be built, the marginal facility, for this is the one about which a decision must be made. This is especially important for technologies like coal, where relatively rapid change has occurred recently. Correcting the black-lung figures and mine-accident figures to reflect the more recent data, as discussed above, decreases the upper-limit occupational risk in the coal fuel cycle from 73 to 42.1 worker-days lost per megawatt-year. Correcting the public disease figures decreases the upper-limit public risk in this category from 2000 to 1500 person-days lost per megawatt-year.

The effects of incorporating these partial corrections into AECB-1119's estimates of the risks of coal are shown in Table 2.2.

TABLE 2.2

EFFECTS OF PARTIAL CORRECTIONS OF AECB-1119'S ESTIMATED RISK OF THE COAL-ELECTRIC SYSTEM

(All figures in worker-days lost (WDL) or public person-days lost (PDL) per megawatt-year.)

	AECB-1119 (REV-2)	Our partial corrections
Occupational risk (WDL)	18 - 73	19 - 43
Public risk (PDL)	20 - 2012	20 - 1500

Two points about the values in the table need emphasis. The first is that the partially corrected estimates are not "right". They are superior to

AECB-1119's numbers in that they are internally consistent and consistent with the stated assumptions; but those assumptions leave out some potentially important sources of harm to human health (e.g., oxides of nitrogen, trace metals) and tend to overstate the harm from sources that are included (e.g., the mining casualties are still probably unrealistically high for new, large mines of the sort likely to be used for new coal power plants, and the upper-limit figures for public disease from air pollution assume a power-plant sited just 60 kilometers upwind of New York City). Whether the omitted effects or the overstatement of included effects are the more important is difficult to say with confidence. Second, the relative effects of our partial corrections of AECB-1119's coal numbers are modest compared to the changes in AECB-1119's numbers for unconventional technologies. The main reason for this is that most of the report's figures for coal risks came directly from the risk literature, offering fewer opportunities for mistakes than did the construction of risk estimates for the unconventional technologies using AECB-1119's method. Nevertheless, the errors in the fossil-fuel sections of AECB-1119 reveal the same lack of attention to detail, failure to carry through the stated approach, and inappropriate use of the available literature common to all sections of AECB-1119.

III. OIL

The residual fuel oil (RFO) power plant and fuel cycle considered in AECB-1119 were also of the most conventional types, except that the crude oil was considered to come from offshore wells. The refinery was assumed to produce only RFO, a simplifying assumption common in the literature.

III.A. APPENDIX B'S DISCUSSION OF WHAT WAS DONE

What follows here are a few brief commentaries on passages from AECB-1119's Appendix B, where oil is discussed. In general, the errors are very similar to those made in the coal section.

> "As in the case of coal, we assume that the plant meets present-day air quality standards, i.e., that there is no special scrubbing system installed." (AECB-1119, p. B-1)

As shown above, the emissions considered in the coal system did not meet present standards. In any case, however, emissions at the maximum level permitted for coal would not meet the standards for emissions from oil-fired plants because the standards for oil are 1.5 times more stringent (0.8 versus 1.2 lb sulfur dioxide per million Btu input). Yet, in Table B-2, the public impacts from oil plants are exactly the same as those from coal in Table A-2. They should, of course, be 1.5 times lower than the corrected values for coal or about 2 times lower than those actually shown for oil.

> "The materials used to construct pipelines are included in the calculations.... These lower [materials] requirements are reflected in the lower incidence of risk ... [compared to coal]." (AECB-1119, p. B-1)

As mentioned above, the risks of acquiring materials, fabricating components, and constructing facilities are not included in AECB-1119's estimates of risk. For example, the risk of death in the "Electricity Production" category of AECB-1119, Table B-2, is taken directly from Smith <u>et al</u>. and the risk of injury is only slightly different. Neither Smith <u>et al</u>. nor any of the other sources cited included any risks except those of operation and maintenance. In his risk analysis of renewable energy technologies (see below under Chapters 4-10), Inhaber derived such risks from the labor and materials requirements for these systems. This method was not applied to the fossil and nuclear systems.

> "A potential release of 35,100 metric tons release per year of sulfur dioxide was assumed (185). After abatement, the release was 19,100 metric tons. The U.S. EPA emission standards for new power plants imply a release of about 21,300 metric tons per year (see Appendix A) ...." (AECB-1119, p. B-1)

The release of 19,100 metric tons of SO_2 corresponds to the reference oil-burning plant in WASH-1224 (AECB-1119 reference 185), a 1000-megawatt facility operating at an annual load factor of 0.75 and a thermal-to-electric efficiency of 38.5 percent. (This information is not supplied in AECB-1119.) The 21,300 metric tons of SO_2 annual emissions <u>permitted</u> for this plant by the New Source Performance Standards (0.8 $lb/10^6$ Btu for oil-burning plants) bears only coincidental resemblance to the 21,800 metric tons mentioned as an <u>actual</u> release for a coal-burning plant in Appendix A. As was the case in the coal

appendix, moreover, Appendix B's figures for public deaths attributable to SO_2 corresponds to none of the emissions figures stated but to much higher ones, which are illegal in new plants.

III.B. SUMMARY FOR OIL

The same kinds of errors have been made in calculating the risks from oil as were made in the coal section:

-- No risks were included for any activities other than operation and maintenance. If, as the roughest approximation, the occupational risks from materials and component acquisition were taken to be equal to those from these phases of the coal fuel cycle, about 1 worker-day lost per megawatt-year would be added to Inhaber's figures.

-- The public risks were based on old and now illegal air pollution emissions. At emissions levels corresponding to the New Source Performance Standards for oil, the upper-limit estimate of public person-days lost would fall to about 1000 per megawatt-year.

These partial corrections are shown in Table 2.3.

TABLE 2.3

EFFECTS OF PARTIAL CORRECTIONS OF AECB-1119'S ESTIMATED RISK OF THE OIL-ELECTRIC SYSTEM

(All figures in worker-days lost (WDL) or public person-days lost (PDL) per megawatt-year.)

	AECB-1119 (REV-2)	Our partial corrections
Occupational risk (WDL)	2 - 18	3 - 19
Public risk (PDL)	9 - 1920	9 - 1000

IV. NATURAL GAS

The treatment of natural gas in Appendix C of AECB-1119 is such a mess that its conclusion--that natural gas has the lowest risk of any of the energy sources considered--must be viewed as an assumption rather than a result. (The conclusion may be right, but few of the data presented are even relevant to the issue, and those that are provide so incomplete a picture that no conclusion is justified.)

The shortcomings of AECB-1119's natural gas appendix can be summarized more specifically as follows:

(1) As in the case of coal and oil, the data presented for labor and materials requirements of a natural-gas system are entirely unrelated to the risks attributed to this system. No calculations were done to translate the former into a contribution to the latter.

(2) The labor and materials requirements presented are in themselves completely uninformative as to the requirements of electricity generation using natural gas; they are a combination of requirements for an oil fuel-cycle and requirements for a coal-gasification plant coupled to a combined-cycle electric power plant.

(3) The risk figures presented consist only of occupational deaths and injuries due to accidents in operation and maintenance. Not only is there no contribution from creation of the facility, but there are no occupational disease figures from operation and maintenance and no public risks whatever.

Since Item 1 is simply the same failure to count risks of creation of facilities that has already been discussed for coal and oil, we elaborate in what follows mainly on Items 2 and 3.

IV.A. LABOR AND MATERIALS

Inhaber describes his procedure for obtaining labor and materials requirements for the natural-gas fuel cycle as follows (AECB-1119, p. C-1):

"To determine the materials and labor required per megawatt-year net output, the gasification (130) and residual fuel oil (33) data from Smith et al was [sic] amalgamated. Comar and Sagan (23) indicate that natural gas has low overall risk compared to other conventional sources. In consequence, the lower of the two possible values for each category of material or labor was generally chosen."

The numbers resulting from the procedure described in the first sentence can nave little relationship to the actual materials and labor requirements of a natural-gas-fired power plant and associated fuel cycle, except by coincidence. Although the word "gas" is used in the coal gasification cycle, it describes a completely different type of system--a system that in Smith et al. is called the "low Btu gasification combined-cycle combustion" fuel cycle. In this system, coal mining and cleaning are followed by coal transport and gasification to a low-Btu gas at the power-plant site. The residual-fuel-oil fuel cycle includes a refinery. Thus, these fuel cycles have several kinds of facilities that have no counterpart in a natural-gas fuel cycle. Moreover, the use of Comar and Sagan's conclusion about "overall risk", to justify choosing the lower of the materials and labor requirements of oil and coal-gasification fuel cycles to represent natural gas, is simply bizarre. There is almost no connection between Comar and Sagan's risk figures and requirements for material and labor, since these authors considered no risks due to materials acquisition and none due to component fabrication or plant construction.

Not only is Inhaber's procedure for obtaining materials and labor requirements for the natural gas fuel cycle bizarre, but he does not seem even to have followed it consistently. The materials figures for power plant construction are illustrative (Table C-1). For pipes and concrete, figures from the residual-fuel-oil cycle were used; for major (fabricated) equipment and other equipment, the figures come from the low-Btu gasification combined-cycle combustion system (both in Smith et al.). For structural metals, however, the number (0.03 metric tons per megawatt electric year) comes from neither the oil nor coal-gasification tables of Smith et al. (these list 0.058 and 0.33 respectively), nor is any other source referenced. There is little hope that such a mixture of figures can represent a realistic estimate for natural gas. (Not one number comes from a natural-gas power plant itself.) AECB-1119 need

not have resorted to such an indirect attempt at deriving these figures, since at least one of the references cited in other sections included calculations of the materials requirements for gas-fired plants and their fuel cycles (Bechtel 1975: AECB-1119 reference 10).

IV.B. <u>SUMMARY FOR NATURAL GAS</u>

The origin of AECB-1119's risk estimate for natural gas is described by the author in the following paragraph, which we quote in full (AECB-1119, p. C-1):

> "Producing electricity from natural gas also incurs risk. This risk was combined using two sources. The information of Comar and Sagan (23) was used in compiling Table C-2. All of the risk mentioned in Reference 23 was occupational, as opposed to public."

What Inhaber means by saying that risk "was combined using two sources" is unclear. His Table C-2 contains the only risk figures for natural gas to be found in AECB-1119, and these come, as he says in the next sentence, entirely from Comar and Sagan. (The footnotes to Table C-2 list five other references; four of these are listed as "Cited in (23)", which suggests to us that they were not actually consulted in preparing the table. All the numbers in Table C-2 are identical to those in Tables 3 and 4 of Comar and Sagan.)

As Inhaber notes in the last sentence of the passage quoted, all of the risk for natural gas in Comar and Sagan, and hence all of the risk in his Table C-2, is occupational. As Inhaber does <u>not</u> point out, all of this occupational risk is for operation and maintenance only, and the tables in Comar and Sagan from which the data came are so labeled. The risk to the public is taken to be zero, because Comar and Sagan considered only the disease effects of oxides of sulfur and assumed sulfur to be entirely absent from the natural gas used. Inhaber notes that all natural gas is not free of sulfur, and that other air pollutants than oxides of sulfur may produce public risks. For AECB-1119's purposes, however, he assigns a value of zero to these possible contributions to public risks. He also mentions the possibility of accidents in shipping and handling of liquified natural gas, but makes no entry for this risk in his tabulation. Since Inhaber's risk figures for natural gas leave out the hazards of materials acquisition, component manufacture, facility

construction, LNG accidents, and air pollution, then, it is hardly surprising that the total risk estimate is low. A realistic value would almost certainly still be much smaller than the estimate for coal and oil, but there is no way even to begin to derive such a value from the information presented in AECB-1119.

CHAPTER 3: NUCLEAR POWER

I. INTRODUCTION

The risks of nuclear power are considered in AECB-1119 mainly in Appendix D. The treatment of nuclear technology also receives some special attention in the body of AECB-1119 and in the published summaries, presumably because the comparison between nuclear power and the renewables provides the report's most interesting conclusion and could be expected to draw the most critical outside scrutiny. The specific nuclear fuel cycle considered is that of the light-water reactor as described in Smith et al. (1975, p. 30). Fuel reprocessing is formally included, although the data available on the health risks of this operation were and are sketchy and unreliable. Plutonium recycle is mentioned in Appendix D but appears not to have been included in the risk estimates. (The main effects of plutonium recycle that show up in the figures of Smith et al. are a reduction in uranium mining, hence less mining risk, and an increase in the amount of plutonium transported. Figures for possible plutonium-related increases in fuel-processing and reprocessing risks and in reactor-accident consequences were not available.)

As in the case of the fossil fuels, AECB-1119 displays almost no actual calculations of the risks of nuclear technology; instead risk estimates are taken directly from the literature, the great bulk of them from Smith et al. (1975) and Comar and Sagan (1976). The one notable exception in the nuclear case is a short calculation to show that the "best estimate" accident risk from the Rasmussen report (U.S. Nuclear Regulatory Commission 1975) falls within the range of estimates of public risk from nuclear power presented in AECB-1119's nuclear-risk table.

Also as in the case of fossil fuels, AECB-1119's treatment of nuclear risks omits altogether the risks of creating nuclear technologies--materials acquisition, component manufacture, and construction. This point is discussed further below. But the coverage of nuclear power's health risks is also incomplete in other ways. No figures are provided for genetic risks, except as equivalent person-rem not translated into deaths and illnesses. The

uncertainty surrounding genetic risks is very large, but conceivably they could be as important as the somatic effects that are included(1) (Nuclear Energy Policy Study Group 1977). Also excluded are the genetic and somatic effects in future generations from exposure to very long-lived radioactive isotopes mobilized by present nuclear-fuel-cycle operations. The most important example is the radioactive gas, radon-222, emitted from tailings at uranium mills as a consequence of the presence in these materials of 80,000-year half-life thorium-230. The population dose from these emissions, per reactor-year of operation, may be 100 to 1000 times larger than the dose from all other fuel-cycle operations combined, although it would be spread out over hundreds of thousands of years (Nuclear Energy Policy Study Group 1977, National Academy of Sciences 1979).

Of course, there are analogous omissions from AECB-1119's treatment of fossil fuels (and the treatments by others): toxic trace metals, such as cadmium and mercury, mobilized in combustion of coal and oil, have a "half-life" of forever; these and other emissions, including oxides of nitrogen and secondary pollutants formed from them in the environment, may have genetic as well as somatic effects. Although it is generally held that these omitted health effects of fossil fuels are probably small compared to the sulfur-oxide/particulate effects included (Nuclear Energy Policy Study Group 1977), the uncertainties are considerable. The cloud of uncertainty that obscures risk comparisons of nuclear power and fossil fuels because of these and other deficiencies in knowledge will not be quickly lifted; and, of course, its existence is not Inhaber's fault. But he does a disservice in AECB-1119 by presenting comparisons based on very incomplete estimates, with almost no mention of the possible significance of what has been left out.

II. CREATION OF FACILITIES

Table D-1 of AECB-1119's nuclear appendix is entitled "Light-Water Nuclear Electrical Production System Materials and Labor". The numbers it contains are taken directly from Smith *et al.* (1975); the labor figures include operation and maintenance and on-site construction, but no materials

(1) Somatic effects are those experienced by the individuals who receive the radiation. Genetic effects appear only in the offspring of those exposed and in subsequent generations.

acquisition or component manufacture. There is no sign that Inhaber applied the method described in the body of his report to convert the materials figures in Table D-1 into risks of materials and equipment acquisition, or to convert the figures for construction labor into construction risks: comparison of the occupational risks in Table D-2 ("Light-Water Nuclear Electrical Production System Risk") with the references cited shows that the figures presented there are for operation and maintenance only. (One must locate a copy of the first or second edition of AECB-1119 to find the appropriate part of Table D-2, because two-thirds of this crucial table, whose information is not duplicated in the text, was omitted from the third edition through a gross printing error.)

As we noted in Chapter 2, a passage in the main body of AECB-1119 suggests that Inhaber was aware that the risk figures he took from his references were for operation and maintenance only, but there is no sign of this awareness in the nuclear appendix. We note also that Inhaber could have obtained risk figures for nuclear power that do include materials acquisition and construction from the final report of the JPL project (Caputo 1977, p. 6-36), which he cited on many other points, but he chose not to include or mention these estimates. Caputo's figures for materials acquisition and construction sum to 1.4 worker-days lost per megawatt-year of nuclear power. Adding this amount to Inhaber's figure in Table D-2 (1.7 to 8.7 worker-days lost per megawatt-year) increases the lower bound of nuclear power's total occupational risk to 1.8 times its former value and the upper bound to 1.16 times its former value. Simply applying Inhaber's method to the materials data in his Table D-1 gives similar results.

The unpublished IAEA critique, made available to Inhaber well before the third edition of AECB-1119 appeared, also strongly criticized his nuclear construction estimates (Black et al. 1978, p. 3):

> "From the data given in this report it is impossible to calculate risks of building a conventional plant. It would be preferable to keep data on material and labor requirements for building and for fuel supply separate. However, some data indicate that building risks have been largely underestimated. Our own calculations show, using an input/output methodology, that the risk of building a 1000 MW(e) LWR is given by

> 50,000 lost man-days due to illnesses, 3.7 deaths occupational, 3.8 deaths while commuting."

Ignoring the commuting deaths (since these are not counted for other technologies by Inhaber) and counting the occupational deaths at 6000 worker-days lost, one gets 3.4 worker-days lost per megawatt-year from the IAEA estimates for building a nuclear plant (30 year plant lifetime, 0.70 load factor). This figure is 2.5 times Caputo's, not surprising since input/output models capture labor overlooked in simpler approaches. Using the IAEA figure increases nuclear's occupational risk to 3 times the Table D-2 value at the lower bound and 1.4 times at the upper bound.

III. WASTE MANAGEMENT AND ACCIDENTS

Two of the most controversial aspects of nuclear risk are long-term waste management and reactor accidents. Both problems are characterized by large uncertainties that arise both from inadequacy of theoretical models and from lack of actual experience (this being essentially zero in long-term waste management and small in the case of reactor operation as it relates to clarifying accident probabilities). AECB-1119 indicates that both these sources of risk are included in its risk estimates; and the author attempts to defuse controversy over whether the full range of uncertainty was accounted for by suggesting that he has erred on the side of overstatement by using the estimates of a nuclear critic. Thus one finds in the nuclear appendix of AECB-1119 the following (p. D-4):

> "Calculated risk for nuclear electricity generation from light-water reactors is shown in Table D-2. As noted in Reference 123, the data is obtained from a source not well-known as a friend of nuclear power. An extra column has been added to take account of waste management. Although it is not known exactly what form or forms nuclear waste management will take in the future either in Canada or elsewhere, estimates were made of risk incurred by this aspect."

The "source" referred to is Smith, Weyant, and Holdren (1975), and "Reference" 123 (actually an explanatory note, not a reference) seeks to characterize Holdren as antinuclear by quoting from articles in which Holdren discusses nuclear risks. Consulting Inhaber's Table D-2 reveals citations to 12

sources, not one. It is clear, from the way these sources are cited and from the passage we quote below, that Inhaber took all the numbers from Smith et al. (1975) and Comar and Sagan (1976), both of which are literature surveys that identify the other 10 references as the initial sources of most of the data Inhaber uses. Of these other 10 references, not one is by anyone who could be construed as a nuclear critic.(2) Under these circumstances, it is hard to interpret Inhaber's phrase, "the data is obtained from a source not well known as a friend of nuclear power", as anything other than a deliberate attempt to mislead the majority of readers who wouldn't trouble to check which references are which.

The impression, conveyed by the passage above, that risks of waste management were included is also a misrepresentation and also seems to be deliberate. Inhaber knew that the numbers in the waste column of Table D-2 include only spent-fuel transport and fuel reprocessing but not long-term waste storage or disposal; a footnote to Table D-2 says so. He also has indicated (Inhaber 1979e) that he knew the numbers for the transport and reprocessing risk were taken from WASH-1224 (a 1974 Atomic Energy Commission comparison of risks of coal-fired and nuclear electricity generation) and thus could hardly be considered the handiwork of a nuclear critic. This knowledge did not stop him from implying otherwise for readers of the text of his nuclear appendix (quoted above), the main body of AECB-1119 ("Risk of waste disposition has been calculated for nuclear power." p. 16), and published summaries ("Risk of waste disposition was calculated for nuclear power." Inhaber 1979a, p. 720). None of the published summaries contain any clue that "disposition" excludes long-term management altogether.

Inhaber's misrepresentation of the basis of his figures for nuclear-accident risks is much more serious. Just down the page from the passage quoted above (AECB-1119, p. D-4), he continues:

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(2) Three are reports of the U.S. Atomic Energy Commission, two are from the Argonne and Brookhaven National Laboratories, two are from other federal bureaucracies (the EPA and the CEQ), one is from the National Academy of Sciences, one is an article in Nuclear Safety, and one is an article by Sagan in Science.

> "Where changes were made to the data of Table D-2 because more complete information was available, these were generally in the direction of increasing the risk. In effect, the higher values of either Comar and Sagan or Smith *et al* were used. This 'maximization' of risk was used only in the case of nuclear power, and not for other energy systems. In particular, the Smith *et al* estimate of public deaths from both routine and catastrophic operation of nuclear reactors from electricity production was multiplied by between 17 and 50 times to accord with the Comar and Sagan data. Much of the data for the values of between 3 and 23 x 10^{-5} is based on the Rasmussen report (44)."

(The sentence beginning "This 'maximization' ..." appears twice in the original; we have removed the extra one, but the passage is otherwise exactly as in AECB-1119.) The range of 3 to 23 x 10^{-5} refers to what Inhaber meant his Table D-2 to show under public deaths from nuclear electricity production, per megawatt-year of output; what is actually there in the third edition (this being the part of the table that was not omitted) is 3 - 23 x 10^{-6}, another misprint. The higher figure of 23 x 10^{-5} deaths per megawatt-year comes from Comar and Sagan (p. 588), and includes both routine emissions and accidents. What part of this total is due to accidents as opposed to routine emissions is not made entirely clear by Comar and Sagan, although they do indicate (p. 596) that only seven-tenths of a percent of the upper-limit figure (1.6 x 10^{-6} out of 23 x 10^{-5} deaths per megawatt-year) is due to "catastrophic" accidents--those causing more than 1000 early and delayed fatalities each. Their basis for this conclusion was the figure given in the draft Rasmussen report for the frequency of accidents of this size or larger, a figure revised upward by a substantial amount in the final report.

The most serious misrepresentation in the passage quoted above from page D-4 of AECB-1119 is its assertion that "the Smith *et al*. estimate of public deaths from both routine and catastrophic operation of nuclear reactors was multiplied by between 17 and 50 times to accord with the Comar and Sagan data." Table E-3 ("LWR Health Effects") of Smith *et al*. (1975, p. 159), which is exactly the page to which Inhaber refers as the source of the data in his Table D-2, has separate, adjacent rows labeled "Public Disease" and "Large Accidents". It is clear from the accompanying text and notes that the "Public Disease" numbers exclude the accidents tabulated adjacently, and this is clear

from the numbers themselves--the upper limits of deaths and disabilities under "Large Accidents" are much bigger than the upper limits under "Public Disease", so the former could not possibly be included in the latter. The Comar and Sagan figure of 23×10^{-5} deaths per megawatt-year, used by Inhaber as the upper limit for routine operation and accidents combined, is 17 times the Smith *et al*. upper-limit figure for routine operation alone, but 43 times *smaller* than the Smith *et al*. upper-limit figure for accidents prominently displayed in the same table with the routine-operation numbers. (The Smith *et al*. upper-limit accident figure was based on the quantitative criticisms of the draft Rasmussen report that had appeared as of early 1975.) We emphasize: had Inhaber actually used the Smith *et al*. accident figures he ignored while claiming to include them, the upper-limit figure in AECB-1119 for public deaths due to light-water reactors would have been more than 40 times larger.

What is nearly as interesting is that the upper-limit figure for reactor accidents given in the final Rasmussen report itself (U.S. Nuclear Regulatory Commission 1975) is higher than Inhaber's "upper limit", a point Inhaber fails to mention while taking credit, in the passage quoted above and elsewhere, for using the final Rasmussen report results. Of course, both official and unofficial reviews of the final Rasmussen report agree that the actual uncertainties are much larger than stated there (see especially the report of the prestigious review group commissioned by the Nuclear Regulatory Commission itself, Lewis *et al*. 1978), which means that the Rasmussen "upper limit" is itself not really the most pessimistic figure.

