#802

Conservation Law Foundation
Docket No. 5330
Exhibit CLF-PLC-R1

STATE OF VERMONT
PUBLIC SERVICE BOARD

SURREBUTTAL TESTIMONY OF

PAUL CHERNICK
PLC, Inc.

ON BEHALF OF THE

CONSERVATION LAW FOUNDATION OF NEW ENGLAND
VERMONT PUBLIC INTEREST RESEARCH GROUP
VERMONT NATURAL RESOURCE COUNCIL

GRAND COUNCIL OF THE CREES (OF QUEBEC)
NEW ENGLAND COALITION FOR ENERGY EFFICIENCY AND THE ENVIRONMENT

February 6, 1990

TABLE OF CONTENTS

1. INTRODUCTION

Q: Mr. Chernick, please state your name, occupation and business address.

A: My name is Paul L. Chernick. I am President of PLC, Inc., 18 Tremont Street, Suite 703, Boston, Massachusetts.

Q: Are you the same Paul Chernick who presented direct testimony in this proceeding?

A: Yes.

Q: What is the purpose of this surrebuttal testimony?

A: The purposes of this testimony include responding to:

- the rebuttal testimony of Mr. Boucher, on the conflict between DSM and the HQ contract, and on the role of DSM in utility planning;
- the rebuttal testimony of Dr. Johnston, on DSM potential and risk, and on externalities; and
- the direct testimony of the DPS, on externalities, conservation potential, risk, and least-cost planning.

2. THE POTENTIAL FOR DSM/HQ CONFLICTS

Q: Does Mr. Boucher's testimony change your position on the conflict between the HQ contract and cost-effective DSM?

A: No. Mr. Boucher does not dispute my computation of Participant energy requirements, and of the very small amount of those requirements which the HQ purchase would allow DSM to displace.[1] Instead, he repeats the Participants' consistent error of concentrating on peak load; he directs the Board to an exhibit (GMP-TCB-7) which compares GMP's capacity requirements to its capacity resources. Mr. Mallory's Exhibit RM-4 provides a similar comparison of the Participants' projection of the <u>capacity</u> need for Vermont as a whole. Neither of these analyses addresses Vermont's electric <u>energy</u> supply situation.

None of my comments indicated that Vermont's <u>capacity</u> requirements would be filled by the HQ contract. My testimony clearly established that Vermont's <u>energy</u> requirements would be largely filled with fixed-cost resources (nuclear, hydro, and purchases). Mr. Boucher does not make any attempt to respond to my analysis.

The Participants have all the current data on their power supply commitments, including the energy production of existing

---

[1]Since Mr. Boucher repeatedly confuses energy and capacity, as the Participants have done throughout this case, I should emphasize here that the conflict is between HQ and <u>conservation</u>. Load shifting, which is generally much less socially valuable than conservation, is less threatened by the purchase.

and planned hydro facilities and the amount and terms of small power and QF projects under contract and in negotiation. In response to my testimony, they could have updated and corrected any of my assumptions which were obsolete or incorrect. Their failure to do so leaves unchallenged my analysis of Vermont's energy supply glut from the mid-1990s to the retirement of Vermont Yankee.[2] Since energy is generally much more expensive than capacity -- peaking capacity runs about $100/kW-yr, while the HQ contract will cost roughly $420/kW-yr for Schedule C-1 -- decisions in energy supply are generally more important than those relating to total capacity needs.

Q: Is it surprising to find that the HQ purchase and other baseload power supplies will substantially fill Vermont's energy needs for the next 10-20 years, but not all of Vermont's capacity needs?

A: No. The HQ purchase would provide much more energy than capacity. Each MW of the HQ purchase would provide about 6.6 GWH at a 75% capacity factor. At the 63% load factor ESRG reports for 1988, that 6.6 GWH of load would correspond to 1.19

---

[2]The DPS runs indicate that the amount of baseload generation without the HQ purchase may be even higher than I assumed. FOr example, file OHQBGHF1.SPM indicates 1999 generation of 0.6 TWH from Vermont and NYPA hydro, 1.2 TWH from small power, 2.1 TWH from nuclear, and 0.1 TWH from the CU HQ contract, for a total of 4.1 TWH. Another 0.5 TWH of energy is supplied by existing units costing less than 3.6 cents/kWh (1990$): Stonybrook on gas, McNeil on wood, and Wyman. In addition, the runs with the minimum HQ purchase appear to back out small portions of most of the existing baseload supplies, suggesting that the marginal energy cost following the HQ commitment would be near zero in many hours of the year.

MW of winter peak load. Adding 20% reserves produces a total capacity requirement of 1.43 MW to support the 6.6 GWH. Thus, every MW of HQ purchase requires at least another 0.43 MW of other capacity for reserves and peaking.[3] The 450 MW HQ purchase would thus require roughly another 240 MW of capacity, in order to serve a typical slice of Vermont load.[4] The same pattern would pertain for nuclear and much QF capacity (depending on utility assumptions on capacity factors). Hence, it is not at all surprising that the HQ purchase would contribute to a glut of baseload energy, without satisfying all of Vermont's capacity needs.

Q: Do Mr. Boucher's assertions regarding the resale price of HQ power change your analysis of this issue?

A: No. Mr. Boucher simply asserts that the power can be resold at a profit. He does not specify any of a number of important assumptions regarding the resale market he assumes, including:

- when the power would have to be resold to achieve these prices,
- the length of time for which the power would have to be resold,

[3]In fact, the peaking capacity would have to operate some of the time, providing still more energy, which would require yet more capacity to firm up.

[4]This 690 MW of capacity would serve 575 MW of peak load, and about 3.2 TWH of energy, of which 3 TWH would be provided by the 450 MW of HQ capacity and the other 0.2 TWH would be provided by the peaking capacity at an average 10% capacity factor.

- the lead time for commitments which would achieve these prices, and
- the extent of future DSM and QF development, and hence of future competition for power sales markets.

Mr. Boucher does not appear to rebut either my description of the short-term energy sales market or ESRG's projection of the future market for excess baseload capacity. As I understand ESRG's estimate of resale prices, those prices would usually be lower than the cost of the HQ contract, and would consist only of Vermont's marginal fuel cost (likely to be a low value if Vermont has a glut of baseload power), plus 1.8 cents/kWh in 1989$.[5]

Mr. Boucher does point out that capacity has some value, above the split-savings energy price I discussed in my testimony. I do not believe that Vermont is as likely to suffer from a glut of capacity as it is from a glut of energy, and hence I assumed that only the energy would be resold.[6] If Vermont has excess generation capacity, and if capacity

---

[5]ESRG's workpapers indicate an expectation of significant baseload power sales, up to 150 MW for five years and over 50 MW for 15 years. With the base load forecasts, ESRG expects sales of 150 MW for 2-3 years, and 50 MW or more for 9-11 years. Similarly, GMP's Integrated Resource Plan (Exhibit 4-U) projects that it would have to make off-systems sales over the period 1998-2017 with a present value of over $80 million, which is about half of the net benefits ESRG projects for the entire purchase by all of the Participants (e.g., in Table 9.6). Thus, it is clear that resales (and the prices assumed for them) are very important in this case, and in the evaluation of DSM following this case.

[6]As noted above, Mr. Boucher's direct and rebuttal testimony concentrates on the threat of a <u>capacity</u> deficiency.

continues to be tight, Vermont could probably sell capacity at about $100/kW-yr or 1.5 cents/kWh of HQ energy.[7]

Q: If the Vermont utilities could demonstrate that they had an unlimited market for the resale of HQ energy, on a short-term and long-term basis, would such a demonstration resolve your concerns about the conflict between the HQ contract and conservation?

A: Not necessarily. My primary concern with the issue of HQ resales is that the utilities, in evaluating conservation investments, will argue that their avoided energy costs are low because of the large commitment to the HQ purchase, and will not include full-cost resales in this calculation. If conservation investments are limited to those which are cost-effective compared to the variable half of the HQ energy charge, or to Vermont's remaining low-variable-cost sources, very little conservation will be achieved.

My concern with this issue is not hypothetical. Utilities generally assume low or zero prices for off-system sales of energy freed up by DSM or QFs. I used the split-energy-cost example in my direct testimony, because that is how CV models

---

[7]The $100/kW is from recent utility estimates (e.g., NEPCO's W-10 filing at FERC) of the short-term cost of purchasing peaking capacity. CV's estimate of this cost in its New Hampshire filing is less than $80/kW. Utility projections of new peaking capacity (which limit the long-term market price) are usually lower than even the CV estimate of short-term purchases. I assume a 75% capacity factor for converting capacity value to HQ energy terms. It is important to recall that within this decade, capacity was available for under $20/kW-yr, due to the relationship of supply to demand.

economy energy transactions in its avoided-cost computations, as demonstrated in Exhibit CLF-PLC-R2.[8] Many utilities with an excess of low-cost energy maintain that they have no predictable markets for the power, and refuse to include any resale credit in their evaluation of QFs and DSM. Exhibit CLF-PLC-R3 provides an excerpt from testimony by Northeast Utilities on this subject.

Q: Do you have a proposal for resolving the conflict, if the Participants believe Mr. Boucher's conclusions regarding resale?

A: Yes. In order to ensure that the HQ contract does not foreclose future development of cost-effective conservation resources, the Participants would have to commit to assuming that any energy freed up by conservation could be resold at the cost of the most expensive HQ purchase in effect at the time.[9] If the energy is to be priced without capacity, the price could be reduced by the market value of capacity, which should not be higher than the cost of new peaking capacity. This approach would essentially place a floor on the hourly

---

[8]This Exhibit is an excerpt from CV's Least-Cost Plan filing in New Hampshire.