The Ford-MITRE study (Nuclear Energy Policy Study Group 1977), which Inhaber cites in his article for *Science*, indicates that "extremely pessimistic" assumptions can lead to an "upper limit" estimate of accident risk for light-water reactors that is hundreds of times higher than Inhaber's upper limit. The authors of that study state (p. 179) that "the expected number of cancers could be several times higher, depending on the assumed dose-response model used in deriving the risk estimates", than the values given in the Rasmussen report (WASH-1400). On the same page, they note that "the WASH-1400 probability estimate could be low, under extremely pessimistic assumptions, by a factor of as much as 500." If "several" (as it relates to the uncertainty in the number of cancers) is taken to mean 3 to 6[3], then the implied upper

(3) The higher number is not hard to defend. The Rasmussen report's "central estimate" used a value of 34 cancer deaths per million person-rem of low-level radiation from an accident, which

limit on the product of probability and consequences is a factor 1500 to 3000 larger than the Rasmussen report "best estimate". Inhaber's "upper limit" is only 6.7 times the Rasmussen report "best estimate". Some of the relevant "upper limit" risk estimates are summarized in the Table 3.1. None of these figures account for the possible contribution of sabotage to "accident" probability.

TABLE 3.1

"UPPER-LIMIT" RISK ESTIMATES FOR NUCLEAR ACCIDENTS

All figures are expected deaths per million megawatt-years
(assuming reactor size is 1000 megawatts and load factor is 0.70)

Comar and Sagan, catastrophic accidents only	1.6
Smith, Weyant, and Holdren, routine operation only	13
Rasmussen report (final), central estimate for all accidents	33
Comar and Sagan, sum of routine operation and all accidents	230
AECB-1119, sum of routine operation and all accidents	230
Rasmussen report (final), upper limit for all accidents	490
Smith, Weyant, and Holdren, upper limit for all accidents	10,000
Ford/MITRE, upper limit for all accidents	50,000 - 100,000

We must emphasize that it is not our contention that the higher figures in the above table are the "right" ones. Our point is rather that there is an enormous range of legitimate uncertainty about the appropriate value to use for expected casualties from reactor accidents. A case could be made for using a value even lower than the one chosen by Inhaber--say, the Rasmussen

is 5 to 6 times smaller than the "best estimate" of 160 to 210 cancer deaths per million person-rem given in the 1972 report of the U.S. National Academy of Sciences report on Biological Effects of Ionizing Radiation (NAS 1972), and almost 14 times smaller than the Academy's "upper limit". (The dose-response relation for low level radiation is the appropriate one, because typically 99 percent of the total deaths from accidents studied by the Rasmussen report are delayed cancers from low doses of radiation rather than early fatalities from high doses.)

report "central estimate"--as long as one made clear where the number came from and what uncertainties were attached to it. Our complaint is that Inhaber has claimed and implied repeatedly that he used highly pessimistic estimates of accident risk, when in fact he did not. In addition to the passages from AECB-1119 quoted above, for example, one finds in the summary published in Science (Inhaber 1979a, p. 719):

> "To avoid any bias in favor of nuclear power, I used the highest values of public risk from reactors taken from a wide number of sources (in some of these, Rasmussen's values were used)."

Inhaber's earlier article in New Scientist is even more explicit (Inhaber 1978b, p. 446):

> "Instead, a survey was taken of the major papers in the scientific literature which had estimated aspects of nuclear risk, including a monograph written by a well-known nuclear critic, John Holdren of the University of California at Berkeley. For each component of risk, the highest value from the group of scientific sources was used."

As we have shown, this statement is false.

In Inhaber's response to our draft letter to Science, where we criticized him for these misrepresentations, he dodged the issue of his earlier claims that he used the highest values and instead tried to argue that such values are unreasonable. Referring to our statement that the Ford/MITRE upper limit was some 200 times higher than his own, he wrote (Inhaber 1979c):

> "Let us now consider whether the values for nuclear public risk were, as is claimed, 200 times too low. I used an upper value of 23×10^{-6} deaths per megawatt-year. Multiplying by 200 yields 4.6×10^{-3} deaths. About 60,000 megawatt-years of nuclear energy were produced in the Western world in 1978, according to Nucleonics Week. This would imply about 280 deaths directly related to nuclear reactor accidents in 1978, a value which I do not find reasonable."

Whether Inhaber thinks the value is reasonable is of course beside the point, given that he was claiming and implying previously that he used the highest values he could find. Nevertheless, the passage is interesting. In it,

Inhaber appears to have been confused again by one of his own typos. The upper value he used was 23 x 10^{-5} deaths per megawatt-year, or 230 x 10^{-6}, not 23 x 10^{-6}, which was a typo in Table D-2 in the third edition of AECB-1119. (The correct value is not only found in the first and second editions but is reflected in the "Total" column in Table D-2 and in the accompanying text in the third edition.) Multiplying the "correct" number by 200 of course produces a figure--2800 deaths from 60,000 megawatt-years--that Inhaber presumably would find even less reasonable than the number he derived from a typo. Perhaps it has not occurred to him that, if 50 years of operation at the 1978 level of global nuclear power produced one accident with 140,000 casualties (mostly latent cancers), then the annual expected value would be 2800 deaths, corresponding to 200 times his own figure. So high an accident risk--frequency times consequences--may be unlikely, but neither analysis nor the modest amount of experience to date can rule it out.

IV. SUMMARY

If one adds to AECB-1119's estimate of the occupational risk of nuclear power the figures for materials acquisition and construction that Inhaber could have taken from Caputo (1977)--or could have derived, by applying his "standard" method to nuclear's materials requirements as tabulated in AECB-1119's Table D-1--the risk estimate goes up by 1.4 person-days lost per megawatt-year. Using the IAEA figure for materials acquisition and construction would add 3.4 person-days lost per megawatt-year.

If one uses, for the public risk from nuclear accidents, the "upper limit" from Smith et al. that Inhaber ignored while claiming otherwise, this would add to the upper limit 58.6 person-days lost per megawatt-year due to deaths, and 10 person-days lost per megawatt-year due to disabilities. (The Smith et al. upper limit for accident-caused disabilities is 0.1 disability per megawatt-year (p. 159); we assumed 100 person-days lost per disability.)

Making these partial corrections to the figures in AECB-1119 produces the figures shown in Table 3.2. (We have not changed the lower limit of the accident risk.) We emphasize that these changes do not include any contributions from genetic effects, or from long-term hazards from uranium-mill tailings, or from disposal or long-term storage of radioactive wastes. They also

do not include any estimates of the risks of nuclear terrorism or of the contribution of nuclear power to the proliferation of nuclear weaponry among nations.

TABLE 3.2

EFFECTS OF PARTIAL CORRECTIONS OF AECB-1119'S ESTIMATED RISK OF NUCLEAR POWER

(All figures in worker-days lost (WDL) or public person-days lost (PDL) per megawatt-year.)

	AECB-1119 (REV-2)*	Our partial corrections
Occupational risk (WDL)	1.7 - 8.7	3.1 - 12.1
Public risk (PDL)	0.3 - 1.5	0.3 - 70.0

* assumes missing part of Table D-2 is unchanged from REV-1.

CHAPTER 4: HYDROELECTRICITY

I. INTRODUCTION

Inhaber divides the risk analysis of Appendix K, Hydroelectricity, into two principal sections: risks associated with materials acquisition and construction and with operation and maintenance; and risks to the public associated with catastrophic dam failures. Inhaber's conclusions concerning these risks are based on an extremely small data base, and it is difficult to attach much meaning to his results.

II. MATERIALS ACQUISITION

As Inhaber points out, data on the materials required for the construction of hydroelectric dams are not plentiful. The data used for his estimates, however, do not come from the available published literature but from a personal communication with the Director of Design and Development at Ontario Hydro in Toronto. It is not clear whether Inhaber's figures include all hydroelectric dams in Ontario or a smaller number of dams. By using these data to describe a standard 1000-average-megawatt system, however, Inhaber is making the claim that the experience in Ontario is typical of all hydroelectric facilities. Fortunately, it is possible to compare Inhaber's data with published data available on hydroelectric facilities constructed in the United States and with comprehensive studies of the materials requirements for various energy technologies (TVA 1954; Mitre 1979; Bechtel 1978).

Detailed data on the hydroelectric dams in the Tennessee Valley Authority system are available in a useful form (TVA 1954). An analysis of the concrete, earth and rock, structural steel, and reinforced steel requirements for 25 hydroelectric dams in the TVA system indicates that the materials data quoted by Inhaber are a factor of two too low for steel and nearly five too low for concrete. These 25 dams have a total installed capacity of 2688.5 megawatts. Although Inhaber notes that the nature of hydroelectric dams is such that there is an enormous variety of designs, he presents data for an unspecified number of facilities without discussing the types of facilities,

the total installed capacity, or the range of values that exist in the data.

More important is the fact that the tremendous variability in materials requirements for hydroelectric facilities makes it meaningless to choose figures for a small group of dams and to claim that these are average or statistically significant values. Inhaber identifies a Bechtel Corporation report (Inhaber's reference 143) that lists earth and rock requirements seven times greater than his own, though he never actually presents the Bechtel data. In addition, a comprehensive Mitre Corporation study (Mitre 1979) describes the materials requirements for a 200-megawatt (electric) hydroelectric facility and lists steel requirements five and a half times greater than Inhaber's, concrete requirements over two times smaller, and earth requirements over twelve times greater. (See Table 4.1.) By considering published historical data and materials summaries, one discovers that the materials requirements for hydroelectric facilities may range from at least two times smaller to twelve times larger than Inhaber's values. The size of this uncertainty eliminates any significance from the values that Inhaber presents for materials acquisition.

TABLE 4.1

MATERIALS REQUIRED FOR A 1000-AVERAGE-MEGAWATT HYDROELECTRIC SYSTEM (10^5 Metric Tons)

	Steel*	Earth and Rock	Concrete
AECB-1119:	0.208	161	27.5
TVA:	0.429	266	103.7
Mitre:	1.145	1945	12.2

* Steel requirements listed here include both reinforced and structural steel.

To further compound the problems with his materials values, Inhaber requires that 100 percent of the structural and reinforcing steel be treated as fabricated metal products. As discussed in Chapter 1, Section III.C.3, this is incorrect because the industrial category "fabricated metal products"

excludes reinforcing steel and all structural steel fabrication work done "at the site of construction" (U.S. Office of Management and Budget 1972). In the case of hydroelectricity, however, the resulting inflation in risk is relatively small, since metals are a minor fraction of the total materials.

In Table K-2 (AECB-1119, p. K-3), Inhaber once again confuses the concrete and cement requirements. Although the materials risk is quite sensitive to the risk from "concrete" acquisition (over 75 percent of the materials acquisition risk in Table K-2 results from the mining of earth fill, rock and concrete aggregate), the net error is extremely difficult to estimate due to the tremendous variation in the total concrete requirements described above.

III. CONSTRUCTION

In calculating the risk from construction of hydroelectric facilities, Inhaber abandons the method that he uses on the unconventional technologies. Rather than guess a construction time and an occupational category relevant for dam construction, Inhaber relies on work done by other researchers who have compiled data on existing facilities. Comparing other data with the numbers presented by Inhaber indicates that the risks from construction fall within the range of the data he quotes.

A study from the Tennessee Valley Authority (TVA 1954) provides detailed construction data for 20 hydroelectric dams including injuries, fatalities, person-days lost, and worker-hours spent in construction of the facility. The 20 dams totalled 2093.5 megawatts of installed capacity. During the construction of these facilities, 106 fatalities occurred. The average fatality rate is therefore 0.051 fatalities per installed megawatt, within the range of the French data quoted by Inhaber. Combining this figure with Inhaber's assumptions of a 50-year lifetime for a dam, a 60-percent capacity factor, and 6000 person-days lost per death results in 10.2 person-days lost per megawatt-year, which is at the upper end of the range Inhaber presents in Appendix K.

For the same series of dams in the TVA system there were 6524 injuries. These injuries resulted in 1,091,945 person-days lost. For 2093.5 megawatts installed the injury rate is 17.39 person-days lost per megawatt-year, about 30 percent higher than the rate calculated by Inhaber. The difference between these figures is insignificant considering the small size of the data base.

IV. OPERATION AND MAINTENANCE

Using data on the number of fatalities occurring during the operation and maintenance of hydroelectric facilities in France between 1953 and 1967, Inhaber applies data on accidents per fatality in U.S. plants to determine the number of accidents per unit energy production overall.

> "Bertolett (150) shows that for operation and maintenance activities in the United States totalling about seven million man-hours, there were 21 accidents per fatality." (AECB-1119, p. K-4)

Checking Inhaber's reference 150 shows us that Inhaber's numbers are correct, but that there were a total of 21 accidents and one fatality during the seven million worker-hours of operation and maintenance activities. This is not a large data base and cannot accurately be applied to the French data to determine person-days lost due to accidents. The author of Inhaber's reference had doubts about the statistical quality of the very data used by Inhaber in determining the accident rates of generating plants. Bertolett (Inhaber's reference 150) states in his last paragraph (Bertolett 1974, p. 41):

> "A comprehensive statistical basis for determining accident rates for all major types of generating plants, and for making detailed comparisons, would require a more complete sampling, and should include all segments of the electric utility industry."

The data used by Inhaber are simply insufficient to make any meaningful determination of the risks associated with this category.

V. ENERGY BACK-UP AND ENERGY STORAGE

Inhaber does not calculate any back-up requirement for hydroelectric facilities. The storage requirement is taken care of by the construction of the reservoir and thus is included in the normal materials acquisition and construction categories.

VI. TRANSPORTATION

As usual, Inhaber estimates the risks from transportation by assuming that all the materials required for construction of the facility cause the same risk per unit weight as U.S.-average coal, which is shipped 300 miles by rail. When this assumption is applied to hydroelectric dams, a particularly large inflation of the risk figures occurs since by far the greatest part of the materials consists of rock and earth fill that is obtained at or near the site of the dam.

Specifically, the total weight of materials calculated by Inhaber is 724 metric tons per megawatt-year net output. Of this total, only the finished steel and fabricated metal products are likely to be transported any substantial distance to the construction site. The weight of these is only 1.38 metric tons, 0.19 percent of the total. If one is conservative and transports everything except the non-metal mining (i.e. the earth and rock), the result is to decrease the transportation risk from Inhaber's calculated range of 3.1 - 9.6 person-days lost per megawatt-year net output to occupational risks of 0.28 - 0.95 worker-days lost and public risks of 0.13 - 0.32 person-days lost per megawatt-year net output.

VII. DAM FAILURES

The calculation of risks associated with rare occurrences is a fairly well developed science, with extensive work already completed on methods of determining the risk from events of high and low probability. Many factors are germane to the evaluation of risks from rare events, including the distribution of the exposed population, the likelihood of multiple <u>measurable</u> events leading to the rare event (i.e. fault and event trees) and the applicability of historical data to present circumstances. Different methods are appropriate for calculating the risk from high-probability events and low-probability events (for a discussion of the differences, see Nuclear Regulatory Commission 1975, pp. 12-15). Inhaber has applied to a low-probability event (catastrophic dam failures) the method appropriate for calculating the risk from a high-probability event. In the case of low-probability events where available historical data provide too small a sample to accurately assess causal relations, methods of low-probability risk analysis must be used.

Even if the historical data were plentiful in this area, Inhaber's calculations would be insufficient, for he fails to give consideration to conditions that influence future risk patterns that were not relevant to past data. These include, but are not limited to, improved safety conditions, larger dams affecting larger flood-plain populations, and better evacuation plans.

As it is, the lack of extensive historical data did not inhibit Inhaber from using limited statistics and inappropriate methods to calculate a risk value. A good example is his use of data from only four dam failures to determine the average injury risk from all such catastrophes. Furthermore, three of these dams were not hydroelectric dams.

VIII. SUMMARY

Inhaber's calculation of risks associated with hydroelectric dams relies primarily on historical data in contrast to the approach that he applies to the other renewable technologies. Where sufficient historical data exist, this is a reasonable thing to do. Unfortunately, the historical data used by Inhaber are so limited that they are insufficient to provide any believable values. This is not to say that Inhaber's hydroelectric risk values must be wrong. They are simply based on insufficient data and cannot be said to describe the average risk at hydroelectric facilities. Much better data, covering a much greater number of hydroelectric plants, are required before any risk calculations can be meaningfully undertaken.

CHAPTER 5:

SOLAR-THERMAL-ELECTRIC SYSTEMS

I. INTRODUCTION

In Appendix E of AECB-1119, Inhaber computes the risks associated with solar-thermal-electric technologies. A solar-thermal-electric plant is one of several proposed schemes for making electricity from sunlight. Its attractiveness lies largely in the fact that in important ways it is similar to conventional steam-electric plants. Like the conventional plants, the solar-thermal-electric plant would have a boiler, turbines, generators, and some mechanism for heat removal (either wet or dry cooling towers). Unlike conventional plants, its boiler (normally called the "receiver") would be mounted on a tall tower, where the sun's energy would be focused on it by a large field of steerable mirrors (called heliostats). This focused energy would boil water, and the steam would drive ordinary turbine-generators. To provide energy storage so that the plant could operate during short periods of cloudiness and for a few hours after sunset (in order to meet the intermediate load portion of the electricity demand) most designs have assumed that a heat-transfer fluid would be circulated during the day through a large volume of rock, from which the heat could later be extracted by the same process.

Inhaber's analysis is based on a plant of particularly materials-intensive design, scaled up for operation under Canadian weather conditions. No experienced analyst has suggested that this would be a rational way to produce electricity. A solar-thermal-electric plant could make economic sense in the foreseeable future only if located in conditions of maximum sunniness and minimum humidity, and only then if operated in a fuel-saving mode (i.e. shutting down relatively cheaply-built fossil fuel plants when the solar plant is producing electricity and using the solar-fossil combination to meet intermediate load demands). The intermediate demand for electricity is that part of the demand between the baseload and peak loads. It generally occurs during the period from about 7AM to about 9PM, so it is reasonably well matched in time to the period when sunlight is available. As a result, a moderate amount

of energy storage will allow a reasonably-sited solar-thermal-electric plant to achieve the desired level of performance.

II. MATERIALS ACQUISITION(1)

In section E-ii of AECB-1119, Inhaber computes the risks associated with materials acquisition for his solar-thermal-electric plant. His data for materials requirements come from a JPL interoffice communication (McReynolds 1976) that was part of the preliminary work for two of Inhaber's other sources (Herrera 1977 and Caputo 1977). One of those sources explains that this plant design was intentionally chosen to represent the upper end of the range of estimates for materials requirements (Herrera 1977, sec. 3.1), so it is probably misleading to call this a "representative" solar-thermal-electric plant.

Inhaber borrows from this interoffice communication not only the materials requirements for the power plant, but also his entire method for evaluation of risks associated with materials acquisition, component fabrication, and construction of the power plant. In doing so, he propagates many errors contained in the source (many of which were found and corrected in the final JPL reports). In the materials acquisition stage he has copied the memo's use of anthracite mining for coal mining (a factor of 3 error in risk). He incorrectly counts the risks of acquisition of the full weight of the concrete requirement in both "cement" and "non-metal mining" (which he has mislabeled "sandstone"). As explained in Chapter 1, Section III.C.3, concrete is only about one-sixth to one-fourth cement by weight, and cement "acquisition" includes the quarrying of the limestone from which cement is made. Consequently, a proper accounting would involve the risks of "acquiring" about four-fifths of the concrete requirement in "non-metal mining" and about one-fifth of the concrete requirement in "cement". Inhaber's procedure misstates the risk of acquiring the materials for concrete by a factor of 2.

Inhaber concludes that the materials-acquisition and component-fabrication phases of creating a solar-thermal-electric power plant entail 1.74 worker-days lost per megawatt-year of electricity produced. This is in reasonable agreement with the final report to which Inhaber's source was a

(1) See the copy of Inhaber's Table E-2 (pp. 136-137), where these risks are computed.

TABLE E-2

Material Acquisition and Construction Risk for Solar Thermal Plants (per megawatt-year net electrical output) (16)(g)

	Materials (metric tons)	Man-hours per metric ton accident	Man-hours (c) (d)	Man-days lost, accident	Man-days lost, illness	Deaths ($x 10^3$)	Man-days lost, total (f)
Material & Equipment Acquisition							
Iron Ore Mining (e)	63	0.44	27.7				
Bauxite Mining (e)	8.7	0.50	4.4	0.012	0.0003	0.001	0.02
Sandstone (h)	180	0.50	90.0	0.025	0.0006	0.016	0.12
Hard Coal Mining (a)	56	1.20	67.2	0.067	0.0014	0.040	0.31
Flat Glass (h)	6.3	38	239	0.007	0.0001	0.007	0.05
Cement (h)	174	0.84	146	0.049	0.0001	0.006	0.08
Steel	39	10	390	0.155	0.0039	0.014	0.24
Fabricated Metal Products	8.3	149	1240	0.553	0.0167	0.056	0.90
Aluminum	2.2	12.4	273	0.011	0.0003	0.001	0.02
Total	616			0.879	0.0234	0.141	1.74
Construction (b)							
Plumbing	5.0	1170	5850	2.057	0.0702	0.673	6.16
Electrical	8.7	56	490	0.131	0.0029	0.043	0.39
Roofing & Sheet Metal	25.3	162	4100	3.53	0.0509	1.15	10.5
Concrete	173	0.33	57.1	0.024	0.0007	0.010	0.08
Miscellaneous Contracting	8.3	114	946	0.613	0.0083	0.196	1.80
Total			11000	6.35	0.133	2.07	18.9

Total	11000	6.35	0.133	2.07	10.9

Notes:

(a) If soft coal mining is specified instead of hard coal mining, results are modified somewhat. The man-days lost due to accidents decrease by 6%, that due to illness by 4%, and deaths by 22%. This will affect conclusions only slightly.

(b) The total weight of materials used in construction is not the same as in material acquisition, due to the intermediate fabrication stages.

(c) One man-year is assumed to be 2000 man-hours.

(d) This column is the product of the previous two.

(e) Iron ore and bauxite mining combined into the category of metal mining for risk assessment.

(f) Death assumed to equal 6000 man-days lost.

(g) Original data was for a 1000 megawatt plant with a load factor of 0.7. Since a lifetime of 30 years was assumed, data was divided by 30 x 1000 x 0.7.

preliminary contribution. That report concluded that these same phases entailed 2.68 worker-days lost per megawatt-year for both the power plant and storage system (Caputo 1977, p. 6-36).

III. CONSTRUCTION(2)

In his analysis of the risks of construction of the solar-thermal-electric system, Inhaber transcribes more errors from the McReynolds memorandum. He assigns the construction of the plant to completely inappropriate industrial categories and uses an impossible figure for the labor intensity of at least one of those industries. Inhaber has the towers for the receivers built by plumbers, the heliostats' mirrors assembled by electricians, and the heliostat supports and guides built by roofers (McReynolds 1976, Tables 2a and 8a; AECB-1119's Table E-2). In reality the towers would be constructed by an industry more closely resembling fabricated structural steel work (SIC 3441), the heliostat assemblies by an industry more closely resembling the automobile industry (which also manufactures large numbers of identical steel, glass, and aluminum devices), and the receivers would be constructed by the "fabricated metal products" industry in the component-fabrication stage. (The actual functions performed in various industrial categories can be found in U.S. Office of Management and Budget 1972.)

Using his erroneous industry assignments for construction, Inhaber concludes that construction of the power plant entails 18.9 worker-days lost per megawatt-year. Fifty-six percent of these occupational hazards are produced by having the heliostats constructed under conditions of risk similar to the roofing and sheet metal industry. Since the heliostats would be a mass-produced item (they would be needed in large numbers), about the only on-site construction they would require would be laying their foundations, bolting them down, and connecting them to power and control cables. None of this work would be very hazardous, and virtually none of it would be off the ground. This is important, because roofing and sheet metal work _is_ done almost entirely off the ground, which is part of the reason that it has an abnormally high injury rate. Consequently, Inhaber's use of this industry is

(2) See the copy of Inhaber's Table E-2 (pp. 136-137), where these risks are computed.

particularly inappropriate.

One-third of the occupational risk Inhaber finds for the construction phase of a solar-thermal-electric plant arises from the use of plumbers to construct the receiver towers. As we note elsewhere, Inhaber has used a value of 1174 worker-hours per metric ton in this industry. This is a level of worker productivity almost ten times lower than observed in any other industry analyzed by Inhaber. Use of plumbers is highly inappropriate for the business of constructing towers. If we are to believe Inhaber, construction of just the towers for one 1000-megawatt power plant would require over 61,000 worker-years, which is about twice the construction labor requirement indicated by another of Inhaber's sources for an entire 1000 MWe solar-thermal-electric plant (ERDA 1977, pp. 18,22).

Inhaber totaled the labor requirements for the construction phase of his solar-thermal-electric plant, finding that it would require over 11,000 worker-hours per megawatt-year. He could easily have compared this figure with the corresponding figures from two of his other sources. They indicate that plant construction would require between 1900 and 2267 worker-hours per megawatt-year, again including storage (which Inhaber leaves to a later section) (Caputo 1977, p. 6-12; ERDA 1977, pp. 18, 22). The discrepancy in this quantity between Inhaber's value and the values in two sources on which he relied for other information is thus about a factor of five.

For the total occupational risk of construction, Inhaber finds 18.9 worker-days lost per megawatt-year. Caputo's estimate for this risk was 3.78 worker-days lost per megawatt-year, a factor of 5 lower (Caputo 1977, p. 6-36). Caputo's estimate, moreover, included construction of both the power plant and its storage system.