[9]The freed energy might not appear in the own-load dispatch model as a reduction in HQ purchases. Instead, it might be a reduction in purchases from other sources. However, the cost of the marginal sources will be lower with HQ than without HQ, and conservation which would be cost-effective compared to the energy supply without HQ (and compared to the full price of the HQ purchase) would be frozen out compared to the variable cost of the energy supply with HQ, unless resale is assumed.

avoided energy cost, equal to the capacity-adjusted HQ energy price.

Q: Under this proposal, what would happen if the utilities were unable to resell the energy at the price of the HQ purchase?

A: In order to ensure that the ratepayers will not be at risk for the utilities' optimism regarding the resale market for HQ power, the utilities' shareholders would have to be responsible for the difference between the price of the contract and the price of any off-system sale. As a corollary, the investor-owned utilities would have to promise to repurchase excess power from the publicly-owned utilities (which have no equity-holders, other than their customers), at the cost of the HQ contract.

Q: Does the ESRG study, or any portion of the DPS's filing, address the conflict between the proposed HQ contract and the development of DSM potential?

A: No. ESRG did not attempt to estimate total cost-effective DSM potential, to determine whether additional conservation potential beyond the "Strong" DSM portfolio was achievable or cost-effective, nor to determine whether more DSM would be cost-effective if it were allowed to compete with the HQ contract, rather than being compared to avoidable system costs following addition of the HQ contract.[10] Thus, the DPS testimony does not address the issues raised in my direct

---

[10] See the next section of this testimony for a further discussion of this issue.

testimony, and does not provide the Board with any guidance related to those issues.

DSM POTENTIAL

Q: Did ESRG attempt to estimate the amount of cost-effective conservation potential in Vermont?

A: No. ESRG explicitly disavows any intention to identify any sort of DSM potential (IR CLF 1-1). The ESRG "Strong" DSM program is simply an example of what might be instituted quite quickly by borrowing well-understood techniques and copying existing programs.

In designing the "Strong" DSM case, ESRG conservatively estimated the effects of a few major programs, composed of a limited set of measures. The programs were limited to those which affected "major electricity end-uses and rapidly growing sectors in the Vermont economy" (p. 3-8), which would produce "significant" energy or demand savings, and which used "tested and available technologies and measures" (p. 3-5). Thus, ESRG excludes all programs and measures which affect minor classes or minor end uses, which produce small incremental savings, or which rely on emerging or future technologies.

Q: Please describe some of the omissions of the ESRG analysis, compared to an assessment of conservation potential.

A: There are several such omissions. First, the ESRG report does not appear to attempt to define the optimal level of conservation technologies. For example, neither the report nor the workpapers indicate any investigation of the most socially cost-beneficial thickness for water-heater wraps or attic insulation, or of the minimum number of hours an incandescent light could operate annually and still be cost-effective to replace with a compact fluorescent. These issues are not relevant to ESRG's purpose of analyzing the effect of a selection of off-the-shelf DSM programs, but they would be vital to the determination of total conservation potential.

Second, the ESRG report is far from comprehensive. The report does not discuss agricultural conservation programs (such as using the reject heat from chilling milk to heat the water needed in dairying), or heat pumps for water heating or space heating (either air-to-air, ground-coupled, or water-source).

Third, ESRG neglects all new, emerging, and future technologies. In the area of refrigerators and freezers, for example, there is no discussion of evacuated-panel insulation technology, passive storage cooling (a promising use of one of Vermont's major resources, cold weather) or even the mass production and mass marketing of the Sunfrost refrigerator, which is already commercially available and uses only about 200 kWh/yr as opposed to ESRG's projection of 900 kWh/yr for the average refrigerator in 2008. Similarly, no improvements in compact fluorescents are addressed in the report, even though this is a rapidly changing technology.

Q: Does the ESRG report properly integrate DSM and the major supply-side option it considers, the HQ purchase?

A: No. ESRG does not allow conservation to compete with the HQ purchase. Instead, conservation is evaluated against the lower avoided costs which would remain after the HQ contract becomes final.[11] Hence, ESRG would screen out DSM which is cost-effective compared to the HQ contract, but which is more expensive than the avoidable energy costs after the HQ contract is added.[12]

Q: Do you have any specific responses to the ESRG analysis?

---

[11]ESRG has not yet provided any documentation of the derivation of its avoided costs, so it is not clear whether it assumed that baseload power freed up by conservation could be resold at the same price excess baseload power from the HQ purchase.

[12]In fact, ESRG has not attempted to determine the total size of the DSM resource, for any cost-effectiveness test. Hence, ESRG may not have examined the marginal programs and measures at all.

A: I have several specific observations.[13] First, ESRG incorrectly accounts for free riders, and in the process overstates the costs of conservation. Free riders are defined as those customers who would have undertaken the conservation program anyway, regardless of the existence of the program. Since ESRG performed a pre-program load forecast and then derived a DSM program coordinated with that forecast, ESRG should be able to demonstrate that the free riders incorporated in the DSM evaluation are incorporated in the base case load forecast. To date, ESRG has not done so.

To account for free riders, ESRG reduces kWh and kW savings, and hence program benefits, by a fraction which represents ESRG's estimate of free riders. In other words, it subtracts the savings which would have happened without the

[13]The ESRG report covers too much material, and is too sparsely documented, to allow for comprehensive review within the scope of this proceeding, given the late date at which the analysis was filed. In order to allow for timely review, the report should have included such items as: the annual values of the avoided costs, and a complete explanation of their derivation; the derivation of the externality adders; a complete list of the programs and measures considered, and the analysis which screened out programs and measures; the derivations and sources of all of the assumptions underlying the projections of costs and savings; detailed program assumptions, such as the number of units included in the program for each year, and the derivation of those projections; and the computations of program cost-effectiveness. While some of these materials were provided on discovery, much of the material provided in response to discovery is incomplete, contradictory, and/or incomprehensible. At least a second round of discovery would be required for a full review of ESRG's results. The same is true for DPS's own workproduct, in the testimony of Doug Smith. His Exhibit DCS-3 is described in his testimony as NEPLAN assumptions for generic additions, but it has nothing to do with generic additions. Exhibit DCS-4 is entitles "Vermont Generation Mix," but the values within the table cannot be in MW or GWH, or annual dollars, and they do not appear to be total $/MWH or $/kW-yr.

program. This is a reasonable approach, if all of the costs which the free riders would have incurred without the program are also subtracted. In many cases, such as weatherization, lighting and fuel switching, the measure costs would be much higher for individual customers than for the rationalized and efficient utility program. At the very least, they will be at least as large as the average direct costs (i.e., excluding administrative costs) in the utility program. However, ESRG does not reduce the program costs to eliminate any of the costs which the free riders would have incurred without the program. Hence, the ESRG approach does not represent social cost, has no relevance to least-cost planning, and is biased against conservation.[14]

Second, ESRG has used inconsistent assumptions in the evaluation of programs. For the water heater control program, which fits well with the HQ purchase, ESRG assumes that each controlled water heater saves 1.03 kW on the winter peak; since most control schemes are not 100% effective, an uncontrolled water heater must use more than 1.03 kW.[15] However, for the efficient-water-heater program, and for the fuel-switching program, both of which compete with the HQ purchase, ESRG assumes that water heaters contribute only 0.19-0.20 kW to

[14]Load management generally has no free riders, so the methodology is not biased against load management.

[15]The water heater tank wrap program appears to use equivalent values.

winter peak.[16] This set of assumptions biases the analysis against fuel switching and other electric conservation measures, and toward load management and HQ.[17]

Third, some of the ESRG programs appeared to be inefficiently designed. For example, while the ESRG tank wrap program saves 1000 kwh per participant for about $56, the "best tank" program saves only 276 kWh for $70.[18] It appears that ESRG would have projected higher DSM savings (at a lower cost) if it had dropped out the "best tank" program, and specified wrapping and aquastat adjustment for all new tanks. Still higher savings can be achieved if new water heaters are efficient models and are also wrapped and adjusted.

Fourth, ESRG appears to ignore all customer benefits from conservation measures. In most cases, the documentation indicates no credit for the replacement of aging equipment which otherwise would have been replaced within a few years in any case; examples of this effect include ballasts in the C/I/I

---

[16]Among other things, ESRG assumes that all water heaters which are controlled are average 1984 models, but that the water heaters which would be fuel-switched comply with the new federal efficiency standards. This set of assumptions also biases the analysis toward a combination of water heater control for load shifting, and the HQ contract for energy, and away from conservation and fuel switching.

[17]It is difficult to estimate the magnitude of the effect on ESRG's result, since ESRG has not yet provided a derivation of its "Strong" DSM case. It is possible that ESRG would not have included any more fuel-switching, regardless of the cost and effectiveness assumptions.

[18]ESRG's documentation is inconsistent on some of these points. For example, various parts of the documentation of the tank-wrap program report costs of $56.10 and $65.23.

lighting program.[19] Similarly, no credit is given for compact fluorescents replacing several incandescents, which would have nearly the same direct cost and much higher replacement labor costs for business customers.

Fifth, ESRG appears to assume that no marginal transmission costs are avoidable by DSM, and that avoidable distribution costs are only $15/kW. CVPS's rate design analyses conclude that the marginal cost of T&D per kW of system peak load growth is about $63/kW in 1990$.[20] PLC Inc. analyses of similar data from Boston Edison and Massachusetts Electric indicate that their marginal T&D costs are significantly higher than the CVPS estimates. ESRG does not document the derivation of its T&D assumptions; indeed, the report does not even contain the values used for avoidable T&D costs. Understating avoidable T&D costs would understate the cost-effectiveness of conservation measures, and of some load management.[21]

Sixth, ESRG's analysis is heavily biased against fuel-switching. Of the 155,812 electric water heaters ESRG forecasts for 1999, the "Strong" DSM case fuel-switches 43306, controls 44680, wraps 39856, and encourages a "best tank" for

---

[19]The cost ESRG assumes for water heaters in the fuel-switching program appears to be net of an electric water heater tank. More generally, ESRG does not distinguish between the costs of fuel-switching existing buildings and the much lower costs of fuel-switching new buildings.

[20]See testimony of JC Cater, Docket 4634, Exhibit JCC-5.

[21]Water heater load shifting may increase T&D costs, so the effect of higher marginal T&D costs on load shifting is generally unfavorable.

20391.[22] As shown in Table 1, ESRG's assumptions imply that the present value of the net social benefit of switching a water heater is about $1900, while controlling the water heater is worth only $1200, and the wrap and "best" programs are worth less than $400 and $100, respectively. The fuel-switching should also be credited with another 80% of the gross benefits of control (or $1000), to correct for ESRG's error in assuming different peak savings in the two programs. ESRG has not explained why even the "Strong" program fuel-switches only about 28% of the water heaters, given the substantial superiority of fuel switching over the other measures.[23]

Q: Does the failure to fuel-switch all water heaters leave a substantial amount of potential savings unrealized?

A: Yes. Fuel-switching all of the water heaters in the other programs (and even neglecting those which are not affected by any program) would produce some $230 million dollars of additional social net benefits, above the level in the ESRG "Strong" DSM program. This potential saving alone, without any

---

[22]At least 7,000 water heaters are unaffected; since "best" and wrapped water heaters can also be switched or controlled (in the case of fuel-switching, the new or wrapped water heater would use a fossil fuel), the actual number of water heaters not touched by the program would be greater.

[23] The neglect of fuel-switching may result in part from the previously noted problems with ESRG's methodology, virtually all of which cut against fuel switching. In addition, ESRG has assumed that fuel-switching lasts only 10 years for water heating and 20 years for space heating. ESRG is implicitly expecting that all fuel-switched customers will switch back to electricity as soon as the initial fossil-fueled end-use equipment fails.

other enhancements of the DSM program, swamps the projected benefits from the HQ contract in most of the cases shown in Tables 9.3-9.8. Similar enhancements to the other DSM programs might well produce several times these savings.

Q: Do you have any other reason to believe that ESRG's results for fuel switching are unrealistic?

A: Yes. My own comparison of fuel-switching residential electric end uses to gas indicates that (at least on an avoided-cost basis) gas has sizable advantages. This analysis, performed for Boston-area utilities, is attached as Exhibit CLF-PLC-R4.

Q: Do you have any comments further regarding the water-heater control program which ESRG assumes in its DSM program?

A: Yes. Taken as a whole, ESRG's assumptions regarding the water heater control program are unlikely to describe any actual program. ESRG assumes an unspecified control strategy which:

- is apparently 100% effective, avoiding all contribution to peak load,[24]
- is applied only to average 1990 units, not to any unit which participates in the "best tank" or tank wrap programs,
- does not increase energy usage,
- costs only $50-60 per water heater per installation,

---

[24]This observation is based on the fact that the highest winter peak contribution ESRG reports for water heaters is .227 kW/MWH, and that ESRG assumes 4515 kWh for the average water heater, or 1.025 kW peak contribution per existing water heater. The water heater control program assumes 1.027 kW reduction per water heater.

- costs only $3 per water heater per year for administration (and nothing for operation), and
- requires no customer investments.

It is difficult to imagine what this control mechanism might be. Controlled water-heating programs usually increase usage, since tank size and/or water temperature must be increased to achieve sufficient storage. If tank size is to be increased, the customer's cost for the larger tank must be included. The benefits of load-shifting programs are reduced by conservation programs. Perhaps most fundamentally, the combination of characteristics and costs ESRG has assumed are simply inconsistent.

Inexpensive control equipment, such as time clocks, are generally set for long interruptions, to increase the probability they will reduce peak load. These long interruptions especially require large tanks and high water temperatures, and (unless service quality is to be reduced) require that the top element be left on, so some hot water will be available at the end of the interruption period.[25] The combination of timer failures and top-element operation usually results in a substantial amount of clock-controlled water heaters operating during the interruption period, especially towards the end of the period. The clocks generally require periodic service and resetting (especially for the transition

---

[25] If the top element is not left on, then the customer costs associated with the degradation of service must be included in the cost-benefit analysis.

from summer to winter load patterns, but also to correct for mechanical or electronic problems). If the clocks do not have backup batteries, they will have to be reset after each outage; if they do have battery backup, the batteries will have to be replaced periodically. It is difficult to believe that none of these activities will have any costs.

More sophisticated control equipment, such as radio and ripple control, allows for greater flexibility, shorter interruptions, smaller tanks, lower temperatures, and hence lower standby losses. The interruption at peak will also tend to be larger, since more of the water heaters are apt to be controlled at peak (and top-element operation may not be necessary). However, limited storage and limited interruption periods may result in some monthly peaks outside the interruption periods; for example, with a three-hour allowed interruption, the utility may interrupt the load at 5 pm, only to find that load continues to grow and is actually higher at 8 pm when the water heaters must be returned to service.[26]

As noted above, the emphasis on water heater control, and the corresponding neglect of conservation and fuel switching biases ESRG's analysis in favor of a larger HQ purchase.

---

[26]This problem is compounded by the fact that the water heaters will typically use roughly 2-3 times as much power in the first hour after the interruption as they would have used without interruption. Even if the water heaters return after the system peak, any significant penetration of water heaters is likely to cause new peaks on portions of the distribution system, from line transformers to substations, and even onto the transmission system. ESRG does not seem to have accounted for the "snap-back" effect on generation, transmission, or distribution costs.

Q: Is ESRG's treatment of the "second generation" of DSM programs reasonable?

A: No. The second generation of programs appears to represent only an arbitrary assumption, without any supporting computation. ESRG assumes that the first generation programs end by 1999, even though much of the potential in these programs would not yet be realized.

ESRG also assumes that all conservation achieved in the first generation of programs is undone at the end of the life of the equipment in the program. Implicitly, ESRG assumes either that customers will abandon the conservation technologies or that all of the first generation investments are implicit in the base forecast for the period beyond the year 2000. In order to abandon some technologies, customers need only return to older technology choices, such as standard efficiency ballasts and incandescent light bulbs. This behavior is possible, but it is likely that a substantial portion of the participants will continue to use the more economical efficient equipment. In other cases, participants would have to tear out the investments, such as insulation and infiltration control. It is possible that some customers will reconvert to electric heat or hot water once the fossil heating equipment fails, but it is likely that most will continue to use the service drops, fuel tanks, flues, and other long-lived equipment. Insulation, major infiltration bypass reduction, and design improvements in new buildings are likely to last as long as the buildings. As to the second possibility, ESRG does not attempt to demonstrate that the first-generation programs are subsumed in the base forecast.

When the first generation programs are terminated, ESRG does not replace them with a more aggressive set of programs, based on the higher real avoided costs and better DSM technologies expected in the next century. Instead, ESRG assumes essentially no net conservation from 1999 to about 2003, a very slow growth in total conservation through 2011 (conservation as a percentage of base energy hardly changes from 2000-2005, and declines from 2005-2010), and complete termination of all programs in 2011. No explanation for this projection is offered.

Q: What would be the effect of continuing the DSM programs beyond 1999 at the same pace as the first generation of programs?

A: ESRG starts out fairly aggressively, with increases in savings for 1990-95 equal to 7.4% of 1995 base sales, or 7.2% of 1995 base sales net of the effect of the 1990 savings level. These rates are equivalent to about 1.4% or 1.5% of sales annually. If energy savings continue to increase at 1.4% of sales annually, they would reach 1 TWH by 2000 and 2.5 TWH by 2010. If energy savings increase at 1.5% of net sales (that is, net of the effects of prior conservation programs), they would reach 2.0 TWH by 2010. ESRG's decelerating programs reach only 0.9 TWH in 2000, and 1.1 TWH by 2010.

Hence, simply continuing to ramp up the conservation effort at the same level of activity ESRG assumes for the early 1990s would double the amount of conservation achieved by 2010.

Q: What effect might a larger DSM program have on the cost-effectiveness of the HQ purchase?

A: The ESRG analysis indicates that addition of QF capacity substantial decreases the net benefits of the HQ contract. Large increments of conservation would be expected to have similar effects. If the UPLAN runs examined a realistic choice between more HQ power and more DSM, the benefits of the HQ purchase would be further eroded.

Unfortunately, ESRG did not compare the HQ purchase to DSM, nor did it examine the sensitivity of the purchase's benefits to the size of the DSM program.[27] If a more aggressive conservation program had load effects equivalent to the difference (about 0.6 - 0.7 TWH/yr in the 2000-2005 period) between the base/strong and low/medium combinations of forecasts and DSM programs, it would cut the revenue requirement benefits of the HQ purchase roughly in half (from $43 to $23 million 1989 PV dollars) in the low-fuel case.[28] The effect is likely to be larger in the high-fuel case. This reduction is value for the HQ purchase assumes that the same

---

[27]The Moderate DSM program was used only with the low load forecast, and the "Strong" DSM program with the base and high forecast. Hence, the DPS has not provided any UPLAN results which vary only by the scope of the DSM program.

[28]This analysis compares the DPS's Cases 3 and 6.

amount of conservation would occur with or without the HQ purchase: that assumption is inconsistent with least-cost planning.