IV. EMISSIONS

In contrast to occupational impacts, for which he made his own calculations, Inhaber takes directly from Caputo (1977, p. 6-35) his value of 0.5-1.5 person-days lost per megawatt-year from sulfur oxide emissions associated with making the steel for the solar-thermal-electric plant.

V. OPERATION AND MAINTENANCE

In his discussion of operation and maintenance risk for solar-thermal-electric plants, Inhaber writes (AECB-1119, pp. E-4/E-7):

"Reference 63 indicates that about 2.8 man-hours per kilowatt of capacity is required. Allowing 2000 man-hours per man-year, this is 14 man-years per megawatt of capacity. Assuming that the load factor is 0.7 and that there are no other losses, this translates to 2 man-years per megawatt-year of net electrical output."

Reference 63 is Herrera (1977), who says (Section 5.3):

"The operation and maintenance of power plants with a total of 100 GWe [electrical gigawatts] of capacity would require about 67,000 men for the solar thermal plants including cleaning the mirrors every 5 weeks ... , while 15,000 and 9,000 men would be needed respectively at coal and nuclear plants."

First, let us check Inhaber's calculation:

$$\frac{67{,}000\ \text{worker-yr}}{100\ \text{GWe-yr}} \times \frac{1\ \text{GWe}}{10^6\ \text{kWe}} \times \frac{2000\ \text{worker-hr}}{\text{worker-yr}} = \frac{1.34\ \text{worker-hr}}{\text{kWe-yr}}$$

instead of 2.8. Continuing to check Inhaber's calculation, we find:

$$\frac{2.8\ \text{worker-hours}}{\text{kWe-yr}} \times \frac{1000\ \text{kWe}}{\text{MWe}} \times \frac{\text{worker-yr}}{2000\ \text{worker-hours}} = \frac{1.4\ \text{worker-years}}{\text{MWe-yr}},$$

rather than 14.

Assuming that this is a typo, we can get Inhaber's next number by dividing 1.4 worker-years/MWe-yr by the load factor. At this point Inhaber is off by a factor of about 2 (2.8/1.34), but he reduces this error somewhat by not counting risks for the supervisory personnel (16% of his work force), who, like everyone, suffer some hazards in their work. He more than compensates for this slight generosity to solar-thermal-electric plants, however, with his next sentence. "Because most of the labor is consumed in cleaning mirrors, we can assign this labor, for purposes of risk calculation, to the roofing and sheet metal category." (AECB-1119, p. E-7). The roofing and sheet metal

sector of the construction industry has nothing in common with the business of cleaning mirrors. This industrial category consists of people who work on roofs and ladders, from which they fall and hurt themselves enough to produce accident rates higher than those of normal coal mining. Inhaber could more reasonably have chosen to assign the mirror-cleaners to the "services to buildings" category, which includes window-washers, and which has accident rates about a factor of five lower than the roofing and sheet metal industry.

Actually, the very idea that an electric power plant will be maintained by hordes of mirror-cleaners is probably naive. It is perhaps more realistic to think of the mirror-cleaning being done by special machines, which operators would drive slowly through the fields of heliostats. This job would be both less labor-intensive and less risky than cleaning the heliostat mirrors by hand.

Inhaber concludes that the total occupational risk from operation and maintenance of solar-thermal-electric plants is 9.0 worker-days lost per megawatt-year. A simple estimate of the accuracy of this calculation can be made by assuming that work on a solar-thermal-electric plant will be similar in risk to work on a coal-burning power plant. Caputo (1977, p. 6-12) indicates that 1900 worker-hours per megawatt-year are required for operation and maintenance of a solar-thermal-electric plant, while 407 worker-hours per megawatt-year are required for a coal plant. Multiplying the ratio of worker-hours required by the risks of operating a coal plant gives a range for occupational risk of 1.1 - 6.1 worker-days lost per megawatt-year, rather than 9.0.

VI. ENERGY BACK-UP

This whole aspect of Inhaber's risk accounting makes no sense, as discussed in Chapter 1, Section III.5.2. Inhaber has scaled his solar-thermal-electric plant and energy-storage system (see below) to produce more annual energy output than the conventional plants that serve as his reference point for comparison. In this situation, adding the risk of energy "back-up" to the risk of the solar plant is simply wrong. By erroneously adding to the risk per 1000 megawatt-years of solar-produced electricity the risks of 190 megawatt-years of coal "back-up", Inhaber generates 85 percent of the total

risk he attributes to solar-thermal-electric plants. What he has done amounts to an argument that solar plants are dangerous because coal plants are much more dangerous. This argument is nonsense.

VII. ENERGY STORAGE

Section E-v of AECB-1119 contains Inhaber's computation of risk associated with energy storage. He first quotes a National Academy of Sciences study to the effect that, to produce a baseload solar-thermal-electric plant would require 15-18 hours of storage capacity. The quotation(3) indicates that such a plant would have a load factor of 0.86, while all of Inhaber's calculations have assumed a load factor of 0.70. Inhaber nevertheless takes the average of the NAS figures, 16.5 hours, as the requirement for his reference solar plant. This choice makes Inhaber's solar plant a better annual energy producer than the coal and nuclear plants to which he compares it. Not only does he not credit the solar plant with the performance its large storage system makes possible, however, but he actually charges it with "back-up" energy from coal. (See the preceding section and Section III.B.2 in Chapter 1.)

One of the likely storage systems for a solar-thermal-electric plant is hot rock, using heat-transfer oil to move heat into and out of the rock. Inhaber cites some values for the amounts of rock and oil needed that would have allowed him to compute the risks associated with materials acquisition and construction of such a storage system in a manner similar to that applied to the rest of the materials. (See below.) For reasons that are obscure, Inhaber chose a different (and entirely unsatisfactory) procedure.

He found two tables in different sources listing risks of solar-thermal-electric plants--one contained in the JPL interoffice memorandum that is his main source (McReynolds 1976, Table 1a), and one contained in a JPL report that incorporated work from this memo (Herrera 1977, Table 28). The solar-thermal-electric plants for which the risks were computed differ only in load

(3) The quotation is: "Base-load plants operating for as long as 7500 hours per year will require storage for 15-18 hours to supply power at standard reliability criteria (a loss of load probability of 1 day in 10 years)." (National Academy of Sciences 1976b, p. 74; quoted in AECB-1119 on page E-7.)

factor and in amount of storage, so Inhaber decided that he could compute the risks of energy storage by adjusting the tables so that the load factors are equal and subtracting one table from the other. This would make sense if the entries in the tables were computed using exactly the same methods and data, but in fact, as Inhaber was warned (by the authors of both the memorandum and the final report), errors in the memorandum had been discovered by the time the data for the report were prepared, so that the underlying calculations differ in ways that are not now determinable. The differences, however, completely invalidate this method of risk determination.

Inhaber should have suspected that this was not a proper procedure when he computed the number of deaths expected from construction of the storage system. By his method, the number of deaths associated with the construction of the storage system is <u>negative</u>, even after correcting the obvious mathematical errors in the memo. The number of "negative deaths", if one were to follow Inhaber's method to its absurd conclusion, nearly cancels the other risks associated with storage. Clearly this method of computing risk for the storage system has serious problems.

Inhaber seems to have noticed this odd result, for his Table E-3 notes "Misprint in original. Deaths assumed proportional to man-days lost due to accident." By thus "correcting" his source, he produced a positive number of deaths. The resulting total of worker-days lost, after scaling up to produce 16.5 hours of storage instead of the 6 hours of storage that the JPL report indicated, is comparable to his inflated risk for construction of the rest of the power plant.

Inhaber defends this result as follows (AECB-1119, p. E-8):

> "At first glance the total man-days lost for storage systems, between 8.9 and 15.6, might appear somewhat high in comparison to that lost in construction of the main collector system, 18.9 days. However, there are a number of reasons why computed values are probably correct. First, the ratio of man-days lost in material acquisition as compared to construction is about the same for the main system and the storage system. Second, the ratio of material weight of the 6-hour storage system to the main system was about 0.36 (69), and if we convert this to a 16.5 hour storage system, the weight ratio is then close to 1 (0.36 x 16.5/6).

> This means that the storage system, consisting of rocks and oil, weigh [sic] as much as the collector or main system, consisting of steel, concrete, glass and so on. While the level or [sic] risk per unit weight will clearly depend on the type of material, it is reasonable to expect that the risk incurred in building the main system and the storage system will be of the same order."

This contention is ridiculous. That the ratios Inhaber refers to are similar means nothing, and it should be obvious that the risk involved in acquiring rocks and oil and combining them in a storage system is small compared to the risk of constructing an electric power plant. After all, the rocks that would be used are already fabricated and presumably available at or near the site.

Inhaber later uses the results of this section to represent the risks arising from storage systems for photovoltaics and wind power.

It is not difficult to derive a more reasonable estimate of the risk for constructing the storage system, by means of the procedure one might have expected Inhaber to use--that is, by starting with the materials requirements and multiplying by worker-years per ton and risk per worker-year in the occupations involved. In the introduction to the odd procedure he used instead, Inhaber writes (AECB-1119, p. E-7):

> "Caputo (67) indicated that a system of 2.13 million metric tons of rock and 0.29 million metric tons of heat transfer oil would provide about 6 hours of storage at 70% rated power for a 1000-MW(e) solar plant. However, it is unclear exactly how to convert this into risk data."

Why Inhaber found it "unclear" how to proceed is obscure, since the method he identifies elsewhere as the centerpiece of his study applies perfectly. Dividing the tonnages quoted from Caputo by the lifetime output of the plant (1000 megawatts times 30 years times a load factor of 0.70) gives 101 metric tons of rock and 13.8 metric tons of heat-transfer oil per megawatt-year. Using Inhaber's "non-metal mining" risk for the rock and his "fuel gathering and handling" and "fuel transportation" risks from the oil-to-electricity fuel cycle (plus the conversion, 1700 metric tons of oil per megawatt-year of electricity) for the heat-transfer oil produces materials-acquisition risk for the storage system of about 0.08 - 0.21 worker-days lost per megawatt-year.

Approximating the construction risk by multiplying the total tonnage of the storage system by Inhaber's figures for construction time per ton of concrete and worker-days lost per worker-hour in concrete construction gives about 0.05 worker-days lost per megawatt-year. The sum, 0.13 - 0.26 worker-days lost per megawatt-year, would become 0.36 - 0.72 worker-days if multiplied by 16.5/6 to scale up from Caputo's 6 hours of storage to the 16.5 hours Inhaber has decided to use. This risk figure for the storage system is about 30 times smaller than the one supplied by Inhaber.

VIII. TRANSPORTATION

Inhaber computes the risk associated with transportation by a simple procedure. He adds up the weights of everything that must be moved from one place to another in the whole process of the plant's construction and operation, and then multiplies the total by the risk per unit weight of moving coal by rail, taken from Smith et al. Thus he totals (incorrectly as it turns out) the weight of the iron ore, the steel, the fabricated metal products, the sand, the glass, and so on, from the materials acquisition phase. He then adds 19% of the weight of coal necessary to produce one megawatt-year of electricity from a coal power plant (as a transportation cost of supplying the back-up energy he thinks is necessary), and finally duplicates the erroneous total from the materials-acquisition as the weight of the storage (on the grounds that "the weight of the storage system is close to that of the main system"). He sums these three weights, arriving at a total of 1877 metric tons per megawatt-year, and then multiplies this total by the transportation risk from his coal appendix.

Among the errors in this calculation are the following. First, Inhaber has mis-added the total weight of materials required. Instead of his total of 616, his numbers add to 537.5. But his calculations have also confused cement and concrete, so that the weight of the concrete is counted twice. Removal of these errors reduces by almost half the weight of materials for the plant. In addition, using the risks of shipping coal by rail as a standard for measuring the transportation risks of aggregate for concrete is completely wrong, since 95 percent of aggregate is shipped by truck, and for very short distances (86 percent is shipped less than 50 miles, while coal is shipped an average of 283 miles by rail) (Kearney 1972, pp. 11,19; Federal Energy Administration 1974b,

p. II-7). The other two parts of his transportation calculation are both spurious. The "back-up" coal requirement shouldn't be included at all, given the enormous storage system. Besides, the transportation risks of the coal were already fully accounted for in the section on energy back-up, so including it again here is simply double-counting. And, finally, the rock for the storage system would probably be used in place rather than moved anywhere.

Consequently, even if one accepted the idea that all the other materials were shipped long distances between each processing step, Inhaber's result would be more than five times too large.

IX. OTHER CONSIDERATIONS

In section E-vii, entitled "Other Considerations", Inhaber moves his solar-thermal-electric plant to Canada. This is simply not a sensible idea, because these plants need maximum sunniness in order to be at all feasible. Inhaber recognizes that Canada is less sunny than Arizona, and he presents a mislabeled table documenting the fact. (All of the numbers in it should be ten times larger.) He interpolates from this table that Canada is only 76 percent as sunny as Arizona. To account for this reduced insolation, he multiplies all the previously computed risks by 1/0.76 = 1.32.

Inhaber's whole line of argument in this section is incorrect. Aside from the fact that it is silly to consider a solar-thermal-electric plant in Canada, he uses the wrong information for calculating the scaling factor. Solar-thermal-electric plants operate on only the direct beam portion of the insolation (the part of the incoming radiation that consists of parallel rays coming from the direction of the sun), and this is not a simple function of total insolation. Depending on the plant's location, using the right information could make the correction factor either larger or smaller. Further, the scaling up of all the risk factors is incorrect. To a first approximation, only the heliostat area needs to be increased. Finally, in multiplying all of the risk factors by 1.32, Inhaber has increased the back-up energy contribution from 19% to 25%. If we add this 25% to the 86% capacity factor implied by the massive storage system, we find that Inhaber's solar-thermal-electric plant evidently produces an average of 111% of its rated power!

X. SUMMARY

In Appendix E, Inhaber has tried to calculate the risks associated with constructing, operating, and providing energy back-up for a solar-thermal-electric plant scaled up to produce baseload electricity under sunniness conditions similar to Chicago or New York. Such an exercise can say very little about the risks of solar-thermal-electric power in plausible locations.

TABLE 5.1

EFFECTS OF PARTIAL CORRECTIONS ON AECB-1119'S
ESTIMATED RISK OF SOLAR-THERMAL-ELECTRIC SYSTEMS
(All figures in occupational worker-days lost (WDL) or
public person-days lost (PDL) per megawatt-year)

	AECB-1119		Partial corrections	
	Occup WDL*	Public PDL*	Occup WDL	Public PDL
Materials acquisition	2.30		1.74	
Construction	24.95		3.17	
Transportation	7.5-25	3.6-8.3	1.0-3.6	0.5-1.2
Emissions from steel-making		0.7-1.7		0.5-1.5
Operation and maintenance	11.75		1.1-6.1	
Energy storage	11.4-20.6		0.4-0.7	
Energy back-up	4.5-18.3		0	
		5.1-505		0
TOTAL	62-103	9.4-515	7.4-15.3	1.0-2.7

* All of these numbers contain the factor of 1.32 from moving the solar plant to Canada (see Section IX, above).

Table 5.1 shows Inhaber's results both before and after correction of the more obvious and easily removed errors identified in this chapter. We assume that solar-thermal-electric plants will not be located in cloudy northern climates, so the factor of 1.32 applied in moving the plant from Arizona (Section

IX, above) has been removed. The figure for the occupational risks of materials acquisition and component manufacture is simply Inhaber's figure divided by 1.32. Inhaber's construction risks were inflated by this factor and by a five-fold overestimate of the labor requirements. In addition, his choices of occupational categories for risk calculation were highly inappropriate. Our estimate of this risk was produced by multiplying the 1974 risks of the industry group "contract construction" (U.S. Department of Labor 1978) times the labor requirements given by Caputo (1977, p. 6-12) for a solar-thermal-electric plant identical to that analyzed in AECB-1119. The entire risk for 16.5 hours of storage as estimated in Section VII, above, is entered under "Energy storage". The transportation risks have been recalculated using Inhaber's calculation procedure on Caputo's materials requirements, but leaving out the double-counted risks of transportation of coal for energy back-up and the transportation of rock for the energy storage system. It is assumed that the rock will not be moved farther than materials are moved in the internal operations of non-metal mining and concrete work. The public risks arising from sulfur emissions associated with steel-making are reduced to those of Caputo, which is what Inhaber's risk figures were before moving the solar-thermal-electric plant to Canada. Occupational risks from plant operation and maintenance are scaled from the risks of operating coal plants as described in Section V, above. Finally, of course, the erroneous energy back-up risk was removed.

These partial corrections reduce Inhaber's risk figures for solar-thermal-electric plants by factors of about 7 for occupational risks, and between 9 and 190 for public risks.

CHAPTER 6: PHOTOVOLTAICS

I. INTRODUCTION

In Appendix F of AECB-1119, Inhaber attempted to compute the risk of direct conversion of sunlight to electricity using solar photovoltaic cells. Ignoring the variety of technically feasible semiconductor materials and the diversity of feasible power-system configurations, the analysis of Appendix F limits itself to a single, "representative" design employing silicon solar cells in flat-plate arrays. Inhaber assumes that these ground-mounted arrays are deployed in a 1000-megawatt-average, central-station, photovoltaic power plant with 16.5 hours of energy storage.

The approach and much of the data of Inhaber's Appendix F are taken from his analysis of a solar-thermal-electric system. (See Chapter 5.) Inhaber ascribes risk to six sources. These are: (1) materials acquisition and construction of the photovoltaic power plants; (2) air pollution due to sulfur oxide emissions during steel manufacturing; (3) transportation of ores, primary materials, and fabricated products; (4) construction of energy storage apparatus; (5) operation and maintenance of the photovoltaic plants; and (6) operation of the coal back-up system. Inhaber found that the sum of the risks in all these categories resulting from operation of the photovoltaic power system is between 152 and 700 total person-days lost per megawatt-year of electricity.

Inhaber's conceptual error in attributing risks of coal back-up to the photovoltaic system contributes 75 percent of his upper-limit estimate of the total risk from this technology. Inhaber's error in estimating the size of the construction labor force and his additional error of selecting the wrong composition for this labor force combine to generate 13.3 percent of his total upper-limit risk estimate for photovoltaics. His large storage system, unjustified for a plant operating in the fuel-saver mode and useless for a plant with no machinery to convert heat to electricity, generates 3 percent of the total risk estimated for this technology.

II. MATERIALS ACQUISITION

The photovoltaic power plant that Inhaber claims to analyze is referenced to a design for a 100 electrical-megawatt (rated) power plant by Charles Bell (1975). Although claiming on page F-1 to focus his analysis on the design suggested by Bell, Inhaber ignored the design data and analysis contained in the Bell report. Instead, Inhaber relied for his materials requirements on the same unpublished JPL memorandum from which he constructed his analysis of solar-thermal-electric systems (McReynolds 1976). Although he was aware that the data and calculations of this interoffice memorandum were refined and updated before being incorporated into the final published report of this project (AECB-1119 reference 6: Caputo 1977), Inhaber still chose to base his analysis of materials acquisition and construction of a photovoltaic power plant on the preliminary estimates of the McReynolds memo. The numbers in this memo were considered to be an upper-bound estimate by the project for which it was developed. Most significantly, in the final report aluminum structural members had been substituted for the steel used in the preliminary design analyzed by McReynolds, thus eliminating the need for the quantities of steel and fabricated metal products called for in the McReynolds memo. Fabrication of metal products from steel generated two-thirds of Inhaber's risk from materials acquisition.

The McReynolds memo analyzes a power plant rated at 1000 electrical megawatts (peak). In order to scale the materials requirements contained in this memorandum to correspond to a 1000 electrical-megawatt (average) plant, Inhaber multiplied each of McReynolds' materials requirements by at least two factors. The first is a reasonable ratio of peak-to-average power for a photovoltaic plant. This ratio has a value between 3 and 6 depending on location and plant design. Inhaber chose a value of 4, referenced to the Project Independence Report (Federal Energy Administration 1974a). This factor is all that is necessary to convert the materials data from the McReynolds memo to an estimate for an extremely materials-intensive photovoltaic power plant capable of producing an average power output of 1000 megawatts. Asserting, however, that this increase still leads to an underestimate of the materials requirements, Inhaber inflated all of McReynolds' materials numbers by a second, completely unjustified factor, the Area Correction Factor.

Inhaber's explanation of the Area Correction Factor is as follows (AECB-1119, p. F-1):

> "In spite of this [Peak-to-Average] correction, the data of Reference 16 [the McReynolds memo] is still an underestimate. Herrera (76) suggests that the ratio of land occupied to energy gathered by the system of Reference 16 is 3800 square metres per megawatt-year electrical output. However, in a more comprehensive analysis, Rich and Hewitt (77) show that [the] ratio is in fact 34,500 square metres per megawatt-year electrical output. Using the most favourable assumptions towards the data of Reference 76, i.e., that it refers to average power and that of Reference 77 refers to peak power, the area of the Rich and Hewitt system is still 2.26 [sic] times that of the system of Reference 16. In summary, the latter set of data is multiplied by 4 x 2.27 = 9.1 in order to make it comparable to data in other systems."

This paragraph reveals a thorough confusion about the meaning of peak and average power ratings. First, it is not possible that any figure having the units of "square meters per megawatt-year electrical output" could refer to peak power; peak power has units of megawatts, not megawatt-years. Second, ignoring the problem of the units, if Herrera's area figure referred to average power and Rich and Hewitt's figure to peak power, as Inhaber says he is assuming, then the discrepancy in the figures would be four times larger than the simple ratio of the areas (assuming a peak-to-average insolation ratio of four), not four times smaller. It takes more area to make an average megawatt than to make a peak megawatt. All this confusion about peak and average power is in a way a side issue, however, because the fact is that <u>no</u> correction to the JPL figure is warranted. One does not need to scour the literature to confirm this; a two-minute calculation suffices, as follows: Average insolation on a horizontal surface in the U.S. is about 180 watts per square meter, averaged over seasons and night and day. Assuming that the collectors cover half the land area charged to the plant and that the efficiency of the cells in converting sunlight to electricity is 10 percent, and using the 30-year plant lifetime assumed by Inhaber, yields

$$180\ W/m^2 \times 0.10 \times 0.50 \times 30\ year = 270\ watt\text{-}years/m^2,$$

which gives 3700 square meters per megawatt-year, essentially the same number

as JPL's.

Inhaber further increased his estimate of the materials requirement for silicon and glass by an unwarranted Weight Correction Factor of 2.8 (AECB-1119, page F-3). The Weight Correction Factor is the ratio of the weight of encapsulated arrays in a study by Daey Ouwens (Inhaber reference 78: Ouwens 1976) divided by the weight of glass and silicon initially estimated by Inhaber from the McReynolds memo. The total weight of glass and silicon initially estimated by Inhaber, 400 kilograms per kilowatt (average), was 2.27 times the sum of McReynolds' estimate for glass and metallurgical-grade silicon, 174 kg/kW(avg). The Weight Correction Factor increased the estimated weight of glass and silicon in Inhaber's final estimate for photovoltaic arrays to 2.8 x 400 kg/kW(avg) = 1100 kg/kW(avg). This additional increase in the materials requirements is completely unjustified.

Assuming a 10 percent array conversion efficiency, and using Inhaber's assumptions of a peak-to-average power ratio of 4, implies that his photovoltaic power system, including supports and foundations but without storage, weighs approximately 270 kg per square meter or 2700 kg per kilowatt (peak). The steel, glass, silicon, aluminum and concrete required to build Inhaber's photovoltaic power system weighs 5.5 times as much per unit area as the same materials in the system analyzed by Caputo, which weighs only 48.7 kg per square meter (Caputo 1977). By comparison, a ground-mounted, flat-plate, silicon photovoltaic array which has been commercially available for several years, the 12-volt LECA model array from Hughes Spectrolab Inc., weighs approximately 30 kg per square meter, excluding the concrete used for foundations. Assuming 100 kg of concrete (0.045 cubic meters) is used in foundations for each square meter of these arrays, the Spectrolab system still weighs less than half as much as Inhaber's design. The cost of Inhaber's extremely materials-intensive array design would surely make his photovoltaic power system impractical and uneconomic.

Inhaber applied a "Materials Handling Factor" of 1.38 as a further inflation to the sum he calculated for materials requirements of his photovoltaic power plant (207.8 metric tons per megawatt-year, excluding concrete). He used this product (1.38 x 207.8 te/MWy = 287 te/MWy) to determine the size and composition of the construction labor force for his photovoltaic power plant.

The Materials Handling Factor is a ratio consisting of the total weight of finished materials, excluding concrete, estimated by Inhaber to be needed for construction of his solar-thermal-electric plant (47.3 te/MWy) divided by the weight of materials that he estimated were actually embodied in the plant, again excluding concrete (34.3 te/MWy). This ratio (47.3/34.3 = 1.38) is entirely an artifact of Inhaber's errors; the two numbers should have been identical, giving a ratio of 1.0. (See Chapter 1, Section III.G.3, above.)

Inhaber estimated the occupational risk of materials acquisition for his photovoltaic power plant to be 8.8 occupational worker-days lost per megawatt-year, based on the (incorrect) data contained in the McReynolds memo. The final published report of the JPL project (Caputo 1977), for which the McReynolds memo was prepared, contains a more reasonable estimate for the materials requirements of a photovoltaic power plant. For our first-cut corrections to AECB-1119, we apply Inhaber's methodology to the materials requirements contained in Caputo (1977). We assume, as Inhaber did, that 4 tons of bauxite mining are required for each ton of aluminum, one ton of non-metal mining for sand is required per ton of glass, and 2.1 tons of sand are required per ton of silicon. We also assume that 0.8 tons of sand and 0.2 tons of cement are required per ton of concrete used. Applying the occupational risks per metric ton estimated for these materials by Inhaber in Table F-2 of AECB-1119, and multiplying the resulting risks by 1.32 to account for Canadian climatic conditions, we reduce Inhaber's estimate of risk from materials acquisition by a factor of 14, to 0.65 worker-days lost per megawatt-year.

III. CONSTRUCTION

Although Inhaber relied on the McReynolds memorandum for his estimates of materials requirements for photovoltaics, he completely ignored McReynolds' analysis of the labor requirements for photovoltaic plant construction. Instead, Inhaber fabricated a schedule of construction labor requirements which he derived from McReynolds' estimate of construction labor for a solar-thermal-electric plant. In so doing, Inhaber added to McReynolds' photovoltaic-plant construction requirement an additional 20,000 worker-hours per megawatt-year of "roofing and sheet metal" work, 4,250 additional worker-hours per megawatt-year of "electrical" work, and 9,000 extra worker-hours per

megawatt-year of "miscellaneous contracting". By introducing these additional and unnecessary worker-hours, Inhaber has expanded the size of the plant construction labor requirement identified in his source by a factor of 70. Inhaber's construction labor requirement is 42 times as large as that estimated by Caputo.

Employment of this enormous and inappropriate construction labor force results in Inhaber's risk estimate of 94.8 occupational worker-days lost per megawatt-year. Despite his assertion on page F-4 of AECB-1119 that the assignment and distribution of labor among construction trades is irrelevant to the resulting risk assessment, the accidental injury and death rate of the unnecessary roofing and sheet metal workers dominates all other risks in the construction of Inhaber's photovoltaic power plants. Correction of Inhaber's error in estimating the occupational risks of plant construction cannot, however, be accomplished by substituting the incorrect estimates of McReynolds for those of Inhaber. A more reasonable estimate of the labor requirements of photovoltaic plant construction is contained in Caputo (1977). For our initial correction to Inhaber's estimate of occupational risk of photovoltaic plant construction, we use the plant construction labor requirement given in Caputo (808 worker-hrs per megawatt-year) multiplied by the average occupational risk experienced in the broad SIC industry group called "Contract Construction". This broadly defined group includes all of the industrial occupations identified in Inhaber's photovoltaic plant construction labor force. Applying Caputo's labor requirement per megawatt-year to 1974 U.S. risk data (U.S. Department of Labor 1978) and multiplying the result by 1.32 to account for Canadian climatic conditions, reduces Inhaber's estimate of occupational risk from plant construction by a factor of 53, to 1.78 worker-days per megawatt-year. Inhaber's error in estimating the risk of constructing the photovoltaic power plant accounts for almost half of his estimate of the total occupational risk of this technology.

IV. EMISSIONS

Inhaber calculated the emissions risk of his photovoltaic power plants by multiplying the estimated public health risk from sulfur oxide emissions during steel manufacturing for his solar-thermal-electric system by 1.1, his estimate of the ratio of the weights of steel required in constructing these

two types of plants. (See Section III.E. of Chapter 1 above.) Since the final photovoltaic-plant design considered by the JPL project contains almost no steel, in contrast to the preliminary version whose materials requirements Inhaber used, applying Inhaber's method of scaling emissions to this newer design would produce a low risk estimate indeed. Such a result would simply be an artifact of the defect of the method, namely, its being based only on steel. Doing a decent job of estimating emissions risks from materials acquisition would require starting from scratch, and is beyond the scope of this critique. We settle for the crudest partial correction here, namely, removing the factor of 2.27 inflation in Inhaber's result that comes from this inflation of the steel requirement taken from McReynolds. This partial correction reduces Inhaber's estimate of public health risk due to emissions to a range of between 0.32 and 0.96 public person-days lost per megawatt-year.

V. OPERATION AND MAINTENANCE

Inhaber asserts on page F-4 of AECB-1119 that the operation and maintenance (O & M) requirements for a photovoltaic power plant are the same as those for a solar-thermal-electric plant of equivalent capacity. He referenced this assertion to the Caputo study (1977). He then applied to photovoltaics the estimated risk of operation and maintenance for his solar-thermal-electric plant. Inhaber estimated occupational risks of photovoltaic power plant operation and maintenance to be 11.8 worker-days lost per megawatt-year.

As we showed in the preceding chapter on Inhaber's solar-thermal-electric system, the dominant source of risk from O & M activity is the misconceived assignment of 84 percent of the O & M labor to roofing and sheet metal workers. Inhaber assigns the remaining 16 percent of the labor force to allegedly risk-free work in supervisory roles. The assignment of operation and maintenance labor to roofers was justified by Inhaber for his solar-thermal-electric plant by his assertion that they were employed to clean the heliostat mirrors. It is extremely unlikely that the ground-mounted, flat-plate photovoltaic arrays will be cleaned manually by roofing and sheet metal workers. It is far more likely, as assumed by Caputo, that the arrays will be cleaned by truck-mounted automatic cleaning devices.

It is reasonable to assume that operation and maintenance of a

photovoltaic power plant will not be any more dangerous per worker-hour than operation of a conventional coal power plant. An upper-limit estimate for the occupational risks of operation and maintenance of a photovoltaic power plant operating in the Southwestern U.S. can be calculated by multiplying Inhaber's estimate of the operation and maintenance risk for a coal electric plant by a ratio consisting of Caputo's estimate of operation and maintenance worker-hours per megawatt-year for a photovoltaic plant divided by Caputo's estimate of operation and maintenance worker-hours per megawatt-year for a coal electric plant. To account for the effect of Canadian climatic conditions on the size of the collector area, we multiply this product by 1.32. (See Section IX, below.) Using this approach, the "corrected" estimate for risk from photovoltaic plant operation and maintenance is 1.4 to 8.0 worker-days lost per megawatt-year.

VI. ENERGY BACK-UP

On page F-6 of AECB-1119, Inhaber asserts:

> "The photovoltaic system has approximately the same characteristics in terms of peak power and zero power intervals as does solar thermal electricity. Its need for energy back-up is then the same."

This assertion, which is both erroneous and irrelevant, introduces the largest contributor to total risk from the photovoltaic system. The risk from coal back-up for the photovoltaic system is taken from Inhaber's analysis of the solar-thermal-electric system. Inhaber estimated the occupational risks of the coal back-up to be between 4.5 and 18 worker-days lost per megawatt-year; the public health risks he estimated at between 5.05 and 505 public person-days lost per megawatt-year. As explained in Chapter 1, Section III.B.2, the photovoltaic system analyzed by Inhaber needs no net back-up energy. Both the occupational and public risks of this stage of the energy cycle should therefore be zero.

VII. ENERGY STORAGE

Inhaber's analysis of risk from energy storage for the photovoltaic power plant is taken entirely from his analysis of his solar-thermal-electric system. It is discussed in detail in Chapter 5, above. The total risk due to

materials acquisition and construction of the energy storage facilities for the photovoltaic power plant is estimated by Inhaber to be between 11.4 and 20.6 person-days lost per megawatt-year of output.

Inhaber asserts that the storage requirements for the photovoltaic and solar-thermal-electric plants are identical. This leads to an engineering absurdity, namely storage of the electric energy output of the photovoltaic plants as heat in a storage system using hot oil and rocks. This storage technology converts the electricity from the photovoltaic arrays, which is of high thermodynamic quality, to thermal energy of relatively low thermodynamic quality. Furthermore, a photovoltaic power plant, of course, is incapable of changing the heat back to electricity. In other words, this is a storage system in which energy can be stored but not removed (as electricity) without building an adjacent thermal-electric plant. Inhaber's storage system is not only over-sized and inappropriate, it is useless in the selected application. No dedicated energy storage system is necessary for operation of the photovoltaic power plant in a fuel-saver mode. In our partial corrections to AECB-1119, we omit the unnecessary storage system entirely.

VIII. TRANSPORTATION

On page F-7, Inhaber states that he "will assume that the transportation risk is proportional to the weight of materials, equipment, back-up and storage" facilities. Inhaber estimated the occupational and public health risks due to transportation of materials for his photovoltaic power system by multiplying his estimates of risk due to transportation of his solar-thermal-electric system by a ratio consisting of the estimated weight of his photovoltaic system, including back-up and storage, divided by the estimated weight of his solar-thermal-electric system, including back-up and storage. As we showed in Chapter 5, above, the weight of Inhaber's solar-thermal-electric system is computed incorrectly and the risk due to transportation is significantly overstated. The storage and back-up systems are inappropriate and unjustified for Inhaber's "near-term" photovoltaic power plant. The occupational risk from transportation of the materials for the photovoltaic power plants, their storage sub-systems, and the coal back-up system is estimated by Inhaber to be between 10.2 and 34.2 worker-days lost per megawatt-year. Inhaber estimated the public health risk from this stage of the photovoltaic

energy cycle to be 4.4 public person-days lost per megawatt-year.

There are several significant errors in Inhaber's analysis of risk from transportation. He has double-counted the risk of transporting coal for back-up. This is already included in the analysis for the energy back-up stage of the fuel cycle. The assumption made by Inhaber on page F-7 that all materials for the photovoltaic power plants, the storage sub-systems, and the coal electric back-up systems will be transported by rail the average distance traveled by U.S. coal is untenable.

A conservative estimate of the corrected risk from transportation of materials for a photovoltaic power plant can be constructed by assuming rail transport for the average haul length of U.S. coal for only the concrete, silicon, aluminum, and glass required to construct Caputo's photovoltaic power plant, plus the bauxite from which the aluminum would be manufactured and the non-metal mining required for acquisition of silicon and glass. We multiply this weight by 1.32 to account for the larger collector area required in the cloudy Canadian climate. This correction reduces Inhaber's estimate of the amount of materials to be transported, 2,590 metric tons per megawatt-year, by a factor of almost seven, to 390 metric tons per megawatt-year. It reduces Inhaber's estimate of the occupational risk from transportation to a range of 1.1 to 3.9 worker-days lost per megawatt-year. The public health risk of transportation is reduced to a range of 0.56 to 1.29 public person-days lost per megawatt-year. It is not possible, short of completely redoing the analysis, to estimate accurately a true "corrected" value for this stage of the energy cycle because this would require disaggregating the transport modes and the distances that the required materials typically travel to a construction site.

IX. OTHER CONSIDERATIONS

The section in Appendix F of AECB-1119 entitled "Other Considerations" presents the rationalization for Inhaber's use of the Canadian-Insolation Escalation Factor of 1.32 to inflate the risk values of his reference solar thermal and solar photovoltaic systems. This factor is applied to all his previously-derived risk estimates before these values are incorporated into Table F-4. While it is not unreasonable to increase the size of the collector

field for a photovoltaic power plant which is moved to an area of low average annual solar insolation, it is incorrect to inflate the materials requirements and risk from construction of energy storage devices and coal back-up facilities.

Although one can argue that locating a photovoltaic power plant in the cloudy Canadian climate is not sensible, this argument is not as persuasive as it is in the case of the solar-thermal-electric plant considered in Chapter 5. The difference is that the photovoltaic plant can use the diffuse radiation so important in cloudy climates, whereas, the solar-thermal-electric plant, by virtue of its focusing collectors, cannot. In our partial corrections, we do not remove the factor of 1.32 inflation of the photovoltaic risks that Inhaber added to account for the difference in insolation between Canada and the Southwestern U.S. Strictly speaking, this factor should apply only to those components of risk that are directly proportional to collector area. We assume, however, that essentially the entire materials requirement (and hence the entire materials acquisition, construction and operation and maintenance risks) of the photovoltaic plant are associated with its collectors. (This assumption would not be reasonable if these risks contained contributions from energy storage and back-up as was the case in Inhaber's analysis. Since in our partial corrections we assume the near-term, fuel-saver mode of operation wherein no storage or back-up is necessary, this assumption is more reasonable.) We emphasize that estimates of the risks of photovoltaic systems for the sunnier climates should not include this inflation factor which arises from Canadian conditions.

X. SUMMARY

In Appendix F of AECB-1119, Inhaber attempted to compute the risks of deploying solar photovoltaic cells in a central-station power plant of 1000 electrical-megawatts (average) capacity. Inhaber estimated the occupational risks of his photovoltaic power system to be between 142 and 188 worker-days lost per megawatt-year and the public health risks to be between 10.1 and 512 public person-days lost per megawatt-year. Our first-cut corrections of Inhaber's estimates of the health risks of photovoltaic deployment include the following changes:

(a) Occupational worker-days lost per megawatt-year due to materials acquisition is reduced by a factor of 14 as a result of substituting the materials requirements contained in Caputo (1977) for the incorrect data taken by Inhaber from the McReynolds memorandum (1976).

(b) Occupational worker-days lost per megawatt-year due to plant construction drops by a factor of 53 based on the use of Caputo's estimate for plant construction labor requirements in lieu of Inhaber's erroneous expansion of McReynolds' (incorrect) data.

(c) Public health risks due to emissions from coal burning for steel-making drop by a factor of 2.27 due to removal of the Area Correction Factor.

(d) Occupational worker-days lost due to operation and maintenance drops by a factor of 1.5 resulting primarily from elimination of the risky roofers from the operation and maintenance labor force.

(e) Occupational worker-days lost and public person-days lost due to energy back-up fall to zero since no back-up energy will be required by photovoltaic plants operated in the fuel-saver mode.

(f) Occupational risks due to energy storage fall to zero since no dedicated energy storage is necessary for near-term photovoltaic plants operated in the fuel-saver mode.

(g) Tonnage transported drops by a factor of almost seven, from Inhaber's estimate of 2590 metric tons per megawatt-year to 390 metric tons per megawatt-year, after elimination of Inhaber's back-up and storage systems and substitution of Caputo's materials requirements.

These changes do not include any attempt to rationalize the transportation risk by disaggregating types of material and and their typical transport modes. They also do not include any estimate of the public health risk of aluminum smelting and refining. Table 6.1 shows the results of our partial corrections.

TABLE 6.1

EFFECTS OF PARTIAL CORRECTIONS ON
AECB-1119'S ESTIMATED RISK OF PHOTOVOLTAIC SYSTEMS
(All figures in worker-days lost (WDL) or
public person-days lost (PDL) per megawatt-year.)

	AECB-1119 (REV-2)	Our partial corrections
Materials & Equipment, WDL	8.8	0.65*
Construction, WDL	94.8	1.78*
O&M, WDL	11.8	1.42 - 8.01*
Transport, WDL	10.2 - 34.2	1.1 - 3.9*
Storage, WDL	11.4 - 20.6	0
Back-up, WDL	4.5 - 18	0
TOTAL WDL	142 - 188	5.0 - 14
Emissions, PDL	0.73 - 2.19	0.32 - 0.96*
Transport, PDL	4.4	0.56 - 1.29*
Back-up, PDL	5.05 - 505	0
TOTAL PDL	10.2 - 512	0.88 - 2.25

* These partially corrected values include the inflation factor of 1.32 to account for Canadian climatic conditions.

CHAPTER 7: WIND

I. INTRODUCTION

Generating electricity from the wind is identified in the third edition of AECB-1119 as the most hazardous of all the renewable energy technologies in terms of total person-days lost per megawatt-year of output (AECB-1119/REV 2, Fig. 14). This extraordinary conclusion is mainly the result of three factors: the use of dirty coal "back-up" (unnecessary in the "fuel-saver" mode for which windmills will be used in the near future), accounting for about 63 percent of Inhaber's upper limit estimate of total risk of wind; an overstatement of about a factor of 50 in the materials requirements of windmills; and an overstatement of labor requirements for operation and maintenance by about a factor of 10.

After the third edition of AECB-1119 had been printed in November 1978, Inhaber finally grasped (at least partially) the nature of his materials error for windmills. He then produced a new typescript wind appendix for inclusion in possible subsequent editions of the report (none have yet appeared), in which he partly corrected the mistake. This change was also incorporated into the figure showing total risks in his February 1979 article for Science (Inhaber 1979a), although not into the figure in the same article showing the different contributors (materials, back-up, etc.) to the risk of the various technologies. No correction of the wind error was made in Inhaber's shorter summary as reprinted from New Scientist in the February 1979 IAEA Bulletin (Inhaber 1979f).

We give considerable attention to the materials error here, because it reveals so clearly (a) how little attention Inhaber paid to the physical meaning of the numbers he was using, (b) his persistent failure to grasp the nature of errors once their existence had been called to his attention by critics, and (c) his unwillingness to correct errors completely even after their nature must have been known to him.

II. MATERIALS

II.A. THE INITIAL ERROR

The crucial passage in materials requirements for windmills was essentially unaltered through all three published editions of AECB-1119. It reads (AECB-1119/REV-2, p. H-1):

> "How much material is required for wind electrical energy? Estimates in the literature vary, because there is some confusion between the capacity to generate energy produced in a given year in the future, and the cumulative capacity produced up to that year. One estimate states that the production of 250 megawatt-hours of electrical energy requires 400 short tons of steel, 10 of copper and 60 of fiberglass and plastics (99). Note that this is not rated capacity, but the total energy produced. Translating this to the usual base of 1000 megawatt-years net output, this is 12.6 million metric tons of steel, 0.32 million of copper, and 1.9 million of fiberglass and plastics. These are large quantities, but not unreasonable in the light of the requirements of other non-conventional systems. Results for windmills alone are shown in Table H-1."

The only change in the passage from the first edition was removal of an extraneous "are" after "Results" in the last sentence.

In the first edition, where we first found this remarkable passage, Inhaber assumed a windmill lifetime of 20 years and an annual load factor of 0.34. He did not specify a size for the windmill under consideration and we were unable at first to locate reference 99[(1)], so we assumed a windmill rated at 4 electrical megawatts (see, e.g., Carasso _et al_. 1975, Lockheed Aircraft Corporation 1976), and checked the materials requirements implied by Inhaber's

(1) The actual reference in AECB-1119 is: "Ref. 59, _op. cit_, p. 3938." Reference 59, in turn, reads: "Senate Committee on Small Business, 94th Congress, First Session, May 13-14, 1975, U.S. Government Printing Office, Washington, D.C., 1975." This turns out to be the widely-available _Final Solar Energy Task Force Report_ of the U.S. Federal Energy Administration's _Project Independence Blueprint_ (Federal Energy Administration 1974a), reprinted as an appendix to the hearings record. Because Inhaber uses the reprint's pagination, however, the reviewer of his work is forced to find the committee print.

numbers, as follows. The lifetime output of such a windmill in megawatt-hours (MWh), given the assumptions in the first edition of AECB-1119, would be:

$$4 \text{ MWe} \times 8760 \text{ hr/yr} \times 0.34 \times 20 \text{ yr} = 238{,}000 \text{ MWh}.$$

Use of the ratio of 400 tons of steel per 250 MWh of "total energy produced", as stated in the paragraph quoted above, implies the steel content of one such windmill would be:

$$238{,}000 \text{ MWh} \times (400 \text{ tons}/250 \text{ MWh}) = 381{,}000 \text{ tons}.$$

But the studies of 4-MWe windmills cited above estimated the steel content of one such windmill at 340,000 to 825,000 pounds. Inhaber's figure thus represented an inflation in the range of 1000- to 2000-fold. That he evidently noticed nothing wrong with a figure attributing to 4 megawatts of windmill capacity the weight of 1000 fully-loaded Boeing 747s is a telling commentary on the care Inhaber gave to checking his analysis.

How could such an enormous error have been made at all? An initial attempt at a charitable explanation might be that Inhaber meant to write "250 megawatt-hours of electrical energy per year". This mistake could only account for a factor of 20, however, not a factor of 1000 to 2000, and would seem to be ruled out by the emphasis in the next sentence on "total energy produced", where an author who had merely been victimized by a typo in the preceding sentence surely would have written "annual energy produced". The 1000- to 2000-fold error, moreover, is repeated in different units later in the paragraph, as "12.6 million metric tons of steel" for "1000 megawatt-years" net output. That is, letting "te" stand for metric ton, "t" for short ton, and "MWy" for megawatt-year, one gets Inhaber's second ratio from his first by straightforward conversion factors:

$$\frac{400 \text{ t}}{250 \text{ MWh}} \times \frac{8760 \text{ hr}}{\text{yr}} \times \frac{0.907 \text{ te}}{\text{t}} = \frac{12.7 \times 10^3 \text{ te}}{\text{MWy}} = \frac{12.7 \times 10^6 \text{ te}}{1000 \text{ MWy}}.$$

(Inhaber has 12.6×10^6 te in his text but 12.7×10^6 te in Table H-1.) We emphasize: the two ratios, 400 t/250 MWh and 12.6×10^6 te/1000 MWy, are separately spelled out on page H-1 in plain English, and they each represent

an error of a factor of 1000 to 2000 in the materials requirements.

This intermediate error did not persist unaltered through Inhaber's full calculation, however. Table H-1, although it contains the same numbers identified in the text (as quoted) as being for "1000 megawatt-years net output", is labeled "Materials Required for 1000 Megawatt Average Output Wind System", and the numbers are treated as such in subsequent calculations. The difference between requirements per 1000 megawatts average output and requirements per 1000 megawatt-years is a factor of 20, the assumed system lifetime in years. This factor of 20 reduces the 1000- to 2000-fold error Inhaber initially made to an error of 50- to 100-fold in the wind requirements as they entered subsequent calculations. Our initial conclusion was that he must have first confused pounds with tons to get the bigger mistake, and was then partly rescued by the fortuitous subsequent confusion between lifetime and annual output made in labeling Table H-1.

These errors persisted completely unaltered through the third edition of AECB-1119, even though the materials numbers were identified by early critics as being a factor of 50 to 100 too high. One would assume that, given such criticism, Inhaber would re-examine the section in question rather closely. If he did so, he evidently was unable to find anything amiss.

When we later discovered that the elusive reference 99 was actually the Project Independence Solar Task Force Report in disguise, we were able to unravel what Inhaber actually had done. It was in fact a somewhat more elaborate mistake than merely confusing pounds and tons. The Project Independence Report (Federal Energy Administration 1974a) indicates that 400 tons of steel builds 4 MWe of rated capacity (20 windmills of 200 kilowatts rated capacity each), which run at a load factor of 0.34 to deliver 12,000 megawatt-hours per year or 240,000 megawatt-hours in the 20-year lifetime assumed by Inhaber. But Inhaber found one combination of numbers, in the same table with all the correct ones, that apparently referred to a start-up phase in which many windmills were under construction but almost none were operating; that combination was 400 tons of steel used and 250 megawatt-hours produced in 1977. Thus Inhaber took a ratio, 400 tons/(250 MWh/yr), which was about 50 times too high to be the physical quantity he had in mind, and then dropped a "per year" to multiply the error by an additional factor of 20. Subsequently, in labeling

Table H-1, he mixed up annual and lifetime output once more and got his factor of 20 back, reducing the factor of 1000 error in his intermediate result for wind's materials requirements (given the figures in his source) to an error of a factor of 50 in the final tonnages used in his subsequent calculations.

II.B. THE WANDERING LOAD FACTOR

Although Inhaber did not change at all his estimates of the materials requirements for wind or the associated calculation of risk between the first and the third editions of AECB-1119, he did change the annual load factor assumed for his wind machines. This is significant because, although the load factor does not enter Inhaber's calculations explicitly beyond Table H-1 (which remains unchanged through all three editions), the load factor does tell how much rated capacity corresponds to a given average or annual or lifetime output.(2) A very low load factor means it takes a large amount of rated capacity to produce a modest average or annual output. Thus, choosing a low load factor would make high materials requirements per average 1000 megawatts (as in Table H-1) seem less unreasonable--the lower the load factor, the more windmills of any given size are needed to make 1000 average megawatts.

In the first two editions of AECB-1119, Inhaber had this to say about the load factor of wind machines (AECB-1119, p. H-1):

> "One of the problems in calculating windpower risk is determining the relationship between maximum power and average power. The second quantity divided by the first is the load factor. Decades ago, a load factor of about 0.02 was indicated for windmills of that time (97) [Putnam, 1948].
>
> "Improvements in wind systems in the three decades since Reference 97 was published have increased the load factor. A load factor of 0.34 is used in Reference 98 [Energy Research and Development Administration 1977b], and we shall use this value in subsequent calculations."