Since the HQ purchase would sharply limit the development of conservation resources, the relevant comparison is between a high-HQ/low-conservation scenario and a low-HQ/high-conservation scenario. This comparison would be even less favorable for HQ than the previous one. For example, moving from DPS's Case 6 (Base load growth) with HQ, to Case 3 (Low load growth) without HQ, would save $552 million in 1989 PV dollars. The "Strong" DSM case has a social present-value cost of only about $240 million and reduces energy requirements by 0.9 - 1.0 TWH annually in the 2000-2005 period.[29] Even if achieving the additional load reductions required doubling the social cost of the DSM program, the high-conservation/low-HQ case would still be over $300 million less expensive than the low-conservation/high-HQ Case 6.[30]

---

[29]As noted above, the assumptions driving the evaluation of the ESRG DSM programs are biased against conservation. The actual cost of a well-designed package of programs with the projected effectiveness is likely to be lower than ESRG projects.

[30]In fact, the low-HQ case would still include 68 MW of CU purchases from HQ.

3. RISK OF DSM AND OF THE HQ CONTRACT

Q: Which witnesses testify on the risk of DSM and of the HQ purchase?

A: Dr. Johnston testifies on behalf of the Participants. The portion of the ESRG report sponsored by Dr. Nichols assumes a risk-reduction benefit for conservation.

Q: What is your assessment of Dr. Johnston testimony on DSM and the HQ purchase?

A: It is difficult to seriously evaluate Dr. Johnston's testimony, since he provides no data, information, or substantive analysis. Instead, he makes an abstract argument in favor of the HQ contract.

Q: Please outline that argument.

A: Dr. Johnston's discussion consists primarily of aphorisms -- "a bird in the hand is worth two in the bush" or "don't put all your eggs in one basket" -- which are hardly controversial. He then assumes, without any evidentiary support, that the HQ purchase is not risky and that DSM is very risky. From these assumptions, he concludes (not surprisingly) that the HQ contract is superior to DSM.

While Dr. Johnston's assumptions regarding the relative risk of DSM and of supply are diametrically opposed to those in the Proposal for Decision (PFD) in Docket 5270, he fails to respond to the record or analysis in that docket.[31]

Q: Do you have any comments on the body of Dr. Johnston's argument?

A: Yes. As noted above, the bulk of Dr. Johnston's conclusions are simply restatements of his assumptions. However, I have three comments on Dr. Johnston's confusion about the nature of DSM. First, his argument against putting all of Vermont's eggs into one basket cuts heavily against the HQ purchase, which would represent 30-40% of Vermont's energy requirements. Second, his totally hypothetical example supposedly comparing the risk of DSM to that of the HQ purchase is entirely unrealistic. DSM is not a single source, but rather thousands of separate measures and technologies in millions of applications. While any one application might be as risky as Dr. Johnston suggests, the law of large numbers and the independence of the measures and applications imply that the aggregate risk of DSM will be much smaller (proportionally) than the risk of any one application. Some caulking will fail, some wrapped water heaters will be retired early, and so on,

---

[31]See, e.g., "WHile DSM programs vary with regard to their certainty of success, well-designed programs are generally of lower risk than supply procurement." PFD Vol. II, p. 151, paragraph 644. More specifically, the PFD added that " . . . energy efficiency resources . . . reduce risks and improve environmental quality relative to electricity generation." Id., paragraph 646. Citing V.S.A. Secs. 248 (b) and 209 (d), the PFD stressed that the choice of energy efficiency over supply has been given legal recognition in Vermont: "A similar preference has been expressed by the Vermont Legislature." Id., paragraph 647.

but not all applications of any measure will fail, let alone all (or a high fraction of) measures.

Third, Dr. Johnston ignores the dynamic aspects of the risk analysis. If certain DSM measures are not working well, that condition can be identified quickly, those measures can be redesigned, and other measures can be accelerated and expanded to compensate for the shortfall. If HQ is unable to perform under Schedule B, that failure is likely to become apparent only once Schedule B is commissioned (and perhaps on short notice), and Schedule C is unlikely to make up the shortfall, since HQ's problems will affect all of its sales. If load growth slows and fuel prices fall, DSM programs can be slowed down, and if load growth and fuel prices rise, DSM can be accelerated, on lead times of a year or so. Other than the cancellation options (much of which must be exercised four years in advance), the Participants cannot similarly tailor the HQ purchase to changing conditions. The flexibility of DSM has long been considered one of its major benefits. Dr. Johnston totally fails to address these advantages.

Q: What is the basis for ESRG's 10% risk adder in Table 3.3?

A: The 10% value is taken from the PFD in Docket 5270.

Q: Is this value appropriate for comparing DSM to the HQ contract?

A: Assessing the riskiness of supply options is complex. The Vermont utilities have not performed any such analyses for their own system, either for typical incremental investments (such as 10-50 MW shares of combined-cycle units) or for the

HQ purchase. The 10% value appears to reflect the Hearing Officer's review of the Northwest Power Planning Council (NPPC) analyses of its own risks for typical incremental investments, which are roughly 500 MW coal plants. The 10% advantage to DSM includes the short lead time (1-2 years, as compared to several years for most new sources, including most of the HQ energy), the small increments, and the tendency of the effectiveness of many programs to correlate with load growth. This appears to be the best available estimate we have for the advantage of DSM over traditional supply.

The HQ purchase is much larger, and hence much riskier, than are typical additions. Figure 1 shows the NPPC's results for the sensitivity of avoided cost to the size and lead time for supply additions. This analysis considered the uncertainties in load growth projections, but no other risks (e.g., construction schedules and cost, operating costs, fuel costs, plant performance or reliability). The unit size is in average MW (MWH divided by 8760). The reference system is about 20,000 average MW and has an avoided cost of about 35 mills.[32] As the size of the incremental supply increases, the expected cost avoided by DSM increases, since DSM reduces the risks of over- and under-forecasting loads, and those risks are increased by large additions.

[32]This is a real-levelized value, with very low-cost public financing.

Table 2 shows the sensitivity of the NPPC risk adder to addition size, expressed as a fraction of system annual energy requirements. Rounding off a bit, the results indicate that percentage adder for size-related risk is about 1.13 times the 1.86 power of the ratio of addition size to the system size. For an addition of 5% of the system (such as a 50 MW QF at a 75% capacity factor on a Vermont-sized system), the regression suggests that the risk adder should be 0.4% of avoided cost. An addition of 30% of system energy (such as the non-cancellable portion of the HQ purchase) would result in a risk adder of 13.5%, or 13% more than the small unit.

If the 10% risk adder in Docket 5270 is appropriate for typical small supply additions, a 23% adder might be appropriate for comparing HQ to DSM.[33]

[33]Differences in the systems, existing supply sources, and incremental options will make the actual values (when those are estimated for Vermont) different than the values for the NPPC.

4. VALUATION OF ENVIRONMENTAL EXTERNALITIES

Q: Which witnesses testified on the valuation of environmental externalities?

A: Dr. Johnston testifies on behalf of the Participants, and Dr. Rosen sponsors Chapter 9 of the ESRG report, which includes the valuation of environmental externalities.

Q: Please summarize Dr. Johnston's position on externalities.

A: Dr. Johnston's position seems to be (1) that HQ will take sufficient care in protecting the environment, and the Board should thus assume that HQ-provided power will be environmentally benign,[34] and (2) that the Board should not be concerned about global warming, since the size, timing, and causes of global warming are not well understood.

The heart of Dr. Johnston's argument is based on a article in <u>Forbes</u> magazine, an explicitly business-oriented publication, not in a scientific or environmental journal. The Forbes article quotes many scientists who dispute one another's interpretation of data and who have competing climate models. It makes the case that there is uncertainty in the greenhouse debate. Most scientists conclude that the great uncertainty and the enormous potential effects of global warming justify prompt action to reduce or halt the buildup of greenhouse gases. Action to reduce greenhouse gases, even in the absence of perfect information about the effect of those gases, is

[34] See page 27, line 23, to page 28, line 4.

particularly important because the scientific disputes may only be resolved after the warming is unequivocally observable, by which time massive environmental effects will have occurred and additional effects will be unavoidable. The only scientist that the Forbes article cites as supporting a delay in action on global warming is President Bush's science advisor.

Dr. Johnston also quotes with approval short passages from an article by Stephen Schneider in Scientific American. Those excerpts might lead the board to believe that Schneider agrees with Dr. Johnston and the Forbes article. For the convenience of the Board, I have attached the entire article as Exhibit CLF-PLC-R6.[35]

For consistency, Dr. Johnston's concern with the insurance value of the HQ contract should also be applied to the insurance value of mitigating global warming. If the global warming concern is overstated, the world will be relatively well-off in the future, and will hardly miss relatively small sums spent on greenhouse mitigation today. If global warming is as serious a problem as it may well be, the world will be much poorer in the future, and will be struggling with a multitude of expensive and difficult problems; any failure to provide insurance today will exacerbate an already serious problem. Since the costs of mitigating the greenhouse if such mitigation is not necessary are small, and the costs of not

[35]The same issue of Scientific American also contains an article by Gibbons, et al., which concludes that efficiency investment is "the most sensible path available today."

mitigating if such mitigation is necessary are large, risk-averse decision-makers should prefer a high level of mitigation, even levels which exceed those justified at the expected (but not maximum) level of greenhouse effects.[36]

Q: How does ESRG use environmental externalities?

A: ESRG reported other parties' estimates of the direct costs and abatement cost of each of several types of air emissions.[37] It added 9% to the benefits of conservation measures, a value which has now been corrected to 17% for all but the fuel-switching programs, which remain at 9% for space heating and

---

[36]Dr. Johnston also suggests that DSM may have significant environmental costs from fabrication and delivery of equipment. He criticizes Messrs. Goodman and Marcus for failing to include these costs, and even criticizes me on the same grounds, even though my direct testimony did not quantify environmental effects. There will be some such effects, but it is not clear than these are greater than the second-order effects of supply sources (e.g., pollution and carbon emissions from cement manufacture, plant construction, coal mining and transport, oil refining, etc.) Also, DSM programs which collect hazardous materials and waste materials, and recycle and/or properly dispose of those materials (e.g., PCBs from ballasts, mercury from fluorescent tubes, CFCs in refrigerators), can have significant environmental benefits.

[37]ESRG also lists land use effects in acres, but does not use these estimates in its cost-benefit analyses.

14% for water heating.[38] ESRG also includes environmental externalities in its final cost-benefit analyses.[39]

ESRG drops the direct estimates of environmental costs, apparently because of the variability in the results ESRG derives. ESRG then uses an average of low-cost and high-cost abatement, which it apparently considers to be needed as part of an overall abatement strategy.

ESRG's environmental valuation results are dominated by CO_2, SO_2, and NO_x.

Q: Are ESRG's analysis and estimates reasonable?

A: Only in part. ESRG's analysis is clearly much more comprehensive and valuable than any other review of environmental externalities previously presented to the Board. However, ESRG generally understates the values of externalities, and overstates the technical uncertainties, particularly in the direct costing results. ESRG also

---

[38]It is not clear whether ESRG used any externalities in screening or designing its DSM programs. ESRG has not yet provided a derivation of the 9%, 14% or 17% values, and it is not clear why ESRG chose to model externalities (which it had expressed in $/lb and $/kWh) as a percentage of avoided cost. This treatment is unlikely to be correct, and unnecessarily complicates review of the results.

[39]ESRG has circulated a revised version of Chapter 9, which indicates that it was <u>not</u> attempting to value externalities. I assume that the Board is interested in the value of externalities, as opposed to whatever ESRG thought it was estimating. As noted below, if ESRG was attempting to determine the change in cost of achieving a given emissions target, as a function of supply decisions, the marginal costs of control are the relevant value. For valuing the changes in the level of emissions, either the direct estimates of costs or the marginal costs of abatement are useful.

estimates the average cost of abatement, which does not appear to be relevant to the purpose to which ESRG applies the results. Only marginal control costs seem to contain any useful information for valuing externalities. As a result of these problems, the ESRG externality results are almost certainly understated.

Q: Could you provide some examples of the shortcomings in ESRG's analysis of direct costing studies?

A: Yes. Great care must be exercised in applying or interpreting the results of existing studies which provide direct estimates of environmental costs. While ESRG presents a fairly detailed literature search of direct costing studies, it appears at various points to have overlooked several of the necessary cautions. The spread in the results of the various studies can be dramatically reduced if one concentrates on adjusting for the differences in value judgments (for instance, for the value of a life), and resolving differences in interpretation of input information.[40]

The attached PLC Inc. memo (Exhibit CLF-PLC-R7) describes some of the problems with ESRG's analysis of direct estimates of environmental costs. We cannot review all of ESRG's sources, because we have not received responses to our requests for documents.

---

[40]The ESRG report is a summary document, which provides only a brief description of approach, and no derivation of most of the numerical results. In some cases, I may be critiquing what ESRG <u>appeared</u> to do, but not what ESRG actually did.

Q: How do the marginal costs of abatement compare to average costs?

A: The marginal costs of control are higher than average costs for two reasons. First, the most expensive measures and installations are more expensive than average measures and installations in any package of controls. Second, the cost per ton of moving a plant from moderate controls (e.g., low-NO_x burners) to high-cost controls (e.g., selective catalytic reduction) will generally be much higher than the average cost of the high-cost controls.

Q: Why are the marginal costs relevant?

A: The cost of control is relevant either as (1) a measure of the social value of reducing externalities (e.g., the "revealed preference of legislators and environmental regulators) or as (2) the cost of controls which, due to a reduction in emissions, society can avoid and still attain the desired level of total emission levels. In either case, the marginal cost is relevant, either to tell us the most regulators are willing to make society pay, or to tell us what costs a rational society could avoid.[41]

Q: What changes would you suggest making in the ESRG externality values?

[41] Presumably, the abatement costs which are backed out, by a conservation program or by a change in fuels, will be the costs of the most expensive abatement measures which would otherwise be required. The rational utility will not delete from its plans a mix of low-NO_x burners and SCR; instead, it will delete a lot of SCR and replace it by low-NO_x burners.

A: In PLC, Inc.'s December 1989 report on valuing externalities, we recommended using values equivalent to \$22/T of CO_2, \$1750/T of SO_2, and \$3000/T of NO_x. I consider all of these values to be conservatively low. The CO_2 value is much lower than scrubbers or even than very-expensive tree planting. The other two values are much lower than likely direct costs. The NO_x value is also probably lower than the marginal cost of control in New England, particularly for SCR on new units.

As derived in Exhibit CLF-PLC-R7, replacing ESRG's flawed and implausible estimates with PLC, Inc.'s low-end estimates for the major externalities would more than double the value of ESRG's estimate of the total present-value differences of externalities with and without the HQ purchase. For example, for Cases 1 and 2 in Table 9.6, my still-conservative externality estimates would decrease the benefits of the HQ purchase by about \$80 million, to -\$106 million in the low-fuel case and +\$80 million in the high-fuel case. This modest correction to ESRG's externality estimates transforms the expected value (assuming the two fuel prices are equally likely) from about +\$57 million to -\$13 million.

These more realistic externality values make the full HQ purchase even less desirable.

5. LEAST-COST PLANNING

Q: Do you agree with Mr. Boucher's conclusion on p. 2 that the DPS's study demonstrates that the HQ contract is consistent with the pursuit of strong DSM investments and programs?

A: No, for two reasons. First, as I discussed above, the ESRG "Strong" DSM case does not represent a strong DSM effort, but only a compilation of programs which are now off-the-shelf, established approaches, with some severe and unrealistic limitations on the effectiveness of the program. Second, since neither the DPS nor Mr. Boucher (nor any of the Participants) have offered any evidence on the potential for cost-effective conservation,[42] Mr. Boucher cannot support his assertion. My direct testimony on the potential for cost-effective conservation, and on the severe conflict between the HQ contract and DSM remains uncontroverted.

More seriously, Mr. Boucher seems to view the role of DSM in least-cost planning in a very limited manner. Mr. Boucher seems to imply that the utilities' least-cost obligations with respect to DSM are satisfied, so long as they can do <u>some</u> token amount of DSM (such as the ESRG "Strong" case). In fact, least-cost planning requires that the <u>total</u> potential for cost-

---

[42]I exclude Dr. Johnson's testimony, which offers no basis or foundation for any conclusions about DSM cost or potential.

effective DSM be exploited. Otherwise, the resultant energy services will not be least-cost.[43]

Finally, Mr. Boucher suggests (p. 5) that utilities can satisfy their responsibilities regarding DSM by participating in a collaborative design program.[44] I agree with Mr. Boucher that the collaborative process can be very valuable and productive. However, mere participation in the collaborative process does not guarantee that the process will be successful, or that the optimal level (or any level) of DSM investment will result. In fact, if the utilities are successful in over-committing to the HQ contract, and to QF contracts, they may preclude the development of most of the cost-effective conservation potential.[45] Even the best-designed conservation programs will be limited in their application if the utilities have already committed themselves to take-or-pay contracts for the bulk of their energy needs.

---

[43] Mr. Boucher himself notes that "imposition of a DSM target is inconsistent with least-cost utility operations . . . The constraint of fixed DSM resource targets (sic) is inconsistent with prudent resource planning . . . such artificial constraints may prevent the re-optimization of resource-acquisition plans" (Rebuttal p. 4). Curiously, Mr. Boucher objects to Dr. Rosen's proposed DSM minimum targets for the Participants, but does not object to ESRG's (or the Participants') imposition of artificial constraints on DSM resources in their analyses of the HQ contract.

[44] To the extent that Mr. Boucher seeks to interpret the requirements of Section 248, his testimony is a legal analysis, which my clients may address in their briefs.

[45] As noted above, the utilities can get around this problem, at least partly, by promising to pursue DSM as if the HQ contract could be resold at full cost, and to absorb any losses resulting from the failure to sell off the contract.

Q: How does your analysis of the DPS's testimony change your opinion expressed in your direct testimony?

A: The DPS filing, with appropriate corrections, has provided us with enough information to conclude that aggressive DSM will be preferable to the HQ purchase, as currently structured. It has also helped confirm that the proposed HQ purchase (even at the minimum level) and aggressive DSM are mutually exclusive. Hence, I would recommend more strongly than previously that the Board reject the current contract and suggest that the Participants attempt to negotiate a more modest purchase.

Q: Do you have any final general observations regarding this testimony?

A: Yes. I have prepared this testimony prior to receipt of most of the discovery which would be required for a comprehensive review of the testimony to which I respond. Hence, some supplementation of this testimony may be necessary.

Q: Does this conclude your testimony?

A: Yes.

Table 1: ESRG Water-Heater DSM Options

Program	Benefit:Cost Ratio	Utility Cost/unit	Social Cost/unit	Social Benefit	Net Social Benefit	Units Treated	Added Benefit from Fuel Switching per unit	Added Benefit from Fuel Switching Total
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
"Best Tank"	2.3	$60	$80	$184	$104	20,391	$2,796	$57,013,236
Wrap up	7.8	$65	$65	$509	$444	39,856	$2,456	$97,898,293
Fuel Switching	3.6	$230	$733	$2,639	$1,906	43,306	----	----
Control	13.4	$94	$94	$1,260	$1,166	44,680	$1,734	$77,492,992
Total								$232,404,521

NOTES:
[1]: ESRG Table 3.3 revised.
[2]: ESRG Workpapers
Wrap up cost also reported as $56.10,
Control cost is $60 + $3/year for 20 years,
discounted at 5.8% real discount rate.
[3]: ESRG Workpapers
Fuel switching social net present value is 22.3/7 times as large
as utility net present value
[4]: [1] * [3].
[5]: [4] - [3].
[6]: ESRG Workpapers for 1999.
[7]: $2,900 (Fuel switch net benefit corrected for error in peak factor) - [5].
[8]: [7] * [6].