---

(2) The relations are: average output (megawatts) equals rated capacity (megawatts) times load factor; annual output (megawatt-years) equals rated capacity (megawatts) times load factor times one year; lifetime output equals rated capacity times load factor times lifetime (in years).

We are not sure where Inhaber found the very low load factor attributed to Putnam. In that reference, one finds (Putnam 1948, p. 182):

> "At Grandpa's Knob we realized about 1200 kilowatt-hours per kilowatt per year. In 1945 we estimated that a battery of six 1500-kilowatt wind-turbines on Lincoln Ridge would generate an average of 3500 kilowatt-hours per kilowatt per year."

The first figure, referring to the very large wind turbine Putnam actually built and operated on Grandpa's Knob in Vermont, is an annual load factor of 0.14. The second figure is an annual load factor of 0.40. Since Inhaber did not use the figure incorrectly attributed to Putnam but rather the figure of 0.34 referenced to ERDA (and also given, as we noted above, in the Project Independence report that was the source of Inhaber's materials numbers), this mistake is of little consequence except as another indication of Inhaber's apparent confusion.

More interesting is the change in the load factor that Inhaber made between the second and third editions. In the third edition, the first paragraph quoted above from page H-1 remains the same, but the second reads (AECB-1119/REV-2, p. H-1):

> "Improvements in wind systems in the three decades since Reference 97 was published have increased the load factor. A load factor of 0.06 is used in subsequent claculations [sic]. This value is based on experimental data compiled on a wind turbine in the Ottawa area (98) [unpublished data compiled by Maryl Weatherburn], and is in good agreement with the deduced load factor of Reference 99."

Reference 99 is the Project Independence study, cited as U.S. Senate Hearings. The capacity factor given there is 0.34, in good agreement not with Inhaber's new load factor of 0.06 (referenced to a personal communication) but with the initial value of 0.34 that has now disappeared without a trace. It is hard to imagine a reason for this remarkable change other than that Inhaber, unable to find the real source of the problems that had been called to his attention concerning his wind numbers, decided to reduce the size of the <u>apparent</u> discrepancy by deflating the ratio of average to rated output.

II.C. THE "CORRECTION"

When we unearthed reference 99 and discovered the actual nature of Inhaber's materials errors, we sent him a copy of our analysis of the point. He replied to the effect that the nature of the error had already been pointed out to him by a Professor Musgrove in England, and he enclosed a copy of the new typescript wind appendix we mentioned above. In this new appendix, one finds (Inhaber 1979g, p. H-1):

> "One estimate states that the production of 4 MW of rated capacity requires 400 short tons of steel, 60 tons of fiberglass and plastics, and 10 tons of copper (199). This does not include the concrete in the foundations. The sum of these weights is the equivalent of 109 metric tons per MW (rated). ... To transform this estimate into average output, the load factor must be known. It is assumed to be about 0.14 (see above). Then 1 MW (rated) produces 0.14 MW (average)."

Inhaber neglected to supply the references to go with his new appendix, but the materials numbers stated are those given in the former reference 99, the Project Independence report. But yet another load factor, 0.14, has appeared. The load-factor discussion in this new appendix reads (Inhaber 1979g, p. H-1):

> "It has proved difficult to obtain verified experimental values of load factors for working windmills. The largest commercial windmill ever built in North America, at Grandpa's Knob, had a load factor of about 0.14 (195). This value is comparable to that estimated elsewhere: 'A typical windpower generator operates at a load factor ... only one-fourth that of a typical fossil-fueled plant' (201). Since the load factor for coal and oil was taken as 0.70, one-fourth of that is 0.18, close to the above-mentioned value. On the other hand, there is some evidence that this value may be over-estimated. Winds at Grandpa's Knob exceeded 48 km/hr (30 miles/hr) over 70% of the time (97) [Putnam 1948], hardly typical of most locations. In a place (Ottawa, Canada) with perhaps a more average wind distribution, a load factor of 0.06 was found for an experimental windmill (98) [Maryl Weatherburn's data]. In any case, it is clear that the load factor will vary with location. Until more typical data is produced, the load factor will be assumed to be 0.14."

Inhaber has now managed to get right the load factor for the Grandpa's Knob windmill (perhaps with the help of Professor Musgrove), but he still doesn't tell the reader that Putnam's analysis indicated that load factors of 0.40 or more should be achievable. Nor is there any mention of the value of 0.34 from ERDA (and from the source of the now corrected materials requirements, Project Independence).

Why didn't Inhaber use the original value of 0.34, rather than continuing to conceal it? A plausible hypothesis suggests itself: Inhaber seems to want the technical community to believe his initial error was simply a confusion between lifetime and annual output, which would be a factor of 20. In responding in the May issue of Nuclear News to Holdren's elucidation of the initial wind error as a net factor of 50, Inhaber writes (Inhaber 1979b):

> "[The error] arose from a misunderstanding about these requirements on an annual and a lifetime basis, and produced an overestimation by a factor of 20, the approximate lifetime in years of the system. The error was of no other origin."

These assertions are false. As we have already stated and as comparison of the Project Independence report with any of the three editions of AECB-1119 will confirm, Inhaber's wind error arose from: (a) plucking from a table some ratios that were 50 times too high to be the ones he wanted (as comparison with the right ratios, presented in adjacent columns, would have revealed); (b) dropping from these ratios a "per year", which multiplied the initial 50-fold error by 20 on the assumption of a 20-year system lifetime; and (c) subsequently confusing annual and lifetime output a second time, canceling the earlier factor of 20. We emphasize: since the lifetime-vs.-annual output confusion occurs twice in opposite directions, it drops out and contributes nothing to the final net error, which is entirely the factor of 50 of different origin.

We suspect that, in order to be able to revise his materials figures by only a factor of 20 instead of the factor of 50 by which they were actually in error--and thus to be able to give the erroneous impression that it was "only" a lifetime-vs.-annual-output confusion all along--Inhaber simply adopted in his revised wind appendix a capacity factor of 0.14, about 2.5 times smaller than the one (0.34) he used when he first made the materials mistake in the

first edition of AECB-1119. Since Inhaber's final materials requirements were expressed per megawatt-year of lifetime output, choosing a capacity factor 2.5 times lower than before caused these requirements to fall by a factor of 20 even though the requirements per installed megawatt had fallen by 50.

II.D. REASONABLE SPECIFICATIONS FOR WIND MACHINES

What would be reasonable numbers for the load factor, materials requirements per rated megawatt, and the operating lifetime of commercial windmills? Nothing we have found in the literature since Putnam (1948) suggests that his projected load factor of 0.40 is unreasonable for large wind machines intended for routine power production in high-wind regimes. Experimenters will of course put smaller machines in locations that are not particularly windy but happen to be convenient, and will be fiddling with them rather than operating them a good portion of the time. For such machines, one can calculate very low load factors; these calculations tell one nothing about the performance that would be sought and (from all indications) attained for commercial power generators.

The recent major studies of electricity generation from wind confirm the reasonableness of load factors above 0.30 for sites likely to be chosen, often based on detailed wind surveys. We have already mentioned the value of 0.34 used in the Project Independence report (Federal Energy Administration 1974a) and the later ERDA wind document (ERDA 1977b) cited by Inhaber in his first edition. A recent Department of Energy review (U.S. Department of Energy 1979) indicates load factors of 0.38 for "High Wind Regimes" (mean wind speed above 15 mph [6.7 m/sec]), 0.33 for "Moderate Wind Regimes" (mean wind speed 12-15 mph [5.3-6.7 m/sec]), and 0.22 for "Low Wind Regimes" (mean wind speed 9-12 mph [4.0-5.3 m/sec]). Detailed studies conducted for the Swedish National Board for Energy Source Development yield values of 0.31 to 0.38 (Engstrom 1979). The Lockheed study of large wind turbines sited in windy regions (mean wind speed 6-10 m/sec) indicates load factors above 0.50 (Lockheed Aircraft Corporation 1976).

Concerning materials requirements per rated megawatt, the numbers Inhaber finally transcribed correctly (for his new wind appendix) from the Project Independence study are compared with some other values in the literature in

Table 7.1. We conclude that the Project Independence numbers arrived at by Inhaber subsequent to publication of the third edition of AECB-1119 are representative of the numbers used by recent major studies of large wind generators, except for Inhaber's concrete number (not from Project Independence), which appears to be about an order of magnitude low. Since the risk per ton of concrete is extremely low (or would be if Inhaber's calculations did not confuse cement with concrete as well as computing transport risk for unreasonable distances), the effect of the underestimate on the final risk figures is very small.

TABLE 7.1

WIND MATERIALS REQUIREMENTS

(Metric Tons per Rated Megawatt)

	Metals	Fiberglass & Plastics	Concrete
Bechtel (Carasso 1975)	38	0	--
Engstrom (1979)	69	3	350
Inhaber (1979)	93	14	16
Lockheed (1976)	98	0	97
Dept of Energy (1979)	118	10	320

As for system lifetime, which of course directly affects the materials requirements per megawatt-year of lifetime output, Inhaber's wind system is among those he assigns a lifetime of 20 years, in contrast to 30 years for fossil-fueled and nuclear systems and 50 years for hydropower. We see no justification for this assumption. As Engstrom (1979) points out, large wind turbines have relatively low rotational speeds and probably resemble hydro turbines more closely that they do any of the other technologies considered. Stresses in the blades are not higher than those routinely experienced in aircraft, many of which have operated safely for 30 years or more (one thinks particularly of DC-3's). If a blade should break, surely the most likely failure mode after long wear and tear, it could be replaced at a small fraction of the cost of replacing the whole plant. Engstrom also points out that conventional plants typically are dismantled not when they have worn out but sooner, when the efficiencies of new plants have become enough higher that the

fuel costs of running the old ones are excessive by comparison. This situation would not be relevant for wind machines, for which the operating "fuel" is free. We conclude that there is no reason not to use at least the same lifetime for wind plants as for conventional ones and that, in reality, the lifetime of the wind plants may prove to be even longer.

III. CONSTRUCTION

Concerning construction time, the third edition of AECB-1119 has this to say (AECB-1119/REV-2, p. H-2):

> "There have been a number of estimates for construction times. One of the most detailed is contained in Reference 99, which states that 5.6 man-years are required to construct capacity which can produce 250 megawatt-hours of energy. This equals 196 man-years to produce one megawatt-year of energy. Assuming a 20-year lifetime of the wind structures, this is 9.8 man-years per megawatt-year each year."

As in his treatment of materials, Inhaber appears to be profoundly confused here. What reference 99 actually says (Federal Energy Administration 1974a, Ch. IV, Table 1) is that it takes 28 worker-years to build 1 megawatt of rated wind capacity in 200-kilowatt units early in a program of wind energy development; 14 worker-years per rated megawatt in 1-megawatt units after the industry has increased in size; and 7 worker-years per rated megawatt in 1-megawatt units in a mature industry. At a load factor of 0.34 and a lifetime of 30 years, the intermediate figure of 14 worker-years per rated megawatt translates to about 0.73 worker-years per megawatt-year. Inhaber's claim of 196 worker-years per megawatt-year is 135 to 540 times higher than the values in the source he cited.

Inhaber evidently was suspicious of his figure of 196 worker-years per megawatt-year, so, even though that value has the correct units, he divided it by his assumed 20-year plant lifetime to get "9.8 man-years per megawatt-year <u>each year</u> (emphasis added). Then he dropped the "each year" (he had to, since it made the units nonsensical) before incorporating the number into his Table H-2 as "19,600 man-hours per megawatt-year", that is, 9.8 man-years per megawatt-year. (Note that 2000 worker-hours--fifty 40-hour weeks--equals one worker-year.) The net error, compared to his reference on the point, was then

an inflation of a factor of 6.75 to 27.

IV. OPERATION AND MAINTENANCE

The entire discussion of operation and maintenance (O&M) in AECB-1119's wind appendix consists of the following three sentences (AECB-1119/REV-2, p. H-2):

> "The number of maintenance workers for wind power will probably be higher than needed in the solar technologies. There is a requirement of 9 man-years per megawatt-year net output, in contrast to the 1.7 value for solar thermal (102). We can then multiply the risk values listed there by 9/1.7 = 5.3."

Reference 102 is to the Senate Committee on Small Business (Project Independence in disguise), p. 4010. Inhaber had added the explanatory note, "The data is for 1980." Actually, the data on page 4010 are for 1985. It is clear, however, that what Inhaber has done to obtain the figure of 9 man-years per megawatt-year is the following: he starts with a figure given in the reference as man-years worked by "other employees" (assumed to represent operation and maintenance), and clearly labeled as "cumulative to target year" and "from Jan. 1, 1975 to beginning of target year"; this cumulative labor from January 1, 1975 to January 1, 1980 he divides by the the energy produced in the single year 1980. Obviously, this procedure overstates the O&M labor requirement by an amount that is large but not determinable without knowing the number of windmills operating in each year between 1975 and 1980.

There is a very easy way, however, to see that the O&M labor assumed by the Project Independence study is in fact less than 1 worker-year per megawatt-year rather than the 9 worker-years stated by Inhaber. The study estimates (Federal Energy Admistration 1974a, Ch IV, Table 1) that the cost of O&M is 1 mill (0.1 cent) per kilowatt-hour. This amounts to $1 per megawatt-hour, or $8760 per megawatt-year, a sum hardly sufficient to pay for 1 worker-year of unskilled labor, not to mention parts and overhead.

Our conclusion based on this simple approach is in agreement with that of wind researcher R.J. Templin of Canada's National Research Council, who wrote (Rogers and Templin 1979b):

"These machines are in fact designed for automatic, unattended operation. An estimate of labor requirements is hard to make but they are likely at least a factor of 10 less than estimated by Inhaber."

Inhaber's at least 10-fold inflated O&M requirements, of course, are multiplied by the large risk per worker-year associated with the roofing and sheet metal industry, since he earlier assumed all of the solar-thermal-electric O&M labor (from whose risk the wind O&M risk is scaled) is in this category. It is difficult to imagine that, on average, windmill operation and maintenance will be as dangerous as roofing, especially since in many designs the part most likely to need repair--the generator--is on the ground.

V. STORAGE AND BACK-UP

Inhaber's discussion of "back-up" for wind energy reads as follows (reproduced here in full from AECB-1119, p. H-2):

"Wind power is weather-dependent, although it may be more or less so than solar power, according to its location. We will assume that the energy back-up which wind power will require is about the same as solar thermal or solar photovoltaic. We may then use the risk calculated for these two technologies (without using the factor of 1.32 which corrected for the southern location of the solar systems)."

Concerning storage for the wind system, he adds (AECB-1119, p. H-5):

"The remarks made about energy back-up apply [to energy storage] here as well. Using the same storage risk as for solar thermal and photovoltaic may be an underestimate. Reference 100 indicates that 33 hours of storage may be necessary, in contrast to the 16.5 hours we have been assuming. In the interests of consistency, we will continue to use the latter value."

As noted in Chapter 1, windmills in the near future almost certainly will be used as "fuel savers", generating electricity that displaces fossil fuels when the wind is blowing. For this application, no storage is necessary and no back-up is appropriate. The one approach to storage for wind energy in electricity grids that is presently being seriously considered for near-term

application is the use of windmills near existing hydroelectric facilities to pump water from below a dam to above it, making it possible to generate electricity by releasing the water through the dam's turbines at a later time. In this case the "storage" is an existing facility, and the incremental health risk of tying it to the wind system is the very small occupational one of some extra hours of turbine operation.

Of course, even if one undertook the rather silly task of trying to provide a wind generator with enough storage and back-up to make it behave like a baseload electricity generator, one would not use the heat storage whose purported risks Inhaber first calculated for the solar-thermal-electric system and then applied to his photovoltaic and wind systems. The wind system (like the photovoltaic one) has no way of converting heat back to electricity.

Finally, we note that Inhaber did not remove from his back-up and storage risks for wind the factor of 1.32 that "corrected for the southern location of the solar systems", contrary to his claim at the end of the first passage quoted above. Comparison of the risks attributed to storage and back-up in Table H-3 ("Windpower Risk") with those in Table E-5 ("Solar Thermal Electricity Risk") and Table F-4 ("Solar Photovoltaic Electricity Risk") shows that the numbers are identical.

VI. SUMMARY

Our first cut at correcting Inhaber's estimates in AECB-1119 for the health risks of wind systems makes the following changes:

(a) Occupational worker-days lost per megawatt-year due to materials and equipment acquisition drops 75-fold (50-fold for the error in using the wrong data from the Project Independence report and another multiplicative factor of 1.5 from assuming a 30-year lifetime instead of Inhaber's 20).

(b) Occupational worker-days lost per megawatt-year due to construction drops 13.5-fold, based on the intermediate value of construction labor from Project Independence, a 30-year lifetime, and a load factor of 0.34.

(c) Occupational worker-days lost per megawatt-year due to operation and maintenance drops 10-fold (as a conservative approximation).

(d) Public person-days lost per megawatt-year due to emissions from coal burning in steel acquisition drop by the same 75-fold as the steel requirement itself.

(e) Tonnage transported drops from 6500 metric tons per megawatt-year to 69.7 metric tons per megawatt-year, after eliminating back-up and storage and dividing the rest by 75. Following Inhaber's method, we applied this reduced tonnage to the range of transportation risks per ton of coal, given in AECB-1119's Appendix A.

These changes do not include: (a) reductions in occupational risk per worker-year realizable by more appropriate choices of occupational categories for the tasks actually performed; (b) reductions in the risk associated with coal for steel-making, due to use of more recent data relating to Black-lung incidence, mine safety, and due to higher productivity in ordinary coal mining than in anthracite mining; (c) any attempt to rationalize the estimate of transportation risk by disaggregating kinds of materials and transportation modes. Table 7.2 shows the results of our partial corrections.

Note that the largest remaining contributor to risk in the partially corrected estimates is operation and maintenance, the figure for which is probably still a substantial overestimate. If the O&M labor requirement were taken to be 0.5 worker-years per megawatt-year (probably more consistent with the Project Independence cost figure of $8760 per megawatt-year than the 0.9 worker-years we used) and if the risk per worker-year were taken to be that of the electric utilities as a whole rather than that of construction workers in roofing and sheet metal, the O&M worker-days lost would fall further from 6.2 to about 1.2 per megawatt-year. This change would drop the occupational risk estimate by about another factor of two, from 9.7 - 11 to 4.7 - 5.1 worker-days lost per megawatt-year.

TABLE 7.2

EFFECTS OF PARTIAL CORRECTIONS ON

AECB-1119'S ESTIMATED RISK OF WIND SYSTEMS

(All figures in worker-days lost (WDL) or

public person-days lost (PDL) per megawatt-year.)

	AECB-1119 (REV-2)	Revised Appendix H	Our partial corrections
Materials & Equipment, WDL	89	4.5	1.2
Construction, WDL	29	29	2.1
O&M, WDL	62	51	6.2
Transport, WDL	20 - 66	4.7- 16	0.2 - 0.6
Storage, WDL	11 - 21	11 - 21	0
Back-up, WDL	4.5-18	2.7 - 6	0
TOTAL WDL*	217- 286	104 - 129	9.7- 10.1
Emissions, PDL	8.5 - 26	0.5 - 1.3	0.11- 0.35
Transport, PDL	8.4	2.3 - 5.3	0.10- 0.23
Back-up, PDL	5 - 505	1.8-124	0
TOTAL PDL*	22 - 539	4.6 - 130	0.21- 0.58

* These totals may not add due to rounding.

CHAPTER 8:

OCEAN THERMAL ENERGY CONVERSION

I. INTRODUCTION

Inhaber's analysis of risk from ocean thermal energy conversion (OTEC) is presented in the same format as his other appendices treating unconventional technologies. Although his results indicate that OTEC presents the lowest risk from unconventional technologies, the analysis is badly flawed, resulting in a significant overestimate of the level of risk. As we show below, Inhaber miscalculates the materials requirements, uses a lower load factor than those assumed by comprehensive OTEC analyses, and seriously overestimates labor requirements for construction and operation and maintenance. Although a great deal of work has been done on system design for ocean thermal energy systems, Inhaber obtained his data from secondary, often inaccurate studies. Some of his errors, traceable to these secondary reports, could have been prevented simply by checking the detailed design features presented in the principal references. Correcting these errors and simple calculational blunders reduces the occupational risk from Inhaber's figures of 23.3 - 29.9 to a range of 2.2 - 4.6 worker-days lost and the public risk from Inhaber's figures of 0.8 - 1.4 to 0.4 - 0.9 person-days lost per megawatt-year net electrical output.

II. MATERIALS ACQUISITION

Inhaber's calculation of the materials requirements for an ocean thermal energy system contains a remarkable series of errors. In his analysis of the materials requirements for ocean thermal energy systems, Inhaber quotes from an article by W.G. Pollard in American Scientist (Pollard 1976). The Pollard article, a short summary of various solar energy systems, discusses an "authoritative" OTEC report published by Lockheed in 1975 (Lockheed 1975) and presents some of the materials requirements as calculated by Lockheed. The Lockheed report is one of the most complete OTEC studies done to date. Although Inhaber must be aware of the report (since it is Pollard's principal OTEC reference and Inhaber is quoting Pollard), it is apparent that he did not

consult it, for he does not catch a serious error made by Pollard.

Pollard cites the materials requirements for the Lockheed OTEC platform and for the "power system" but fails to note that the Lockheed design calls for four such power systems in each 160-electrical-megawatt plant. Pollard's power-system materials requirements are thus a factor of four too low. Because the power-system requirements are a small part of the total materials needed for the Lockheed design, the net result of this error alone would be to underestimate the total materials requirements by a modest amount.

Inhaber's next error occurs when he claims that the Lockheed design described by Pollard is a 160-MW "nominally rated" system. Pollard specifically (and correctly) states that the design is 160 MW electrical (Pollard 1976, p. 428). Inhaber, however, multiplies this value of 160 MWe by an assumed efficiency of 60 percent and an assumed load factor of 70 percent to come up with a "net electrical output" of 67.2 MWe. The correct size of the Lockheed design is 240 megawatts gross electrical output minus 80 megawatts of electricity consumed internally (operating pumps, support equipment, and other auxiliary systems) times a load factor of 90 percent, (160 MWe x 0.90) or 144 MWe net electrical output (Lockheed 1975, Vol. 1, pp. 2-49).(1) Thus, Inhaber claims to be describing the materials requirements for a 67.2 MWe system that is actually a 144 MWe system. This second error produces an overestimate of the materials requirements by a factor of 2.14 and is a result of misquoting his reference and failing to check the original source.

Inhaber then makes a third error when he incorrectly assumes that the materials requirements described by Pollard are given in short tons and proceeds to convert them to metric tons. Had Inhaber read the Lockheed report he would have realized that the materials requirements as presented by Pollard

(1) The comprehensive Lockheed report as well as numerous other OTEC studies (Federal Energy Administration 1974a; ERDA 1977c; Perry et al. 1978), specify a load factor of 0.9. It is interesting to note that Inhaber quotes two of these references in his OTEC analysis but apparently fails to note that they use a load factor significantly different from his own. Whether or not OTEC plants will be able to achieve such high load factors is uncertain at this time. As we shall see later, these same references offer detailed information on construction, and operation and maintenance requirements for OTEC systems as well, though these data are also ignored by Inhaber.

are already given in metric tons (Lockheed 1975, Volume 2, p. U-5).

The net result of these three blunders can be seen in Table 8.1, which presents Inhaber's incorrect values together with the correct Lockheed values. The materials requirements as calculated by the Mitre Corporation (Mitre 1979) are also presented in Table 8.1 for comparison. That this remarkable series of errors does not result in an enormous discrepancy between Inhaber's values and the Lockheed values is sheer luck--the biggest errors cancel each other out. Unfortunately Inhaber's luck does not hold when he attempts to calculate the risks associated with construction and operation and maintenance.

Inhaber's next step is to assign some of the materials to different industrial categories for fabrication. As he does for most of the other unconventional technologies, he assigns 100 percent of the metals requirements to the fabricated metal products sector. The result is that nearly 80 percent of the total materials risk comes from this category. Included in the metals requirements, however, are items specifically excluded from the fabricated metals category by the Standard Industrial Classification Manual, including turbines, generators, reinforcing steel, and basic steel products. Assigning the different components to the proper categories reduces the overall risk, since the fabricated metals sector has an extremely high level of risk associated with it.