Table 2: NPPC Data on Unit Size and Risk Adder

Mills	USize	Adder Y [1]	Unit Size X [2]
0.04938	500	0.1%	2.5%
0.10935	1000	0.3%	5.0%
0.52911	2000	1.5%	10.0%
1.31572	3000	3.8%	15.0%

ln(Adder) ln(Y)	ln(Unit Size) ln(X)
-6.564	-3.689
-5.769	-2.996
-4.192	-2.303
-3.281	-1.897

Regression Output:

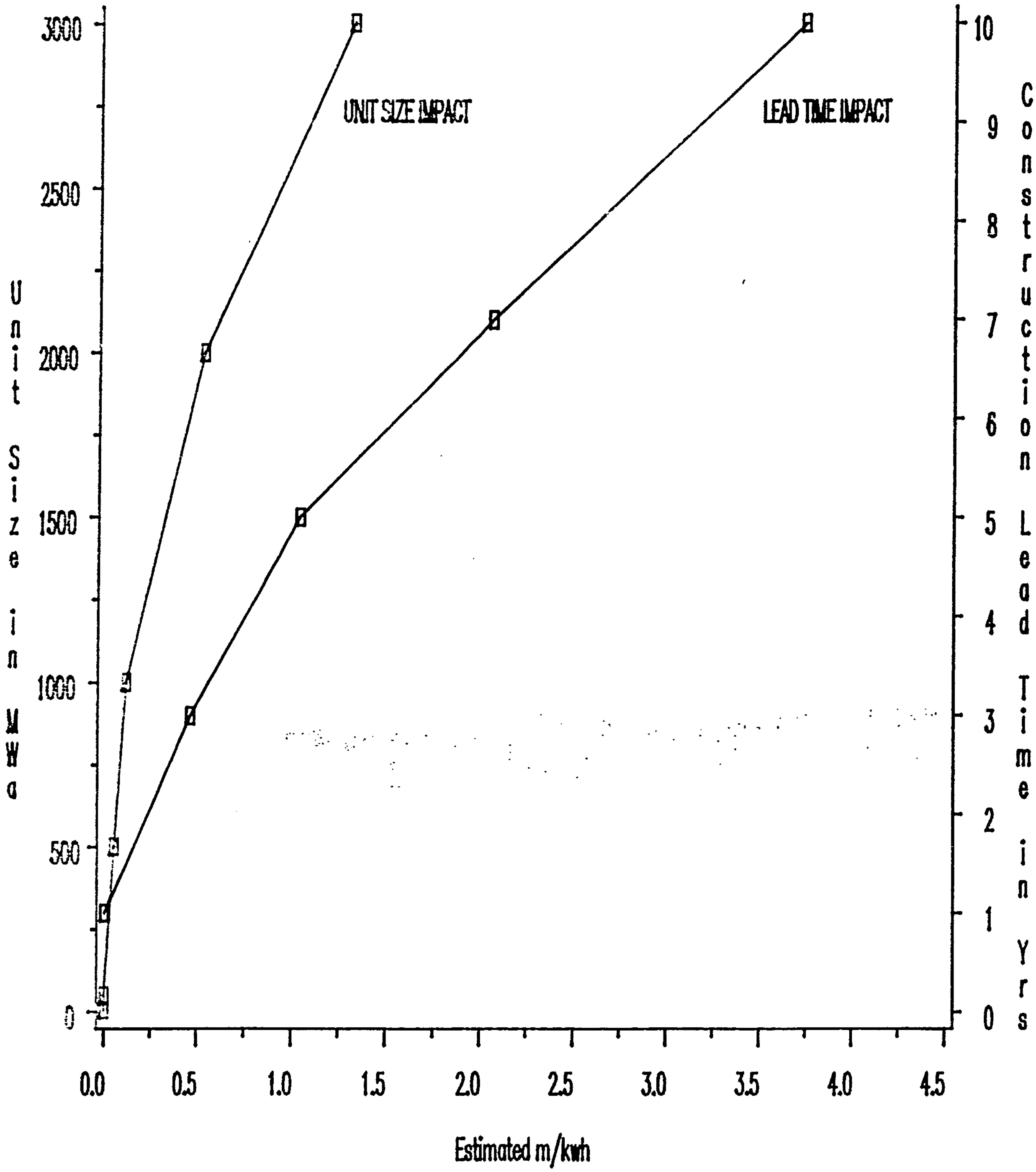
Constant		0.122
Std Err of Y Est		0.272
R Squared		0.978
No. of Observations		4
Degrees of Freedom		2
X Coefficient(s)	1.864	
Std Err of Coef.	0.199	
t-statistic	9.347	

Adder = e^0.122 * Size ^1.864 = 1.129 * Size ^1.864.

NOTES: [1]: Change in avoided costs
[2]: Unit Size as % of 20,000 MWH system.

FIGURE 1

Estimated Penalty of Increases in Unit Size and Lead Time



Conservation Law Foundation
Docket No. 5330
Exhibit CLF-PLC-R2

Excerpt from

CVEC Least-Cost IRP Filing

Conservation Law Foundation
Docket No. 5330
Exhibit CLF-PLC-R3

Excerpt from

Testimony of William Stillinger in

M.D.P.U. 88-123

THE COMMONWEALTH OF MASSACHUSETTS

DEPARTMENT OF PUBLIC UTILITIES

DOCKET NO. DPU 88-123

PETITION OF RIVERSIDE STEAM AND ELECTRIC COMPANY, INC.

FOR RELIEF PURSUANT TO 220 C.M.R. 8.03(2) AND 8.07(2)

TESTIMONY OF

WILLIAM L. STILLINGER

WESTERN MASSACHUSETTS ELECTRIC COMPANY

SEPTEMBER 1, 1989

C:\89NHAVLC\OFFSYSX.WK1
03-May-89
RJA

CVEC Least Cost IRP Filing
Adjustment to Avoided Cost
For Off System Exchanges
(In Nominal cents/KWH)
Base Year is 1988

	Fuel Escal. Rate	(A) Proxy Energy Cost (cents/kWh)	Sell (93% of A) (cents/kWh)	Buy (107% of A) (cents/kWh)	Own-Load Lambda (1)	Own-Load Lambda X 1.01 (inventory & work. cap)	Net System Exchange Sale (Purchase)	Own-Load Lambda with Exchange
	-----	-----	------	------	------	------	------	------
1988	--	2.44	2.27	2.61	--	--	--	0.00
1989	7.6%	2.62	2.44	2.81	2.60	2.63	0.00	2.63
1990	11.6%	2.93	2.72	3.13	2.83	2.85	0.00	2.85
1991	10.9%	3.25	3.02	3.47	2.85	2.88	0.07	2.95
1992	10.5%	3.59	3.34	3.84	3.26	3.30	0.02	3.32
1993	6.6%	3.82	3.56	4.09	2.57	2.60	0.48	3.08
1994	6.6%	4.08	3.79	4.36	2.74	2.77	0.51	3.28
1995	6.6%	4.35	4.04	4.65	2.91	2.94	0.55	3.49
1996	6.6%	4.63	4.31	4.96	3.10	3.13	0.59	3.72
1997	10.6%	5.12	4.77	5.48	3.42	3.45	0.66	4.11
1998	10.6%	5.67	5.27	6.06	3.76	3.80	0.74	4.54
1999	10.6%	6.27	5.83	6.71	4.14	4.18	0.82	5.00
2000	10.6%	6.93	6.45	7.42	4.57	4.62	0.92	5.54
2001	10.6%	7.67	7.13	8.20	5.03	5.08	1.02	6.10
2002	9.0%	8.36	7.77	8.94	5.47	5.52	1.12	6.64
2003	9.0%	9.11	8.47	9.75	5.95	6.01	1.23	7.24
2004	9.0%	9.93	9.23	10.62	6.47	6.53	1.35	7.88
2005	9.0%	10.82	10.06	11.58	7.04	7.11	1.48	8.59
2006	9.0%	11.80	10.97	12.62	7.66	7.74	1.62	9.36
2007	9.0%	12.86	11.96	13.76	8.33	8.41	1.77	10.18
2008	9.0%	14.02	13.03	15.00	9.06	9.15	1.94	11.09

Proxy Unit Information

Heat Rate : 10,600 BTU/kWh
Fuel Cost (2): $2.30 $/MMBTU
Energy Cost : 2.438 Cents/kWh

Fuel Cost (3): $14.46 $/BBL

(1) Avoided Cost of Energy Before Adjustments
(2) Includes 0.05 $/MMBTU Fuel Adder
(3) From GTF 1989 Exhibit 18, p.35; Residual Oil; Heat Content = 6.287 MMBTU/BBL

SALES REVENUES

Q. RSECO claims through Mr. Chernick's testimony that the Company should credit QF purchases with additional revenues from off-system sales. Do you agree this is legitimate?

A. No. The basics of forecasting capacity sales involve an estimate of capacity available for sale, and an estimate of the market for short-term sales. One cannot simply assume there will be such a market in the early 1990s, or that the Company will have capacity available for that market.

The DPU first considered and rejected the notion of including potential revenues from inter-utility sales in the development of avoided costs for the evaluations of conservation and load management in a recent WMECO retail rate case (DPU 86-280-A). The Department further rejected the idea in its decision in Docket No. DPU 88-19.

As we have explained previously, the Company will be active in the area of capacity sales offers if system and market conditions at the time permit. But it is system and market conditions, not simply QF levels, that will determine the availability of the Company's capacity for inter-utility sales.

The addition of resources (such as QFs, demand-side C&LM, Millstone 3, etc.) do indeed work in the direction of increasing the likelihood of short-term sales, but rising customer load levels and unit retirements decrease this likelihood. Trying to assign particular sales to particular resource additions is an especially fruitless task. Even if certain sales could be deemed attributable to QFs, such sales are made in order to reduce ratepayer costs resulting from the mandatory QF purchase during the years when the QF payments are above avoided cost. Management consciously makes decisions to reduce costs whenever the opportunity arises, thus the Company sees no merit to crediting QF projects with capacity sales revenues.

OTHER ISSUES

Q. On page 47 of his July 24 testimony, Mr. Chernick suggests that the RSECO project would bring about transmission cost savings,

Conservation Law Foundation
Docket No. 5330
Exhibit CLF-PLC-R4

"Analysis of Fuel Substitution

as an Electric Conservation Option"

[Bound Separately]

Conservation Law Foundation
Docket No. 5330
Exhibit CLF-PLC-R5

"The Valuation of Externalities

from Energy Production, Delivery, and Use"

[Bound Separately]

Conservation Law Foundation
Docket No. 5330
Exhibit CLF-PLC-R6

"The Changing Climate"

by Stephen H. Schneider,

Scientific American, September, 1989.

The Changing Climate

Global warming should be unmistakable within a decade or two. Prompt emission cuts could slow the buildup of heat-trapping gases and limit this risky planetwide experiment

by Stephen H. Schneider

In 1957 Roger Revelle and Hans E. Suess of the Scripps Institution of Oceanography observed that humanity is performing a "great geophysical experiment," not in a laboratory, not in a computer, but on our own planet. The outcome of the experiment should be clear within decades, but it essentially began at the start of the Industrial Revolution. Since then human beings have increased the atmospheric content of carbon dioxide by about 25 percent by burning coal, oil and other fossil fuels and by clearing forests, which releases carbon dioxide as the litter is burned or decays.

Carbon dioxide makes up only a thirtieth of 1 percent of the atmosphere, but together with water vapor and other gases present in much smaller quantities, such as methane and the chlorofluorocarbons (CFC's), it plays a major role in determining the earth's climate. As early as the 19th century it was recognized that carbon dioxide in the atmosphere gives rise to a greenhouse effect. The glass of a greenhouse allows sunlight to stream in freely but blocks heat from escaping, mainly by preventing the warm air inside the greenhouse from mixing with outside air. Similarly, carbon dioxide and other greenhouse gases are relatively transparent to sunshine but trap heat by more efficiently absorbing the longer-wavelength infrared radiation released by the earth.

STEPHEN H. SCHNEIDER is head of the interdisciplinary climate-systems program at the National Center for Atmospheric Research (NCAR) in Boulder, Colo. Schneider, who holds a Ph.D. from Columbia University, has written more than 100 scientific papers and has often been a spokesman for climatology—as a witness before Congress, an adviser to the federal government and an author of several popular books. The views expressed in this article are not necessarily those of the National Science Foundation, NCAR's sponsor.

By now the atmosphere's heat-trapping ability has been well established. For example, as seen from space, the earth radiates energy at wavelengths and intensities characteristic of a body at −18 degrees Celsius. Yet the average temperature at the surface is some 33 degrees higher: heat is trapped between the surface and the level, high in the atmosphere, from which radiation escapes. There is virtually no doubt among atmospheric scientists that increasing the concentration of carbon dioxide and other gases will increase the heat trapping and warm the climate.

What, then, is the question that the ongoing geophysical experiment will settle? Even though there is virtually no debate among scientists about the greenhouse effect as a scientific proposition, there is controversy. Will the rising concentrations of greenhouse gases raise the earth's temperature by one, five or eight degrees C? Will the increase take 50, 100 or 150 years? Will it be drier in Iowa or wetter in India? There is still more controversy when it comes to policy: Should steps be taken to reduce the greenhouse warming or to anticipate its effects? What steps, and when? In the face of so much controversy, an understanding of what is well known, known slightly and not known at all about the greenhouse warming is essential.

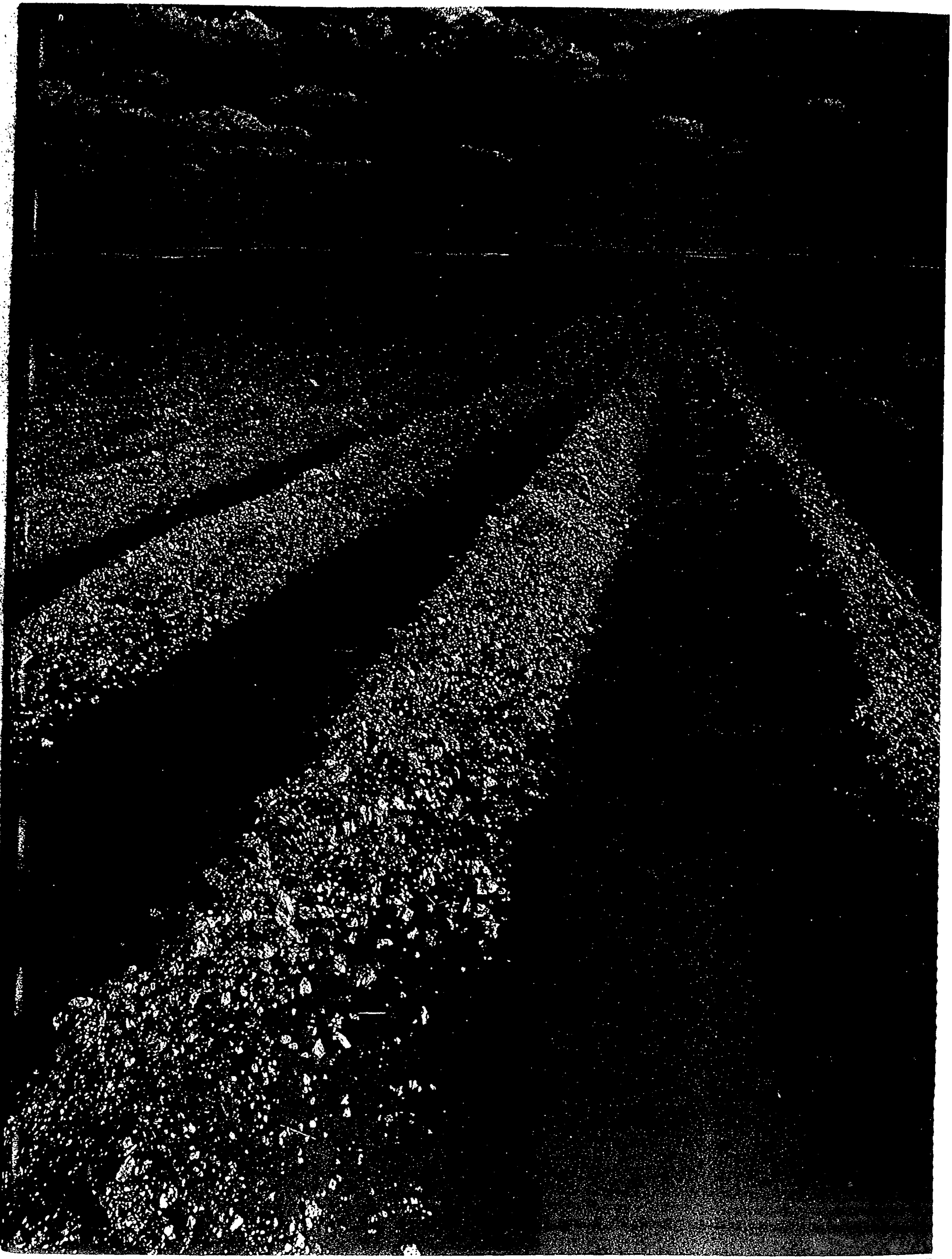
Circumstantial evidence from the geologic and historical past bears out a link between climatic change and fluctuations in greenhouse gases. Between 3.5 and four billion years ago the sun is thought to have been about 30 percent fainter than it is today. Yet life evolved and sedimentary rock formed under the faint young sun: at least some of the earth's surface was above the freezing point of water. Some workers have proposed that the early atmosphere contained as much as 1,000 times today's level of carbon dioxide, which compensated for the sun's feeble radiation by its heat-trapping effect.