Based on the Lockheed study, we estimate that, for the 160-megawatt system, about 18,110 metric tons of metals will undergo the type of fabrication done in the fabricated metal products sector. This is equivalent to 4.2 metric tons per megawatt year (assuming a load factor of 0.90 and a lifetime of 30 years). Almost all the rest of the metals requirement is cold-rolled steel for the hull, platform, and cold-water pipe (Lockheed 1975, Volume II, Appendix U). Such materials are direct outputs of the primary steel industry, so Inhaber had already fully counted the risks of their acquisition when he calculated the risks of the steel requirements. By requiring that all of the metal be handled by the fabricated metal products industry, he double-counts much of the metals risk. The size of this error is:

$$\frac{\text{Inhaber's input to the fabricated metals sector}}{\text{Corrected input to the fabricated metals sector}} =$$

TABLE 8.1

MATERIALS REQUIRED FOR A 1000-AVERAGE-MEGAWATT OTEC SYSTEM (10^5 Metric Tons)

	Aluminum[a]	Steel	Concrete	Copper	Ammonia
Inhaber	0.24	6.2	36	0.27[b]	0.32
Lockheed	0.50	5.43	18.54	----	0.17
Mitre	0.80	2.58	29.0	0.11	0.14

a The original design study called for titanium heat exchangers. Inhaber replaced this with an equivalent mass of aluminum.

b Inhaber claims to calculate his copper figure from the Project Independence summary (Inhaber's reference 59, FEA 1974a), though the correct figure from this report should be 0.018 x 10^5 metric tons (te) for a 1000-megawatt plant, not the 0.27 x 10^5 metric tons stated by Inhaber. The Project Independence study says that 1591 metric tons (1750 short tons) of copper will be required for a 1000-MWe system with a load factor of 0.90 (FEA 1974a, pp. VI-12/VI-14). This quantity of copper ignores the copper requirement for transmission lines. Inhaber does not consider transmission lines in this part of his analysis (AECB-1119, p. I-3). To convert these plant requirements to requirements per average megawatt, one divides by the load factor: 1591 metric tons for 1000 megawatts of capacity divided by 0.90 equals 1770 metric tons for 1000 average megawatts. How Inhaber arrived at his figure of 27,000 te (0.27 x 10^5 metric tons) is impossible to trace. He might have obtained his copper figure from the Project Independence study since he cites that study and an ERDA report (ERDA 1977c) for his ammonia and copper requirements, but the ERDA document does not discuss the copper requirements for an OTEC system. Yet as shown above, the calculation for the copper figure results in 1770 metric tons for a 1000-megawatt system, 13.3 times lower than Inhaber's value.

$$\frac{22.4 \text{ te/MW-year}}{4.2 \text{ te/MW-year}} = 5.33$$

The risk from fabricated metals thus drops from 2.43 WDL per megawatt-year to 0.46 WDL per megawatt-year. This reduces the total materials risk from 3.06 to 1.09 worker-days lost, without correcting any of Inhaber's other

overestimates in this category. Correcting the one material requirement significantly underestimated, aluminum, does not cause a large change in the level of risk from materials acquisition, adding only 0.013 worker-days lost per megawatt-year. This increase is more than compensated for if the risk from the materials overestimates for steel and copper is corrected.

III. CONSTRUCTION

In order to calculate the construction risk associated with these quantities of materials, Inhaber had to determine the time required to build an ocean-thermal system. Ignoring references that clearly describe the construction time for OTEC facilities of specified sizes, Inhaber chose a report that describes the construction time for a plant of unspecified size (ERDA 1977c).

Inhaber describes this plant as "probably a standard 60 megawatt net prototype" (AECB-1119, p. I-3) with a load factor of 0.70. Though Inhaber's reference does not specify the nameplate capacity of the plant, it does clearly specify a load factor of 0.90 (ERDA 1977c, p. 26). Inhaber nevertheless calculates a construction time from this report by assuming that it is "probably" a 60 MW net plant. If Inhaber had used either his own reference 59 or any other of the thorough OTEC analyses, he would have seen that his result was much too large.

The Project Independence study (Inhaber's reference 59: FEA 1974a), used earlier by Inhaber (incorrectly) to calculate the copper requirements for an OTEC system, clearly states that manpower requirements for construction of a 1000-MWe plant "assume a 3-year average construction period, with about a 20% increase over the 1 man/MW per year required for nuclear power plants" (Federal Energy Administration 1974a, p. VI-16). The report also specifies a load factor of 0.90 (Federal Energy Administration 1974a, p. VI-14), the same as the Lockheed report. The manpower requirements for construction are therefore 3 years x 1.2 workers/MW x 1000 MWe, or 3600 workers-years for a 1000-MWe plant. This figure can be verified by looking in Tables 1a and 1b on pages 4072 and 4077 of Inhaber's Reference 59. Assuming 2000 worker-hours per worker-year (the same as Inhaber), and a 30-year lifetime for the plant (the same as Inhaber), one arrives at a result of:

$$\frac{\text{3600 worker-yrs x 2000 worker-hrs/worker-yr}}{\text{1000 MWe x 0.90 x 30-year lifetime}} =$$

267 worker-hours per megawatt-year.

Doing the same calculation with data from another reference provides a verification of the order of magnitude of the construction time required for an OTEC plant. The Lockheed study (Lockheed 1975) estimated that the construction of a 160-MWe power plant would entail labor costs of $47.3 million at power-plant construction wages of $11.25 per hour (Lockheed 1975, Volume 2, pp. S-27, S-36, S-38). Thus a simple calculation gives an approximation of the total worker-hours required to construct the plant:

$$\frac{\text{\$47.3 million}}{\text{\$11.25 per worker-hour}} = \text{4.2 million worker-hrs.}$$

At 2000 worker-hours per worker-year this is 2100 worker-years for construction. Doing the same calculation done above, one can compute the worker-hours per MW-year required for construction. The Lockheed study assumed a load factor of 0.90. In addition, Lockheed assumed a lifetime of 35 years for the power system. In order to be consistent with the above calculations, however, we will assume a lifetime of 30 years.

$$\frac{\text{2100 worker-yrs x 2000 worker-hrs/worker-yr}}{\text{160 MWe x 0.90 x 30-year lifetime}} =$$

970 worker-hours per megawatt-year.

A comparison of these calculations with Inhaber's result is presented in Table 8.2.

Inhaber has overestimated the construction time required for an OTEC system by between 10 and 50 times. In order to show how badly Inhaber miscalculated the corresponding risk, it is necessary to run through the same calculations that Inhaber did, but with the correct numbers. Before doing so, however, we must repeat an important point. We do not mean to suggest that our calculations give "correct" answers for OTEC risk, nor that any calculation using Inhaber's method will give figures that are correct. Inhaber's method is flawed and incomplete, as described in Chapter 1 and throughout this report. We seek only to show how Inhaber's risk values would change if he had

TABLE 8.2

CONSTRUCTION REQUIREMENTS FOR OTEC PLANTS

(Worker-hours per megawatt-year net output)

AECB-1119:	9300 - 13300
Project Independence:	267
Lockheed :	970

correctly applied his own method to the available data.

The construction industries chosen by Inhaber as those most likely to participate in OTEC construction are shipbuilding and miscellaneous contracting (AECB-1119, p. I-3):

> "For simplicity, we will allot one-half [of the construction time] to each of ship-building and miscellaneous contracting."

Inhaber has chosen to ignore his reference 59, the Project Independence study, which specifies the closest industrial categories for the construction and operation of OTEC facilities (FEA 1974a, p. VI-15):

> "The industrial analogs regarded as closest are construction and operation of nuclear power plants and offshore oil rigs and tankers."

Inhaber's use of the wrong occupational categories for construction results in only a small overestimate of the risk, but the overestimate in construction time and the incorrect ratios of accidents, injuries, and illnesses in the occupations used produce a much bigger inflation.

Correcting the occupational statistics to the proper 1973 values (U.S. Department of Labor 1975), and using the more accurate construction times calculated above, we get the levels of risk shown in Tables 8.3 and 8.4. The total risk from construction of ocean thermal facilities is between 12 and 30 times smaller than the risk calculated by Inhaber.

TABLE 8.3

RISKS IN CONSTRUCTION INDUSTRIES

USED IN OTEC RISK ANALYSIS

(in Worker-days lost (WDL) per 100 Worker-years)[a]

	Accidents	Illnesses	Deaths[b]
Concrete work	95.5	2.7	192
Shipbuilding	165.7	5.07	101
Misc.Contracting	129.7	1.8	252

a (U.S. Department of Labor 1975)

b Assuming 6000 worker-days lost per death.

TABLE 8.4

TOTAL WORKER-DAYS LOST PER MEGAWATT-YEAR

IN CONSTRUCTION OF OTEC SYSTEMS

	Worker-hours	WDL Accidents	WDL Illnesses	WDL Deaths	Total WDL
Concr:	39.6	0.0189	0.00054	0.038	0.0575
Ship-bldg.:	135-500	0.112-0.414	0.003-0.013	0.068-0.253	0.184-0.680
Misc. Contr.:	135-500	0.088-0.32	0.0012-0.0045	0.17-0.63	0.259-0.959
TOTAL:					0.500-1.697

IV. EMISSIONS

Inhaber calculates the risk from emissions in the same manner as he does in the other appendices for unconventional sources, that is, by assuming that the risk is proportional to the weight of steel used. Since he miscalculated

the initial materials requirements, his emission calculations are off by a corresponding amount.

V. OPERATION AND MAINTENANCE

Inhaber seriously overestimates the manpower requirements for the operation and maintenance of an ocean thermal energy plant by making assumptions that are not warranted by the available data. Inhaber quotes the ERDA document that does not define the size of the plant (ERDA 1977c), and he assumes that the load factor of the plant is 0.70, whereas the report specifies a load factor of 0.90. Using these data, Inhaber arrives at a figure of 1520 to 1670 worker-hours per megawatt-year net output.

Several comprehensive reports detail the operation and maintenance requirements for ocean thermal plants, and they give results substantially different from Inhaber's. The Project Independence report, Inhaber's reference 59, clearly states that the operation and maintenance requirements are "thrice the three men/100 MWe required for nuclear power plants, and include shore-based support" (Federal Energy Administration 1974a, p. VI-16). Thus ninety men are required for a 1000-MWe plant with a lifetime of 30 years and a load factor of 0.9 (Federal Energy Administration 1974a, p. VI-14). The total O&M requirements are therefore:

$$\frac{\text{90 workers x 30 yrs x 2000 worker-hrs/worker-yr}}{\text{1000 MWe x 0.9 x 30-year lifetime}} =$$

200 worker-hours per megawatt-year net output.

The Lockheed OTEC study says that 35 persons are required for the operation and maintenance of their 160-MWe OTEC plant with a load factor of 0.9. Although they say that the expected lifetime of the power system is 35 years, we assume 30 years here to be conservative and consistent with the above calculation. The worker-hour requirements for this design are thus:

$$\frac{\text{35 workers x 30 years x 2000 worker-hrs/worker-yr}}{\text{160 MWe x 0.9 x 30-year lifetime}} =$$

486 worker-hours per megawatt-year net output.

Thus two different reports clearly indicate that the manpower requirements for operation and maintenance described by Inhaber are enormously inflated.

TABLE 8.5

OPERATION AND MAINTENANCE REQUIREMENTS FOR OTEC PLANTS

(Worker-hours per megawatt-year net output)

AECB-1119:	1520 - 1670
Project Independence:	200
Lockheed:	486

This gross overestimate of the operation and maintenance requirements for an OTEC plant is further compounded by Inhaber's assumptions concerning the type of workers needed to operate and maintain such a facility. Inhaber uses the Standard Industrial Classification 446, Water Transportation Services, as the category that would operate and maintain an ocean thermal energy system (AECB-1119, p. I-6). This category has the highest risk of any industrial occupation by a significant margin, and consists of dock maintenance, ship loading and unloading, stevedoring, canal operation, marina operations, and so on. It has nothing to do with the operation and maintenance of a floating power plant.

As noted above, the Project Independence Report specifically describes the closest industrial analogs for an ocean thermal energy system as being nuclear power plant operation, and offshore oil platform and tanker operation. The Standard Industrial Classification Manual has very clear categories for the operation and maintenance of nuclear power plants (SIC 4911), all oil rigs, including off-shore equipment (SIC 1381), and ship repair and maintenance (SIC 3731) (U.S. Office of Management and Budget 1972). If we assume that one-third of the time is spent in each of these three occupations, a rough estimate, we arrive at the average risk estimates shown in Table 8.6.

Thus the occupational risk from operation and maintenance is between 3 and 4 times smaller than Inhaber calculates, if the relevant occupational

TABLE 8.6

RISKS FROM OPERATION AND MAINTENANCE OF OTEC SYSTEMS

Category	SIC code	Worker-days lost per 100 employees *
Nuclear plants	4911	34.8
Offshore oil rigs	1381	207.4
Ship repair/maint.	3731	171.3
Average Risk: (Total/3)		137.8
AECB-1119 Risk (p. I-6)		506

* (U.S. Department of Labor 1975)

categories are chosen. Including Inhaber's tremendous overestimate of the time required for operation and maintenance, the total error in his risk estimate for this activity is an inflation of a factor of 12 to 24.

VI. ENERGY BACK-UP AND ENERGY STORAGE

Inhaber does not calculate any back-up or storage risks for his ocean thermal section, and none is appropriate.

VII. TRANSPORTATION

The risk from transportation is assumed to be proportional to the weight of the materials used in construction (AECB-1119, p. I-6). The fallacies associated with this assumption are addressed elsewhere. Correcting only the gross errors in Inhaber's materials calculations, however, reduces the total materials requirements from 444 metric tons per megawatt-year (AECB-1119, p. I-4) to approximately 150 metric tons per megawatt-year. Thus Inhaber's transportation risks are inflated by a factor of nearly 3.0.

VIII. SUMMARY

Even with Inhaber's overestimates of construction and operation and maintenance risks, the total risk level from OTEC was the lowest of the unconventional technologies. Inhaber believes his figure underestimates the total risk: "Whether or not this is due to serious underestimates of the materials required and the load factor will not be known until operating data is acquired" (AECB-1119, p. I-6). While no operating data exist for ocean thermal energy systems, the most careful and thorough studies done to date support the conclusion that Inhaber's estimates of materials requirements and construction and operation and maintenance times are grossly inflated. Correcting only the most significant blunders and faulty assumptions reduces the total occupational risk from a range of 23.3 - 29.9 to a range of 2.2 - 4.6 worker-days lost and the public risk from a range of 0.8 - 1.4 to 0.4 - 0.9 person-days lost per megawatt-year net electrical output.

Table 8.7 presents a summary of the major corrections to Inhaber's OTEC risk analysis and compares the corrected values with Inhaber's calculated values. The corrections should not be taken as the true risk values, since many of Inhaber's errors were not corrected, and in some instances, defects in his method were not completely eliminated. These figures do, however, provide a more accurate estimate of the level of risk from the categories listed.

TABLE 8.7

EFFECTS OF PARTIAL CORRECTIONS ON AECB-1119'S ESTIMATED RISK OF OCEAN THERMAL ENERGY CONVERSION (Occupational Worker-days lost (WDL) or Public Person-days lost (PDL) per megawatt-year)

	AECB-1119		Our partial corrections	
	Occup WDL	Public PDL	Occup WDL	Public PDL
Materials	3.1	----	1.09	----
Construc.	14.9-21.5	----	0.5-1.7	----
Emissions	----	0.26- 0.83	----	0.2-0.7
O.& M.	3.9	----	0.16-0.33	----
Back-Up	----	----	----	----
Storage	----	----	----	----
Transp.	1.4-4.5	0.58	0.46-1.51	0.20
Total	23.3-29.9*	0.84-1.41	2.21-4.63	0.40-0.90

* These are Inhaber's totals, not the correct sums of the numbers. Inhaber failed to add the materials risk to the sum at the high end of the range.

CHAPTER 9: SOLAR SPACE HEATING

I. INTRODUCTION

In Appendix G of AECB-1119, Inhaber analyzes the risks of solar space heating. In the introduction to this appendix he says (AECB-1119, p. G-1):

> "All the previous systems considered produced electricity as their final product, while the present one produces thermal energy. We shall assume, to make data comparable, that the thermal energy corresponds to the electrical energy that would have been required to heat a building. This implies that there are no losses in heating water by electricity. The actual losses are about 5% - 10%, so the risk in the following section is probably underestimated by this ratio."

Inhaber has this exactly backwards, of course--his assumption overestimates the solar technology's risk by this ratio, because it compares delivered thermal energy (which is the commodity one wants for space heating) to electricity at the power plant, before it has suffered transmission and distribution losses.(1) Nevertheless, he later cites this example as "an illustration of my general tendency or policy to give nonconventional energy systems the benefit of the doubt, in terms of risk, wherever possible."(2) (Inhaber 1979a, p. 718).

AECB-1119's analysis of the risks of solar space heating is confused,

(1) Inhaber is apparently referring to the fact that, after suffering transmission and distribution losses of about 10%, electricity could be used at about 100% (First-Law) efficiency to heat water or air with resistive heating, at which point it would be equivalent to the solar energy option. Actually, one could heat water or air with a heat pump to achieve a First-Law efficiency of 200 to 300 percent, but Inhaber clearly wasn't assuming this. If he had been, he would have asserted an "underestimate" of solar's risk by much more than 5 - 10 percent.

(2) Inhaber continues: "This policy was adopted to avoid any claims of inadvertent bias. Further examples include assigning lifetimes to nonconventional systems much longer than has been experimentally proved and assumptions of capacity (or load) factors probably higher than justified." His "further examples" are equally specious. (See Chapters 7 and 10.)

perhaps partly because Inhaber's stated intention was to deal only with energy production. The subject of solar space heating, however, is more closely related to end use of energy than to production. Solar space heating is one means of satisfying a very specific energy demand--the demand for a comfortable interior environment. Many researchers have concluded that "active" solar space heating systems using flat-plate collectors are essentially a last resort for meeting that demand. Although similar collector systems often make economic sense for pool heating and water heating, there are many cheaper methods of providing a comfortable interior environment. The first method always should be reduction of the building's energy losses through the use of weatherstripping, caulking, insulation, double-glazing or storm windows, and heat recuperators. These measures, especially when they are part of the building's initial construction, are much cheaper than providing heat from almost any source. Implementation of these demand-reducing measures to an economically sensible degree will result in a new house of ordinary size (1500-2000 square feet) having a heating requirement of 8,000-12,000 Btu per degree-day (Hutchins and Hirst 1978).

After all reasonable efforts have been made to reduce the building's energy losses, the next method is the incorporation into the structure of "passive solar" design features. Placement of windows mainly on the south side with properly designed overhangs, and incorporation of "thermal mass" (such as exposed rock or concrete) into the building's interior, make the building itself an efficient solar collector. Even if the building already exists, there are often possibilities for "passive solar retrofits" that allow these features to be added more cheaply than a flat-plate collector system. Implementation of sensible passive-design elements may be able to further reduce the ordinary house's annual heating requirement by 50-80 percent.

Once these steps have been taken, it is seldom possible to justify economically an active solar heating system with its expensive collectors, because the residual demand occurs mainly during extended cold spells when flat-plate collectors are working least efficiently. In order to meet this residual demand, active solar heating systems require large and expensive long-term storage systems. Since the residual demand is small and sporadic, most houses that have taken the two steps discussed above have chosen to meet that demand either with firewood, with a standard oil- or gas-fired furnace,

so its collectors produce an output of 225 kilowatt-hours per square meter per year (kWh/m^2/yr). (AECB-1119 has 223.) This figure for collector output corresponds to the performance of a mediocre single-glazed collector located in Toronto and coupled to a much smaller storage system than AECB-1119 provides. It is much too small to be a realistic output for a system with a storage tank as big as Inhaber assumes.

The size of the storage system affects the useful output of an array of flat-plate collectors, because it determines the degree to which solar energy can be collected when it is abundant and used when it is scarce. Normally, solar space-heating systems for single-family homes use storage systems designed to get the building through two or three fairly cold days with little sun. Since this is a situation that occurs relatively often, compared to, say, two weeks of continuously cold and cloudy weather, the storage capacity will be efficiently used, and hence economically sized. For a house and collector system of the size considered by AECB-1119, a storage system using 1,000 to 2,000 gallons of water (4 - 8 m^3) would fit this description. Increasing the size of the storage system decreases the frequency with which its capacity will be fully utilized. This makes the storage system less economical by itself. But increasing the storage size also increases the useful output of the collectors, because it allows energy that would otherwise be wasted (e.g., in the late summer and fall) to be used later on.

Inhaber provides a 20,000-gallon hot-water storage tank for his reference solar heating system. This is about ten times larger than two- to three-day storage systems, but several times smaller than "seasonal" heat-storage systems. It can hold enough thermal energy to heat its building for about two weeks of cold and cloudy weather. If well insulated, this volume would allow a significant amount of fairly long-term storage of energy, markedly increasing the useful output of the collectors. In addition, the storage and collector are so large that, if one actually did build such a system, one would almost certainly couple it to the domestic water heating system. This would further increase the useful output of the flat-plate collectors, because it would allow them to provide water heating (or preheating) even when the demand for space heating is small or non-existent.

Determining the exact extent of AECB-1119's underestimation of the useful

or with electric resistance heating, any one of which currently makes better economic sense than the oversized solar heating system that would be needed. Nevertheless, as we show below, Inhaber ignores all this, and calculates the risks associated with an oversized and inefficient active solar heating system for an ordinary house in Toronto.

II. MATERIALS ACQUISITION

To compute materials requirements for solar space heating, Inhaber finds a materials list for one kind of flat-plate collector and then scales it up on the basis of a design for a solar heating system in a report by the Ontario utility company. This report analyzes solar space heating for an 1800-square-foot house in Toronto with an annual heating requirement of 110 million Btu (Ontario Hydro 1975, p. 27).

Toronto has a cold climate in which reducing heat losses makes urgent sense, but this house is not particularly tight. It apparently requires about 16,000 Btu/degree-day,(3) which is a demand about 1.5 times higher than what tight construction could produce. This indicates that the Ontario Hydro design was not the kind of system one would realistically consider building. Not only is this solar heating system applied to a "leaky" house, it is also far larger than an economically sensible system. The Ontario Hydro report says (Ontario Hydro 1975, p. 5): "Recent estimates of the economical levels of solar effectivenesss (fractional energy use from solar) for building heating are 50-80% for S.W. United States and 25-50% for the northeast [sic] (similar to southern Ontario)." The system Inhaber considers, however, is designed to provide 70 percent of the building's heating requirement. It should not be surprising that the Ontario Hydro report finds this solar heating system extremely uneconomical--compared to the 1.6-cent-per-kilowatt-hour electricity it assumes as the alternative.

The hypothetical solar heating system AECB-1119 analyzes has a flat-plate collector system that is 60 percent of the floor area. It is therefore 1080 square feet, or 100 square meters. It delivers 70 percent of the building's heating requirement, which is 77 million Btu per year, or 22,560 kWh per year,

(3) 110×10^6 Btu per year divided by 6827 degree-days (F) per year in Toronto equals 16,000 Btu per degree-day.

output of the collectors requires a modeling process best done on a computer. Simple calculation procedures confirm that AECB-1119's figure of 225 kWh/m^2/yr is easily obtainable in Toronto with a single-glazed collector coupled to a small storage system (Weingart 1979b). The U.S. Office of Technology Assessment (1978, p. 301) indicates that a good flat-plate collector in Boston can deliver 538 kWh/m^2/yr. Boston is not quite as cold or cloudy as Toronto, and this figure may not include losses in storage, piping, etc. It does, however, give a rough idea of the output one could expect in Toronto. Dr. David W.O. Rogers of the Canadian National Research Council has estimated that a sensibly sized solar space heating system will deliver 448 kWh/m^2/yr in Toronto (Rogers 1979, p. 35). This is twice the figure used by AECB-1119, but Rogers' estimate assumes a double-glazed collector and a storage system 28 times smaller than Inhaber's--differences with opposite but indeterminable effects. Finally, data for a house in Toronto, using a solar heating system coupled to a very large storage system, indicate useful collector output more than twice that assumed in AECB-1119 (Higgins 1976, p. 215).

We conclude that Inhaber has understated the useful output per unit area of a good solar collector array by roughly a factor of two. By underestimating the efficiency of the collector system, AECB-1119 overestimates the materials requirements for producing a given amount of energy.

Inhaber, however, didn't give the collectors credit for even this output. Instead, he divided the materials requirements by 0.7 (the "load factor"), thus increasing the materials requirements by an additional factor of 1.43. This error is indicative of confusion about the concept of load factor. The load factor is used in the calculations of the risks of other systems to convert the capacities of those systems (e.g., 1000 MW) to the energy they actually deliver (e.g., 700 MWy/y for a 1000 MW system with a 0.7 load factor). In the case of solar space heating, the data Inhaber uses are already presented in terms of delivered energy, so his application of this factor of 1.43 is simply wrong. (Inhaber has acknowledged this error in his response to our draft _Science_ letter (Inhaber 1979c).)

The application of this spurious "load factor" on top of the underestimate of collector output produces an overestimate of about a factor of three (1.43 times 2 equals 2.8) for the materials requirements for the solar

collectors. Inhaber used these inflated materials requirements to calculate the occupational risks associated with materials acquisition and component fabrication. Most of the risk AECB-1119 ascribes to these stages of the solar heating system results from activity in the fabricated metal products industry (Standard Industrial Classification group 34). Inhaber's figures for worker-hours per metric ton of product and risk per worker-hour in this industry combine to produce high risk per ton of product. Since the fabricated metal products group includes establishments manufacturing items as different as beer cans, screws, and reactor pressure vessels, the average figures for the group are sometimes very different from the figures for its individual industries. This is the case for solar collector manufacturing (classified under SIC 3433), for which occupational risks are a factor of 1.37 lower than for the group as a whole (AECB-1119 reference 15: U.S. Department of Labor 1975, p. 26). The figure Inhaber used for labor requirements (149 worker-hours per metric ton of product) is also much too large. AECB-1119's reference 95 (Westinghouse Electric Corporation 1974, p. U-84) indicates that the fabrication of a solar collector requires 0.84 worker-hours. If we use just the steel, copper, and aluminum parts of Inhaber's collector (as Inhaber did) this is

$$\frac{0.84\ \text{worker-hr}}{\text{collector}} \times \frac{\text{collector}}{0.039\ \text{te}} = \frac{21.5\ \text{worker-hr}}{\text{te}}.$$

This indicates that the figure used in AECB-1119 is about a factor of 7 too high. Inhaber's use of figures for the fabricated metal products group thus produced an inflation of about a factor of 9.5 for the risks of manufacturing solar collectors (7 from overstating the labor requirements times 1.37 from using a riskier industry than appropriate). These numbers, in turn, were multiplied by the inflated metals requirements, producing an overstatement of the collector-fabrication risks per unit energy of about a factor of 27.

Despite all of these inflations of risks from materials acquisition for and manufacture of the solar collectors, AECB-1119 finds that the collectors are responsible for only about one-third of these risks for the whole solar heating system. The rest of the materials-acquisition and component-fabrication risks are associated with the 20,000-gallon steel storage tank Inhaber provides. Inhaber's source (the Ontario Hydro report that he used for

the tank's size and the system's performance) assumed this storage tank to be made of concrete rather than steel. Heat-storage tanks of this size are commonly made of concrete, and there are many companies in the business of building them.[4] But by arbitrarily changing the storage tank to steel, Inhaber more than triples the steel requirements for the whole solar space-heating system. The resulting steel requirement, all of which Inhaber assumes is treated by the fabricated metal products industry, is the source of 62 percent of the solar space-heating system's materials-acquisition risks. Inhaber is aware of this. He writes (AECB-1119, p. G-6):

> "The major component of risk in Table G-2 is the use of steel for storage. For example, the categories of fabricated metal products, steel, coal mining and iron ore mining constitute about 93% of the man-days lost by accidents in material and equipment acquisition.... It might be tempting to try eliminating this risk by substituting plastics for steel, or dispensing with liquids entirely by using rocks. However, this procedure would only substitute one type of risk for another, and it is not clear that the overall risk would be reduced."

On the contrary, it is perfectly clear that the overall risk would be enormously reduced. We have calculated the risks of a concrete storage tank, using a design requiring 4-inch concrete walls and one-half inch steel reinforcing bars every 12 inches horizontally and vertically. Using the same surface area as Inhaber's 20,000-gallon steel tank (121.5 m^2) gives a concrete requirement of 28.5 metric tons and a steel requirement of 2.0 metric tons. This steel requirement does not require treatment by any part of the fabricated metal products industry, because steel reinforcing bars are finished products of the basic steel industry (SIC 33). Assuming the same annual energy production for the system (achievable with half the collector area), and the 20-year system lifetime Inhaber used, gives 0.0515 megawatt-years delivered by the system during its lifetime. This in turn implies that the concrete requirement for the solar heating system is 553 metric tons per megawatt-year and the steel requirement is 42.8 metric tons per megawatt-year. Applying AECB-1119's risk-calculation procedure to these amounts of steel and

(4) See advertisements in magazines such as Solar Age, Solar Engineering, and the Solar Age Catalog.

concrete (including the implied requirements for coal, iron ore, non-metal mining, and cement) gives 1.0 worker-day lost per megawatt-year for the risks of materials acquisition for the storage tank. Inhaber's calculation of the materials-acquisition risks of the steel storage tank, by contrast, produced 16.6 worker-days lost per megawatt-year.

Inhaber departs from his usual assumptions about system lifetimes by assuming that solar space-heating systems will last only 20 years, instead of the 30 years assumed for most of the other technologies. This uniformly increases all the risks for solar space heating by a factor of 1.5. This assumption about lifetime is not necessarily incorrect. Current data are not adequate to predict the lifetimes of solar collectors, so most cost/benefit calculations have conservatively assumed a 20-year lifetime.

No "correct" risk figure can be computed for so ill-conceived a solar-heating system. But just removing the spurious load factor, changing the storage tank back to concrete (as it was in Inhaber's source), giving the solar collectors a more realistic efficiency (twice Inhaber's), and having the collectors fabricated under more realistic conditions reduces the occupational risk from materials acquisition and component manufacture by about a factor of about 12, from 26.8 person-days lost per megawatt-year to about 2.2.

III. CONSTRUCTION

Inhaber's next calculation concerns the risk of construction of solar space-heating systems. He writes (AECB-1119, p. G-2):

> "The construction times are generally unknown. One of the few estimates of the tasks used in construction is made in Reference 95, which states that half the time is employed in roofing, and the other half in plumbing. If we assume that plumbing uses only copper, and the value of 5.3 metric tons of copper derived from material acquisition, the number of construction man-hours used in plumbing is 5.3 x 1174 = 6220. Using the assumption of equal times in plumbing and roofing, the man-hours in the latter category also equals 6220. The weight of materials used in each trade has been adjusted to produce this equality."

This procedure contains three major errors. First, the copper requirement was part of the materials requirement for the solar collector, and it was already incorporated into the collector by the fabricated metal products industry, where it helped to produce the dominant portion of the materials-acquisition risk. Using the amount of copper in the tubing of the solar collectors as a measure of the amount of plumbing labor required is completely irrational. Second, the figure of 1174 worker-hours per metric ton for the plumbing sector of the construction industry is obviously incorrect. This figure implies that a plumber doing construction work installs less than two tons of materials per year, which in turn implies a labor charge of perhaps $15,000 per ton. This is a nonsensical result. The third error is the assignment of the clearly ridiculous plumbing worker-hours figure to the roofing labor requirement as well.

Inhaber had no need to use this roundabout method of generating construction labor requirements, because the very page he quoted to the effect that "half the time is used in roofing, and the other half in plumbing" actually provides a table giving a detailed breakdown of the installation labor requirements (Westinghouse 1975, p. U-25). It shows that it requires 20.2 worker-hours to install the collectors and 20.0 worker-hours to install the plumbing for a system consisting of 33 panels, each of which has an area of 2.11 square meters. This implies that each operation takes about 0.29 worker-hours per square meter. Assuming a collector output twice that of the Ontario Hydro report produces

$$\frac{2 \times 225 \text{ kWh}}{\text{m}^2\text{-yr}} \times \text{20-year lifetime} = \frac{9000 \text{ kWh}}{\text{m}^2}$$

or 9 MWh/m^2 over the collector lifetime. We can then calculate that the plumbing and the work similar to roofing each require:

$$\frac{0.29 \text{ worker-hr}}{\text{m}^2} \times \frac{\text{m}^2}{9 \text{ MWh}} \times \frac{8760 \text{ hr}}{\text{yr}} = \frac{282 \text{ worker-hr}}{\text{MWy}}.$$

Inhaber has 6220 for this value, a factor of 22 larger. Thus, even if we use Inhaber's assignment of occupational categories, using the figures for construction times plainly presented in his source would produce a risk from construction of about 1.02 worker-days lost per megawatt-year instead of 22.4.

To be complete, we should also include the risks from construction of the concrete storage tank. Adding the concrete and steel requirement gives 596 metric tons per megawatt-year. Multiplying this by AECB-1119's risks for concrete construction work gives 0.27 worker-days lost per megawatt-year.

IV. EMISSIONS

Under this heading, Inhaber computed a public-health risk from materials acquisition. His calculation is based on the system's steel requirement. As noted above, AECB-1119's steel requirement for solar space heating is greatly inflated by inclusion of a massive steel storage tank, and the steel requirement for the solar collectors is inflated by incorrect application of a "load factor" and by underestimation of the output they would produce. Even a partial correction of this section of Inhaber's risk calculation requires recomputing the steel requirement. We noted above that the reinforcing bars for the concrete storage tank would amount to about 42.8 metric tons per megawatt-year output. Adjusting the remaining steel requirement (29 percent of AECB-1119's total steel requirement of 185 metric tons per megawatt-year) by removing the spurious load factor and assuming a higher collector output gives 18.7 metric tons of steel per megawatt-year for the collector arrays. The total requirement is thus 61.5 metric tons per megawatt-year, which is about one-third Inhaber's figure. His "emissions" risk of 2.5 - 7.4 public person-days lost per megawatt-year is thus too large by about a factor of three.

V. OPERATION AND MAINTENANCE

Inhaber notes, correctly, that estimates of the operation and maintenance times for solar space-heating systems are rare. He generates an estimate based on his inflated values for solar-thermal-electric and photovoltaic systems, producing a value of 8.6 worker-hours per 100-square-meter "installation" per year. Solar space-heating systems, of course, have almost nothing in common with solar-thermal-electric and photovoltaic power plants, and Inhaber's estimate for operation and maintenance for solar-thermal-electric plants (which was just copied for the photovoltaic system) was 5 times too large for that system. Consequently, Inhaber's figure for the operation-and-maintenance labor requirement for solar space-heating systems is essentially

picked out of the air. We are unable to determine the size of the error produced, however, because data on this subject are simply too scarce. About one worker-day per year for operation and maintenance might be a plausible value for a home heating system, although it is probably toward the high end of plausibility, since it implies a labor charge of about $100 per year. What is certainly not reasonable is what Inhaber does with this invented figure.

First, he divides this construction labor requirement per megawatt-year by the much-too-low annual energy output of this "installation", effectively multiplying the worker-hours required by about two. He then proceeds as follows (AECB-1119, pp. G-6/G-7):

> "The estimate of maintenance time is probably low, for the following reasons. Because of the centralized nature of the two previous systems discussed, their maintenance would be regular and systematic, in contrast to that practiced by homeowners. As well, maintenance personnel would probably be trained to do the job. For these reasons, we multiply the time required by a factor of at least two, although this factor is still probably an underestimate.
>
> "A second point is homeowners are unlikely to be as skilled in maintenance as full-time workers at this task. Not only will it take them longer, but it will also be more dangerous for them. It is well known that the major location of disabling accidents is not the factory, but the home. We have conservatively multiplied risks by two to take account of this."

Neither of these factors of two is at all justified. The first asks us to believe that homeowners will, for some unstated reason, find it desirable or necessary to do all of the maintenance on their solar heating systems themselves. This, of course, would be in marked contrast to what they do with respect to all other kinds of heating systems. Considering the second factor, which means that the homeowners will be doing all of this work (more than 2 days a year) at risk levels more than twice that of normal coal mining, it seems most unlikely that they would choose to do it all themselves.(5) In

(5) Bituminous coal and lignite mining had injuries rates of 139.2 lost workdays per 100 full-time workers in 1972. Roofing and sheet-metal work (which is the industry Inhaber uses for this risk calculation) had 174.7. (U.S. Department of Labor 1975).

fact, there are simply no data on the risk level of maintaining solar-heating systems. There is, however, no particular reason to think that it would be much greater than that incurred in servicing oil or gas heating systems. While it may be plausible that part of the operation and maintenance work will resemble work in the roofing and sheet metal industry, it is clearly not plausible that all of it will. After all, the main things that break down are probably the pumps and control valves, and they are not on the roof.

Inhaber finds that the risk from operation and maintenance for solar space heating is about 36 worker-days lost per megawatt-year. This is probably high by at least the factor of 8 described above (2 from collector inefficiency times 2 from assuming homeowners work twice as long as skilled workers times 2 from assuming homeowners work twice as dangerously as roofers). With the scarcity of data available on this subject, however, one can have little confidence in any number given for this risk.

VI. ENERGY BACK-UP AND ENERGY STORAGE

Inhaber computes no risk for these stages, since he has already incorporated his energy storage system into the materials requirements in Section G-i. This system needs no energy back-up, he says, because it has a load factor of 0.70, the same as his other systems. Here Inhaber has done the right thing for the wrong reason. The fraction of the building's heating need met by the solar installation is conceptually a different thing than the load factor of an electric power plant. But it is true that no "back-up" risk should be assigned, because the solar-heating system is a fuel-saver in the same sense as wind and photovoltaics, not a replacement for baseload capacity.

VII. TRANSPORTATION

Inhaber's method for computing transportation risks is to add up the total weight of materials and then multiply by the risk per unit weight of shipping coal by rail. In fact, what he does in Appendix G is to divide the total materials weight per megawatt-year by his erroneous figure for the materials requirements for solar-thermal-electric plants, concluding that solar space-heating systems require 86 percent of the transportation of the solar-thermal-electric systems. He then computes the transportation risk by

multiplying the transportation risks for solar-thermal-electric systems by 0.86 and dividing by 1.32 (the factor by which he multiplied all of the solar thermal electric risks in moving the system to Canada). In doing so he has made still another arithmetic error--he drops the high end of the range for public accidents and miscalculates the low end, so that his figure for public person-days lost reads 2.1 rather than 2.3 - 5.4.

Recomputing the risks arising from transportation of materials and components requires a recomputation of the weights to be moved. Adjustment for the errors of the spurious load factor and collector inefficiency, and use of the materials requirements computed above for the concrete storage tank yields a total weight of ores, primary and intermediate materials, and finished products of 921 metric tons per megawatt-year. This is 59 percent of Inhaber's total of 1608 metric tons per megawatt-year. A simple correction of the transportation risk figure would then be 59 percent of what Inhaber should have obtained with his procedure. Of course, this procedure still contains the additional uncorrected error (described in Chapter 1, Section III.C.3) of assuming that the materials for concrete (553 out of our total of 921 metric tons per megawatt-year) are shipped the same 300 miles by rail as U.S.-average coal.

VIII. OTHER CONSIDERATIONS

Since Inhaber has already placed his solar space-heating system in Canada, he does not multiply its risks by the 1.32 "correction factor" he used in the consideration of solar-thermal-electric and photovoltaic plants.

IX. SUMMARY

Table 9.1 shows the results of our partial corrections of Inhaber's calculations of the risks of solar space heating. We have removed the spurious load factor, which increased all of the risks from materials acquisition, emissions, and construction by a factor of 1.43. We have also assumed that good flat-plate collectors will deliver about twice the yearly energy output per unit area assumed by Inhaber. The risk of materials acquisition and component manufacture has been further adjusted to more accurately represent the risks and labor intensity of fabricating solar collectors, in accordance with

the data presented in Inhaber's sources. It has also been adjusted to represent the risks of acquiring the materials for a concrete storage tank, also indicated in Inhaber's source. Emissions and transportation risks have been recalculated using AECB-1119's procedures on the resulting mix of materials. Construction risks include the risks of construction of the concrete storage tank, calculated according to Inhaber's procedures, as well as the risks of construction plainly implied by the figures in Inhaber's source on construction-labor requirements. Operation and maintenance risks are simply Inhaber's invented figures, with his completely arbitrary and completely unjustified multiplication factors removed.

TABLE 9.1

EFFECTS OF PARTIAL CORRECTIONS ON AECB-1119'S ESTIMATED RISK OF SOLAR SPACE HEATING

(All figures in occupational worker-days lost (WDL) or public person-days lost (PDL) per megawatt-year output.)

	AECB-1119		Partial Corrections	
	Occup WDL	Public PDL	Occup WDL	Public PDL
Materials acquisition	26.8		2.2	
Emissions		2.5-7.4		0.8-2.4
Transportation	4.9-16.2	2.1[a]	2.8-9.3	1.3-3.1
Construction	22.4		1.3	
Operation & Maint.	35.7		4.5	
TOTAL	91-101[b]	4.6- 9.5	10.8-17.3	2.1-5.5

a As noted above, correct application of Inhaber's procedure would have given 2.3 - 5.4 for this item.

b These are AECB-1119's totals, not the correct sums of the numbers.

Inhaber's figures are wrong by about a factor of 6 for occupational risks, and by about a factor of 2.3 for the public risks. Of course, the "corrected" figures still misrepresent the risks associated with solar space heating because they are still derived from analysis of an oversized active solar space-heating system in Toronto, which is hardly representative of economically realistic systems for using the sun's energy for heating. To be more enlightening, the analysis should have considered systems that are more likely to be installed. Three examples of such systems are solar water heaters, pool heaters, and passive solar buildings. The location of such examples should also have been chosen to offer a realistic range of possibilities, rather than simply choosing one particularly unfavorable location and basing all the analysis on it. At the very least, the reader should be told that the analysis is based on something close to a worst-case example, rather than being misled by claims that the system is "representative" and that the analysis gave the solar heating system "the benefit of the doubt".

CHAPTER 10: METHANOL

I. INTRODUCTION

AECB-1119's Appendix J begins with a confused introduction to methanol synthesis, a "representative" energy-supply technology based on biomass. Many energy-supply systems based on biomass resources are possible; methanol is one of the several options available for supplying liquid fuels. Virtually all liquid fuels today are derived from petroleum feedstocks (grain alcohol makes only a very minor contribution to the world's energy supply). Petroleum products supply 45% of the energy used in the U.S. and the world, and perform many crucial functions (e.g. automotive transportation) that require a portable, liquid fuel. In order to make meaningful comparisons, the biomass-methanol option should be compared to other technologies that supply liquid fuels, such as petroleum refining or coal liquefaction.

The relevant part of the oil fuel-cycle to which the production of methanol can be compared is "gathering and handling fuels" (AECB-1119, Table B-2). For comparison, the methanol fuel cycle need be followed only to the stage of fuel production, the assumption being that the gasoline and methanol will be comparable in terms of risk thereafter. This assumption would impose a bias in favor of gasoline because: (1) gasoline is the dirtier fuel during combustion; (2) methanol has a higher octane rating than gasoline, and is thus a higher-quality fuel.

Instead of using this approach, however, AECB-1119's Appendix J compares the biomass-methanol option with technologies that produce <u>electricity</u>. This conceptual confusion and the ramifications of it make an important contribution to Inhaber's exaggeration of the risks of methanol.

II. MATERIALS ACQUISITION AND CONSTRUCTION

Under this heading, Inhaber purports to calculate the risk due to materials acquisition for and construction of a methanol factory. What he has in fact done in this section is an entirely different matter. His first step was

to construct an inflationary "correction" factor for comparing a methanol factory with an oil refinery by making a remarkable series of assumptions, all of which are incorrect. Then he proceeded to multiply this "correction" factor by a set of data that refers to the health effects of operation and maintenance of an oil refinery. Thus the risk computed for materials acquisition and construction for methanol in AECB-1119 is simply the operation and maintenance risk of an oil refinery multiplied by a spurious correction factor.

Section J-i begins by explaining (p. J-1):

"In this part, we will be concerned only with acquiring materials for and the construction of the methanol factory.

"No estimates could be found for the weight, type of materials required, and construction times to build a methanol factory. However, an oil refinery is a close analogy to this type of factory."

To complete the analogy, a physical "correction" factor (scaling factor) is employed. The scaling factor is a product of three terms with which Inhaber tries to make a physical comparison between an oil refinery and a methanol factory. It is assumed each facility is sized to produce the same fuel-energy output. The three multiplicative factors are the energy density ratio of the two fuels (2.0), the plant lifetime ratio (1.5), and the end-use efficiency ratio (3.0).

To get the first factor, Inhaber assumes that because methanol has only about half the "fuel value per unit weight of ordinary fuel oil" (p. J-2) (it has 46% of the fuel value of gasoline in terms of energy per unit weight), a methanol factory would have to be twice as big as an oil refinery producing the same fuel-energy output. This assumption reflects the author's apparent ignorance of the technologies used in the petrochemical industry. The size and complexity of the equipment and facilities needed to produce a given product are determined both by the physical properties of all of the various substances that are encountered during the process, and by the rate of material throughput in the plant. The energy density of a fuel product is one of the many properties and parameters that affect the size of a fuel-production facility[1]. Crude-oil refining is far more complex than methanol synthesis.

(1) Even Inhaber's choice of process variable is suspect. In the processing of liquid fuels, most engineers are concerned with the rate of material throughput in a plant, and thus they present data

It requires many more operations (especially if the refinery is geared to maximal output of gasoline), and there are many more product streams in a refinery. Inhaber selected one process variable (energy density of the product) that suggests a methanol factory would be bigger than an oil refinery. We have discussed several factors that suggest a methanol factory would be smaller. Both systems are large and expensive, and it would be incorrect to conclude that one is larger than the other on the basis of only a single process variable, when in fact the two procedures hardly resemble each other at all. Methanol synthesis is an established technology, with annual sales of 400-million dollars in 1977 (Chemical and Engineering News 1978). It is unclear why Inhaber did not use published data for methanol factories directly (there is an abundant literature on this subject), instead of relying on a flawed analogy with oil refineries.

The second term of the correction factor is based on the assumption that a methanol factory would have a physical lifetime only two-thirds as long as an oil refinery. In the body of AECB-1119, Inhaber lays out the following ground-rules (p. 17):

> "It is implicitly assumed they [non-conventional systems] will be built with the same durability as systems with which we are more familiar."
>
> "[L]ifetime is an important value. For most of the systems considered in this paper, a lifetime of 30 years was assumed. Exceptions are noted in the appendices."

Presumably, lifetimes different than 30 years will be used only in those cases that present real physical reasons for doing so. In Appendix J, Inhaber assumes that methanol factories will have physical lifetimes of only 20 years. The basis for this assumption is AECB-1119's reference 117, in which a twenty-year "lifetime" was selected as an accounting tool for determining the economic feasibility of a proposed project (Intergroup Consulting Economists 1976, vol. 2, p. I-15):

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for energy density as energy per unit volume, rather than per unit weight. By this definition, methanol has 52% of the fuel value of gasoline.

"This report assumes that appropriate financing and tax structures would exist to ensure that a 15 per cent DCF [discounted cash flow] return over 20 years on total capital (including working capital) will permit a reasonable return on equity."

At no point in the document is a physical plant lifetime of only 20 years discussed. Perhaps Inhaber does not understand the difference between an accounting lifetime and a physical lifetime. In any case, methanol factories are at least as durable as oil refineries, and the same 30-year lifetime should have been used for both.

The third term of the correction factor in Section J-1 results from one of Inhaber's conceptual confusions. He assumes that methanol will be used as a transportation fuel in automobiles, and that mechanical energy derived from methanol in an automobile can be considered equivalent to electricity as available from a power plant (AECB-1119, p. J-2):

"[W]e are assuming that the methanol used is equivalent in terms of mechanical energy to the electricity that could have been used to drive autos and buses."

This contention is not only indefensible, but it flies in the face of an equally silly claim in the body of AECB-1119, which would seem to require the author to consider the fuel energy in the methanol equivalent to an equal amount of electricity (AECB-1119, p. 2):

"[W]e are not concerned here with end uses, but energy production. As a result, all units of energy produced are deemed equivalent."

The author appears to be thoroughly confused about the physics and technology of energy conversion. Different forms of energy can be interconverted, but the conversion efficiency, which is dependent on the initial and final energy forms and on the conversion technology employed, can vary over a wide range of values. For example, in converting the chemical energy stored in methanol into mechanical energy, the activity considered in Appendix J, the conversion can be accomplished with an efficiency of about 12% in an automobile, 40% in a large steam-turbine plant, and up to 60% using a fuel cell coupled to an electric motor. Each of these activities is non-comparable--the costs are

different, and the applications for which the mechanical energy produced will be used are different. Thus each activity must be considered separately.

The suggestion that electricity can be considered equivalent to mechanical energy in cars is simply wrong. It requires that electric cars operate at an efficiency (mechanical energy at the wheels divided by electricity at the power plant) of 100%, whereas 50% is a more reasonable assumption (Hottel and Howard 1971). In addition, current-technology electric cars are not the equals of cars with internal-combustion engines. They cannot match their speed, acceleration, or operating range. Electricity, in other words, cannot presently deliver the same transportation benefit as methanol. If and when electric cars are improved to the point that they can deliver transportation service comparable to that of contemporary cars, biomass could then be used to provide transportation energy in the form of electricity, either through direct combustion or gasification followed by combined-cycle power generation. In either case, the overall conversion efficiency (biomass to mechanical energy) would be significantly higher than in the case of the wood-to-methanol option considered by Inhaber. Biomass can be converted into electricity with the same efficiency as coal--say 36 percent. The efficiency of converting biomass into mechanical energy in a car via methanol is much lower--only 6 percent (biomass-to-methanol at 50%, methanol-to-mechanical energy at 12%). Thus there is a six-fold difference between the conversion-pathway efficiencies of converting biomass into mechanical energy via methanol and converting biomass into electricity.

Since the choice of energy products that will be produced from biomass will be based in part on the end uses to be served, a more sensible analysis of methanol would have been to compare the risks of obtaining a megawatt-year of energy as liquid fuel via the petroleum-gasoline pathway versus via the wood-methanol pathway. The best way to compare the methanol and oil appendices in AECB-1119 is to follow each fuel cycle only to the stage of prepared motor fuel, thus comparing two liquid fuels of equal chemical energy content. To convert AECB-1119's risk data for oil-generated electricity to the same base (i.e. one megawatt-year of chemical energy in liquid-fuel form), first the transportation and electricity-generation contributions to oil's risk should be subtracted, then the remaining risk per electrical megawatt-year should be multiplied by the efficiency of electricity generation (0.36).

The value of 3.0 as the third multiplicative term of the "correction" factor in section J-i results from the ratio of the efficiency of converting oil to electricity (considered in AECB-1119's Appendix B) to the efficiency of converting methanol to mechanical energy in an automobile: 0.36 / 0.12 = 3.0. The derivation of this factor in Revision 2 is obscured by a typo (see Chapter 1, Section III.G.2 of this report) that was introduced while correcting a mistake that appeared in the first two editions of AECB-1119. In those editions, Inhaber treats risk figures per megawatt-year of electricity generated from oil as if they were per megawatt-year of chemical energy in oil, thus making an error of a factor of 2.78 (the reciprocal of the oil-to-electricity conversion efficiency of 0.36)

Combining the three correction factors in section J-i yields a net inflation factor of nine (2.0 x 1.5 x 3.0). A methanol factory producing the same energy output as an oil refinery, according to Inhaber, will be nine times bigger and more costly. Since each of these correction factors is inappropriate, the data in section J-i are inflated by a factor of nine.

After constructing this inflation factor, Inhaber applies it to a set of risks he alleges are for materials acquisition and construction of an oil refinery. The data come from Comar and Sagan, and are clearly labeled as the health effects "associated with operation" (Comar and Sagan 1976, pp. 588-589) of the energy facilities considered. At no point in the article do Comar and Sagan suggest that their data include the risks associated with materials acquisition and construction of energy facilities. It is difficult to believe that Inhaber did not know that he was making this error, because in section J-iii, where a different data set for the risk due to operation and maintenance of oil refineries was used, he states "another estimate (23) gives an upper limit ... an order of magnitude greater." AECB-1119's reference 23 is to Comar and Sagan, so at this point Inhaber evidently understood what the Comar and Sagan data refer to.

In short, none of the data in AECB-1119, section J-i have any relationship to the risk from materials acquisition and construction in building a methanol factory. The real risk associated with these activities was not calculated in AECB-1119, and cannot be calculated based on the data given in the report.

Inhaber closes section J-i with a short paragraph that suggests yet another misunderstanding of his reference on methanol. Inhaber states (AECB-1119, p. J-2):

> "[I]t is of interest to note that methanol production may have a net energy loss (119). That is, more energy is expended on the process than is gained from the resulting product. Taken to its logical conclusion, this implies that the risk is infinite."

AECB-1119's reference 119 refers to a discussion of an electrolysis-hybrid method of methanol production that could potentially utilize Canada's considerable hydropower resources (Intergroup Consulting Economists 1976, vol. 1, p. 59):

> "It must be appreciated that the Electrolysis Hybrid effectively uses two major 'feedstocks' (wood and electricity) to produce methanol. For every ton of methanol produced (yielding about 18.4 MM Btu's), 1.32 ODT [oven-dry tons] of wood (equal to about 23.8 MM Btu's) and 3,740 kwh (equal to about 12.8 MM Btu's after generation) of inputs are required. The process has an overall net energy loss, and therefore can be justified only on the basis of the relative economic values for wood, electricity and methanol."

The footnote explains that about 36.6 MM Btu's of energy would be needed if the feedstock electricity for the process were to be produced by thermal-power generation. We note first that, during any conversion from one energy form to another, there will be a "net loss" of energy. Inhaber's point here is that more process energy (i.e. non-feedstock energy) would be expended in making methanol than would be recovered in the final product. Inhaber is both misleading his readers and misrepresenting his reference. He misleads his readers by implying that all methanol production entails this "net energy loss," whereas in fact standard methanol production does not. He misrepresents his reference both by implying that the hybrid system described is the object of the report (it is only mentioned as one possible alternative to the standard production system), and by obscuring the fact that, as stated in the quote, this unlikely alternative would only be feasible if the value of methanol was much greater than that of electricity. In order for the methanol production system to display the infinite risk that Inhaber predicts, one has

to imagine the hybrid system of methanol production using electricity made (in part) from the produced methanol. This would be the height of folly, and obviously economically infeasible. Indeed, even using wood to generate this electricity would be absurd--only a very cheap source of electricity, especially one (such as nuclear) that could not be converted directly into a liquid fuel, would be appropriate.

III. EMISSIONS

In section J-ii, Inhaber calculates the public risk resulting from routine emissions during the methanol-fuel cycle. Routine emissions constitute an insignificant part of the total risk due to methanol, but are discussed here for completeness. Emissions-related risks are divided into two parts; one resulting from materials acquisition for the methanol factory, the other due to fuel combustion in cars.

The risk associated with materials acquisition is calculated by multiplying the inflation factor of nine (discussed above) by the steel requirements for an oil refinery from Smith *et al*. (1975). (According to AECB-1119's stated method, this should have been done in the previous section as the first step in calculating the risk due to materials acquisition and construction). Coal requirements for steel production are calculated in the study's usual way, and the emissions risks are based on emissions of sulfur compounds during the combustion of the coal. These data are inflated by a factor of nine due to the methanol inflation factor (see previous section), and an additional factor due to the exaggeration of coal's risk (see Chapters 1 and 2 of this report).

Inhaber did not have any data for methanol combustion in cars, so he assumes that the risk is zero, warning his readers that the data "may have to be revised upwards." The same is true of gasoline combustion in cars, which was also not considered in AECB-1119.

IV. OPERATION AND MAINTENANCE

The occupational-health risks associated with routine operation and maintenance for the production of methanol were calculated for two major activities in the methanol-fuel cycle: wood procurement and fuel synthesis.

Before the risks from either of these activities could be determined, their labor requirements were needed. Inhaber's methanol reference (Intergroup Consulting Economists 1976) gives O&M-labor requirements for wood procurement and methanol-production operations to support a 100-million-imperial-gallon-per-year plant. This plant's gross annual-energy output is 246 megawatt-years (this is Inhaber's number, and lies within the range of the lower and higher heating values for methanol). To determine the net output, writes Inhaber (AECB-1119, p. J-3), "Only the efficiency of the methanol-to-end use plays a part... The net energy is then 246 x 0.12 = 29.5 megawatt-years." Thus an unwarranted inflation by a factor of 8.33 (1/0.12) is applied to all of the risk data that depend on the size of the methanol factory.

Inhaber takes O&M-labor requirements for the methanol factory from his reference (Intergroup Consulting Economists 1976). He scales them for an operation that produces one MW-yr of mechanical energy annually, using his net output assumption. He compares this manpower level with that for the oil refinery considered in Appendix B, and forms a ratio. The ratio is 6.9 using Inhaber's methanol-plant net output, so it should be 2.3 (the inflation factor in this instance is only three, because the oil-to-electricity conversion efficiency was included in the data for oil). This ratio is then multiplied by the data in Smith et al. (1975) for occupational-health effects in oil-refineries.

Inhaber warns his readers that the methanol factory O&M risks calculated in this manner may be low for three reasons (AECB-1119, p. J-3):

> "First, risk from disease has been set to zero for lack of data. Second, since methanol is produced from wood and wood chips, handling these materials may be riskier than handling oil products. Third, another estimate (23) gives an upper limit on the total number of man-days lost per unit time in oil refineries which is an order of magnitude greater."

The first two points are misleading, and the third is simply bizarre. First, the risk from disease was also set equal to zero for oil in both Smith et al. (1975) and Comar and Sagan (1976). For symmetry, the same should be done in the case of methanol. Second, the risks associated with the handling of wood and wood chips are already (and properly) included with the logging risk in Appendix J. Inhaber should have known this because the table that he used to

determine the labor requirements for logging clearly includes all of the activities that are required for producing the wood, chipping it, and supplying it to the gasifiers (Intergroup Consulting Economists 1976, vol. 2, pp. II-95). Third, as already noted, Comar and Sagan's operation and maintenance data were included in section J-i, where they were misrepresented as relating to materials acquisition and construction, and inflated by a factor of nine.

Contrary to Inhaber's claim that the risk calculated for methanol-plant operation and maintenance may be low, it is high, even after correcting for the end-use inflation factor of three. This results from a misapplication of occupational-health risk statistics. Inhaber simply applies data on the risks of oil-refinery operations to a methanol factory. Instead, he should have used risk data from the correct industrial sector. Methanol production is classified under the 1967 Standard Industrial Classification (SIC) number 2818 (industrial organic chemicals n.e.c.). (In 1972, it was changed to SIC 2869, same title, but the Bureau of Labor Statistics (BLS) continues to use the 1967 classification numbers in its 1977 publication.) The data for total person-days lost due to injury and illness for this sector are lower by 32 percent than for SIC 291, petroleum refining, according to BLS statistics for 1974 (Bureau of Labor Statistics 1978). Thus the methanol factory operation and maintenance hazards in AECB-1119 are inflated by a factor of about 4.4 (3.0 x 1/0.68).

Inhaber's operation and maintenance logging-manpower requirements are inflated by a factor of 8.33 because of his mistake in determining the net-energy output of the plant. The occupational risks due to logging account for about two-thirds of the total risk for methanol in AECB-1119, so this is the crucial set of data in Appendix J. It has been inflated even beyond the factors already mentioned above. Inhaber's source gives a detailed breakdown of the labor requirements for wood procurement. The tasks involved include wood harvesting, chipping (50% on site, 50% at the factory), transportation, resource estimation, support services, and road building (including the building of primary roads as well as secondary-haul roads, since the enterprise examined in the report involves logging the Canadian forest north of existing operations, where there are no existing primary roads). Inhaber categorizes all of these jobs under SIC 241 (logging camps and logging contractors). In terms of total occupational risk level, this industrial category ranks second

of all the three-and-four-digit SIC industries according to BLS statistics (Bureau of Labor Statistics 1978). It is the wrong category! SIC 2421 (saw mills and planing mills, general) is the correct category for most of the worker categories in the wood procurement operation. SIC 2421 includes chipper mills and wood chips manufacturing. According to the 1972 SIC manual, "Logging camps combined with sawmills, when not separately reported, are included in this industry." (U.S. Office of Management and Budget, Statistical Policy Division 1972). According to a safety officer at Crown Zellerbach Corporation (paper manufacturing is currently the major end use for wood chips), the entire chipping operation, including logging, is reported to the BLS under SIC 2421 (Larson 1979). The labor requirements for the wood-procurement operation in Inhaber's reference can be classified as follows: 68% in SIC 2421, 19% in SIC 08 (forestry), and 12% in SIC 161 (highway and street construction). This would reduce the total risk associated with "logging" by 53% based on the 1974 BLS statistics used by Inhaber. Thus the occupational risk due to logging is inflated by a factor of 2.13 (1/0.47) because of the use of inappropriate data.

Even after correcting for Inhaber's inflation factors (8.33 x 2.13 = 17.74), the risk data for logging are still too high. Inhaber's source states: "In summary, current harvesting operations by pulp and paper mills and by sawmills are not directly applicable to the methanol operation." (Intergroup Consulting Economists 1976, vol. 2, pp. II-70). It goes on to explain that the methods of wood procurement most applicable to a biomass energy operation will be more highly automated than in present practice, and less geared to producing a high-quality product. The large North American paper-manufacturing companies are increasingly adopting more highly automated equipment. They achieve a health and safety record that is significantly better than that obtained by the industry as a whole. It is the performance of the latter that is reported by the BLS (Bureau of Labor Statistics 1978).

The National Safety Council (NSC) also collects occupational-risk data for the various SIC category industries (National Safety Council 1978). Their sampling group is based on subscribing members, and includes nearly all of the big companies, but few of the small "mom-and-pop" operations. Their statistics may reflect more closely the occupational risk level that can be expected in a wood-procurement operation dedicated to biomass energy production. The

NSC occupational-risk figures for SIC 2421 are significantly lower than those of the BLS. The NSC data indicate that for subscribing members, the health and safety record in terms of total worker-days lost due to accidents and illnesses for SIC 241 and SIC 2421 are nearly the same, at about one-third the level reported by the BLS for SIC 241. Accounting for the improvement in safety records due to the use of automated-logging equipment, Inhaber's data are inflated by a factor of three when compared to the result obtained by applying the NSC data to each of the job classifications listed in his reference for the wood procurement operation. Thus, there is a net inflation of at least twenty-five (8.33 x 3) for the logging operation and maintenance risk associated with the production of a "unit energy" of methanol in Appendix J.

Neither the BLS nor the NSC include death statistics disaggregated by industry in their publications. This presumably reflects their judgment that the sampling group is too small for statistical accuracy. Since the method used in AECB-1119 requires that deaths and worker-days lost be added together (at the rate of 6000 worker-days lost per death), Inhaber had to compose a statistic for the occupational death rate in the logging industry. This was accomplished by an analogy with the roofing and sheet metal industry, "another outdoor trade." Table 2 in AECB-1119/REV-2 reveals that the ratio of deaths to total worker-days lost for the roofing and sheet-metal industry is based on the death rate reported by the BLS for the parent industrial sector, contract construction. (See AECB-1119, Table 2, footnote (b)). In effect, then, Inhaber is basing his ratio of deaths to total worker-days lost in logging on the ratio of the death rate in contract construction to the injury and illness rate in the roofing and sheet metal industry. The death rate in contract construction is 2.9 times greater than the death rate for manufacturing, the industrial sector to which logging belongs. Deaths account for 66% of the total logging risk and 44% of the total risk for methanol production in Appendix J. Thus the single greatest factor contributing to the risk of methanol in AECB-1119 is based not on hard data, but on a false analogy. Lumber and wood products (SIC 24) account for only a small fraction of total manufacturing activity, so it is not obvious that the death rate for manufacturing is any more applicable to SIC 2421 than the death rate for contract construction. The exact inflation of operation and maintenance risk associated with Inhaber's treatment of logging cannot be determined from the data in AECB-1119. It was surely at least twenty-five, and possibly as high as 75 or more.

V. ENERGY BACK-UP AND ENERGY STORAGE

Neither back-up nor storage were charged to methanol in AECB-1119.

VI. TRANSPORTATION

Transportation risk is a minor contributor to the total risk for methanol in AECB-1119. It's computation is based on a faulty analogy multiplied by an unwarranted inflation factor.

Methanol transportation risk is compared to the risk data for oil transportation in Smith _et al_. (1975). These data refer to the risk for transporting residual fuel oil from a refinery to a power plant. The methanol appendix discusses a fuel-supply system for automobiles, so transportation risk should be compared to the risk encountered in the present gasoline-supply system. This would imply a different mode of transportation (trucks) than the pipelines that were analyzed in Smith _et al_. (1975), and a more decentralized distribution infrastructure.

The transportation risk data taken from Smith _et al_. (1975) are inflated by "the methanol-to-oil conversion factors deduced above" (AECB-1119, p. J-4). This statement apparently refers to the inflation factor of nine constructed in section J-i. This factor includes the "lifetime" ratio for an oil refinery and a methanol factory, the "end-use efficiency" ratio (electrical vs. mechanical), and the energy-density ratio. The first two obviously don't apply here. The third factor--energy content per unit weight of fuel--would influence transportation risk per unit of energy if the fuel were moved by truck, but Inhaber's derivation of risk figures by multiplying pipeline transport risk figures by two is certainly not correct.

In short, none of the data in section J-vi have any relationship to the risk due to the methanol-supply-transportation system they purport to represent.

VII. APPENDIX J's SUMMARY

Appendix J draws to a close with a calculation of the materials required to build a methanol factory. The unwarranted inflation factor of nine appears again (see section i), and data from Appendix B (Oil) are misused. Table B-1 shows materials required for various facilities in the oil-fuel cycle. The categories are: gathering and handling fuel, transportation, and electricity production. These data are taken directly from Smith et al. (1975). In the reference, Inhaber's "gathering-and-handling fuel" category is disaggregated into two categories, harvesting fuels (based on off-shore oil extraction) and upgrading fuels. Fuel harvesting accounts for 67% of the steel and 38% of the concrete used in all of the activities in the oil-fuel cycle, except for electricity production. It is unclear why Inhaber aggregated the first two categories from Smith et al. (1975) in Appendix B. It is interesting that in section J-ii (emissions), the data for steel from Appendix B were correctly disaggregated when doing the same calculation. The calculation that is done in the summary (materials for the methanol-fuel cycle) is not included in the total risk data in Table J-1, but it does appear in the text of AECB-1119/REV-2, in Table 4 and Figure 6. Steel is inflated by twenty-seven (9.0 x 1/0.33 = 27), and concrete by fifteen (9.0 x 1/0.62 = 14.5) in this calculation.

VIII. SUMMARY

In short, Appendix J is so full of errors and misapplications of the methodology that it would be impossible to fix it, short of simply starting anew. Two-thirds of the total risk for methanol is associated with logging. The logging-risk data are inflated by a factor of 25 to 75. Most of the remaining methanol risk is attributed by Inhaber to material acquisition and construction, but really refers to the risk of operation and maintenance in an oil refinery, inflated by a factor of nine. In section J-iii (operation and maintenance), the risks due to operation and maintenance are counted again, although from a different source and with a different inflation factor. The rest of the sections contribute little to the total risk for methanol, but are just as full of blunders. Only two corrections to the data--removing the fallacious numbers for materials acquisition and construction, and reducing the risk associated with logging by a factor of 50--would reduce Inhaber's total

risk due to methanol from his range of 222 - 348 total person-days lost (AECB-1119/REV-2, Table J-1), to a range of 9 - 10 total person-days lost.[2] The mean value of the total risk due to methanol is reduced by 97 percent. (See Table 10.1).

Again, the reader should be cautioned that neither we nor Inhaber have calculated the "correct" value of risk for methanol, since (a) the method described in AECB-1119 was not applied systematically to this technology; (b) the method itself is insufficient for this purpose (see the first chapter in this report).

There is little of value in AECB-1119, Appendix J (all editions). The system chosen for analysis has a much higher risk than would any system that converted biomass wastes into secondary energy forms (67% of the risk in Appendix J comes from logging). Yet simple economics shows that waste residues like municipal solids would be much cheaper to convert (the opportunity cost of collecting municipal-solid waste for energy production is near zero in many large cities).

Inhaber claims that he is using only technologies that already exist. Yet no commercial-scale facility for making methanol from wood is even planned. Dried and shredded garbage have been burned (co-fired with coal) in commercial-scale boilers, and a great deal of data are available on this technology. It would seem to be an excellent candidate for a report like AECB-1119, which is mainly concerned with electricity generation.

Unfortunately, no part of Appendix J is worth saving or correcting. Public policy regarding biomass-energy utilization can be better formulated with an understanding of the real risks involved, but AECB-1119 cannot be used to determine those risks.

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(2) The reason for the narrowing of the uncertainty range as well as the reduction of the mean is two-fold: (a) nearly all of the uncertainty for total person-days lost in Table J-1 comes from the materials acquisition and construction category; (b) the uncertainty range for this category (5.2 - 121) is exaggerated due to a transpositional error on page J-2, where the data for total deaths for oil processing are given as $0.057 - 1.43 \times 10^{-3}$, but should be $0.57 - 1.43 \times 10^{-3}$ (Comar and Sagan 1976). Correction of this error changes the calculation for total person-days lost in "materials acquisition and construction" to 33 - 121, and the total person-days lost for methanol to 250 - 348 from 222 - 348.

TABLE 10.1

EFFECTS OF PARTIAL CORRECTIONS ON AECB-1119'S

ESTIMATED RISK OF METHANOL PRODUCTION

(All figures in occupational worker-days lost (WDL) or public person-days lost (PDL) per megawatt-year.)[a]

	AECB-1119		"Corrected"	
	Occup WDL	Public PDL	Occup WDL	Public PDL
Materials Acquisition & Construction	5.2-121	--	0[b]	--
Emissions	--	0.047-0.14	--	0.0052-0.015[c]
Operation and Maintenance	221	--	6[d]	--
Transportation	3.1-5.6	--	0.34-0.62[c]	--
TOTAL	222-348[e]	0.047-0.14	6.3-6.6	0.0052-0.015

a These data are for total person-days lost only, counting deaths as equal to 6000 PDL. The risks of accidents and disease have been aggregated from AECB-1119, but occupational and public exposures have been tabulated separately. The "corrected values" (last two columns in the table) are based only on correcting the arithmetic errors and misrepresentations in AECB-1119's Appendix J, and in no way represent the correct values of risk for methanol production from biomass.

b AECB-1119 does not offer any data for this stage of the methanol fuel cycle. Obviously, a complete analysis would require such data.

c Removing the inflation factor of nine.

d Assuming a mean value of 50 for the logging inflation, and 4.4 for the methanol-factory inflation.

e These are Inhaber's totals, not the correct sums of the numbers.

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Persons interested in pursuing the history of this matter in detail may wish to request the contents of AECB file 34-9-1-0 from the Atomic Energy Control Board of Canada, P.O. Box 1046, Ottawa, Canada K1P 5S9.

About the Authors

John P. Holdren is Professor of Energy and Resources at the University of California, Berkeley, Participating Guest in the Energy and Environment Division of the Lawrence Berkeley Laboratory, and Faculty Consultant in the Magnetic Fusion Energy Division of the Lawrence Livermore Laboratory. He holds B.S. and M.S. degrees from M.I.T. (engineering) and the Ph.D. from Stanford University (engineering and plasma physics). He is a member of the Steering Committee of the U.S. National Academy of Sciences Study of Nuclear and Alternative Energy Systems and a member of the Academy's Committee on Literature Survey of Risks Associated with Nuclear Power.

Kent Anderson is a Ph.D. candidate and the Energy Information Specialist for the Energy and Resources Group, University of California, Berkeley. He holds a B.A. in history from Pomona College and the M.S. in energy and resources from U.C. Berkeley.

Peter H. Gleick is a Ph.D. candidate in the Energy and Resources Group, U.C. Berkeley, and a Research Assistant in the Energy and Environment Division, Lawrence Berkeley Laboratory. His B.S. (engineering and applied science) is from Yale University.

Irving Mintzer is a Ph.D. candidate and Assistant Research Specialist in the Energy and Resources Group, U.C. Berkeley. He holds a B.A. (growth and human development) and a Master of Business Administration degree from U.C. Berkeley.

Gregory Morris is a Ph.D. candidate and Assistant Research Specialist in the Energy and Resources Group, U.C. Berkeley. He holds a B.A. (natural science) from the University of Pennsylvania and an M.S. (biochemistry) from the University of Toronto.

Kirk R. Smith is Research Associate and Chairman of the Energy Project, Resource Systems Institute, East-West Center, Honolulu. He holds B.A. (physics), Master of Public Health, and Ph.D. (environmental health sciences) degrees from the University of California, Berkeley. He has been a consultant

to the American Physical Society and Interagency Review Group studies of nuclear waste management and is President-elect of the Hawaiian Chapter of the Health Physics Society.

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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

In the matter of CAROLINA POWER & LIGHT CO. Et al.)
Shearon Harris Nuclear Power Plant, Units 1 and 2)

Dockets 50-400
and 50-401 O.L.

CERTIFICATEOF SERVICE

I hereby certify that copies of 2.758 petition and affidavits and supporting documents **, and of "Contention 15AA" re capacity factor HAVE been served this 30 day of June 1983, by deposit in the US Mail, first-class postage prepaid, upon all parties whose names are listed below, except those whose names are marked with an asterisk, for whom service was accomplished by ________

** The extensive documents ERG-79-3 and "Side Effects of Renewable Energy Sources" are served herewith on Judge Kelley, Applicants, Staff, and 3x to NRC Docketing & Service; available on request to all other parties.

Judges James Kelley, Glenn Bright and James Carpenter (1 copy each)
Atomic Safety and Licensing Board
US Nuclear Regulatory Commission
Washington DC 20555

George F. Trowbridge (attorney for Applicants)
Shaw, Pittman, Potts & Trowbridge
1800 M St. NW
Washington, DC 20036

Ruthanne G. Miller
ASLB Panel
USNRC Washington DC 20555

Office of the Executive Legal Director
Attn Dockets 50-400/401 O.L.
USNRC
Washington DC 20555

Phyllis Lotchin, Ph.D.
108 Bridle Run
Chapel Hill NC 27514

Docketing and Service Section 3x (includes original)
Attn Dockets 50-400/401 O.L.
Office of the Secretary
USNRC
Washington DC 20555

Dan Read
CHANGE/FLP
Box 524
Chapel Hill NC 27514

John Runkle
CCNC
307 Granville Rd
Chapel Hill Nc 27514

Karen E. Long
Box 991
Raleigh NC 27602

Bradley W. Jones
USNRC Region II
101 Marietta St.
Atlanta GA 30303

Travis Payne
Edelstein & Payne
Box 12607
Raleigh NC 27605

Richard Wilson, M.D.
729 Hunter St.
Apex NC 27502

Certified by Wells Eddleman