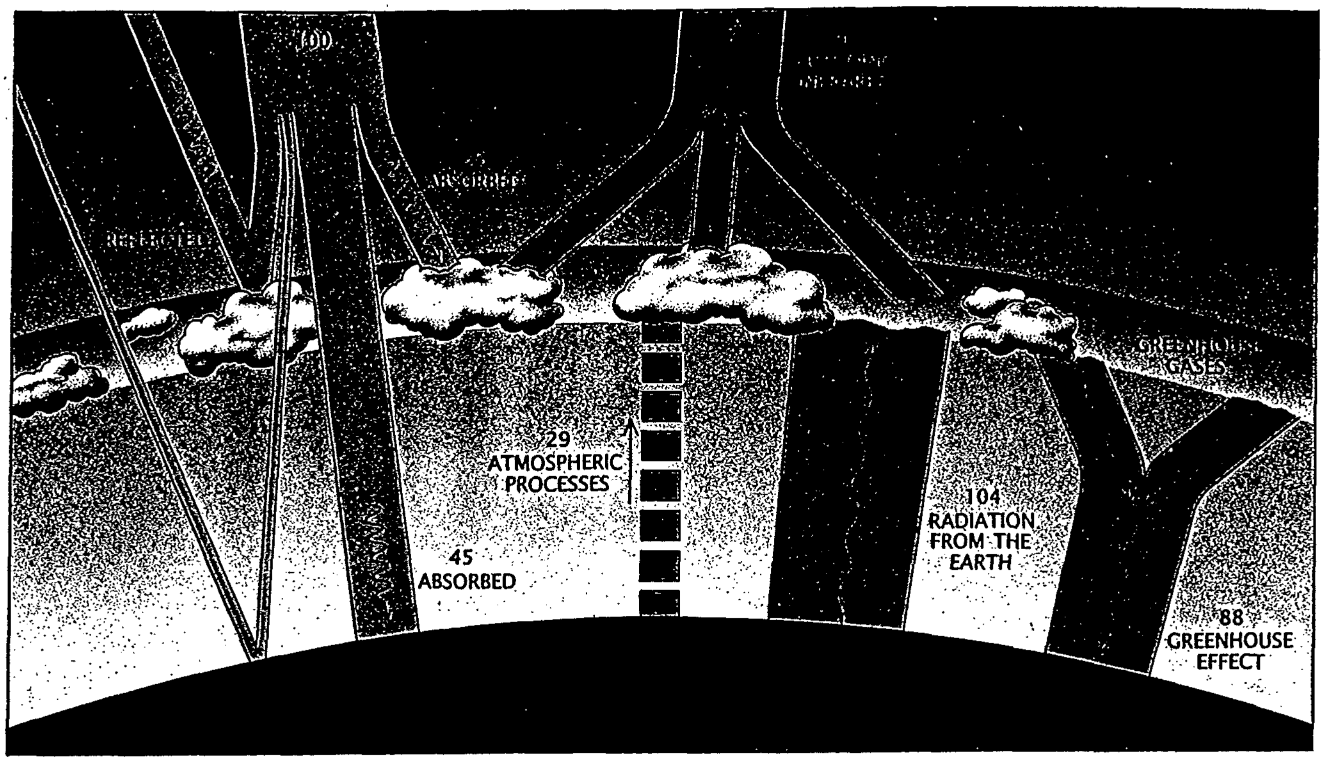
Later an enhanced greenhouse effect may have been partly responsible for the warmth of the Mesozoic era—the age of the dinosaurs—which fossil evidence suggests was perhaps 10 or 15 degrees C warmer than today. At the time, 100 million years ago and more, the continents occupied different positions than they do now, altering the circulation of the oceans and perhaps increasing the transport of heat from the Tropics to high latitudes. Yet calculations by Eric J. Barron, now at Pennsylvania State University, and others suggest that paleocontinental geography can explain no more than half of the Mesozoic warming.

Increased carbon dioxide can readily explain the extra heating, as Aleksandr B. Ronov and Mikhail I. Budyko of the Leningrad State Hydrological Institute first proposed and as Barron, Starley L. Thompson of the National Center for Atmospheric Research (NCAR) and I have calculated. A geochemical model constructed by Robert A. Berner and Antonio C. Lasaga of Yale University and the late Robert M. Garrels of the University of South Florida suggests that the carbon dioxide may have been released by unusually heavy volcanic activity on the mid-ocean ridges, where new ocean floor is created by upwelling magma [see "The Geochemical Carbon Cycle," by Robert A. Berner and Antonio C. Lasaga; SCIENTIFIC AMERICAN, March].

Direct evidence linking greenhouse gases with the dramatic climatic changes of the ice ages comes from bubbles of air trapped in the Antarctic ice sheet by the ancient snowfalls that

PARCHED FIELDS turn to sand during a 1983 dry spell in Texas. Such images could multiply if, as several computer models predict, global warming reduces soil moisture in midcontinental regions, where grain production is concentrated.





HEAT TRAPPING in the atmosphere dominates the earth's energy balance. Some 30 percent of incoming solar energy is reflected (*left*), either from clouds and particles in the atmosphere or from the earth's surface; the remaining 70 percent is absorbed. The absorbed energy is reemitted at infrared wavelengths by the atmosphere (which is also heated by updrafts and cloud formation) and by the surface. Because most of the surface radiation is trapped by clouds and greenhouse gases and returned to the earth, the surface is currently about 33 degrees Celsius warmer than it would be without the trapping.

built up to form the ice. A team headed by Claude Lorius of the Laboratory of Glaciology and Geophysics of the Environment, near Grenoble, examined more than 2,000 meters of ice cores—a 160,000-year record—recovered by a Russian drilling project at the Vostok Station in Antarctica. Laboratory analysis of the gases trapped in the core showed that carbon dioxide and methane levels in the ancient atmosphere varied in step with each other and, more important, with the average local temperature (determined from the ratio between hydrogen isotopes in the water molecules of the ice).

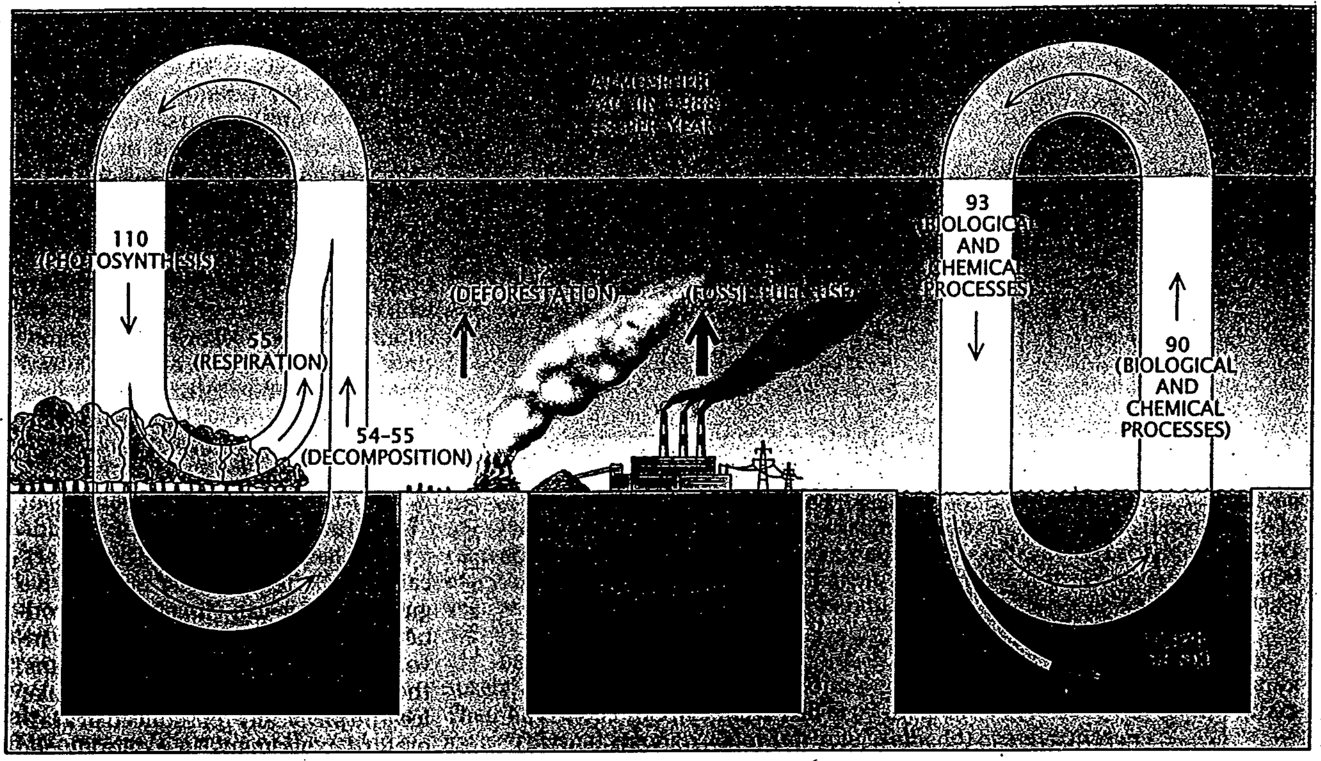
During the current interglacial period (the past 10,000 years) and the previous one, a 10,000-year period around 130,000 years ago, the ice recorded a local temperature about 10 degrees C warmer than at the height of the ice ages. (The earth as a whole is about five degrees warmer during interglacials.) At the same time, the atmosphere contained about 25 percent more carbon dioxide and 100 percent more methane than during the glacial periods. It is not clear whether the greenhouse-gas variations caused the climatic changes or vice versa. My guess is that the ice ages were paced by other factors, such as changes in the earth's orbital parameters and the dynamics of ice buildup and retreat, but biological changes and shifts in ocean circulation in turn affected the atmosphere's trace-gas content, amplifying the climatic swings.

A still more detailed record of greenhouse gases and climate comes from the past 100 years, which have seen a further 25 percent increase in carbon dioxide above the interglacial level and another doubling of atmospheric methane. Two groups, one led by James E. Hansen at the National Aeronautics and Space Administration's Goddard Institute for Space Studies and the other by T. M. L. Wigley at the Climatic Research Unit of the University of East Anglia, have constructed records of global average surface temperature for the past century. The workers drew on data from many of the same recording stations around the globe (the Climatic Research Unit also included readings made at sea), but they had different techniques for analyzing the records and compensating for their shortcomings. Certain recording stations were moved over the course of the century, for example, and readings from city centers may have been skewed by heat released by machinery or stored by buildings and pavement.

This "urban heat island" effect is likely to have been disproportionately large in developed countries such as the U.S., but even when the same correction calculated for the U.S. data (by Thomas R. Karl of the National Climatic Data Center in Asheville, N.C., and P. D. Jones of East Anglia) is applied to the global data set, about half a degree C of unexplained "real" warming over the past 100 years remains in both records. In keeping with the trend, the 1980's appear to be the warmest decade on record and 1988, 1987 and 1981 the warmest years, in that order.

Is this the signal of the greenhouse warming? It is tempting to accept it as such, but the evidence is not definitive. For one thing, instead of the steady warming one might expect from a steady buildup of greenhouse gases, the record shows rapid warming until the end of World War II, a slight cooling through the mid-1970's and a second period of rapid warming since then.

What trajectory will the temperature curve follow now? Three basic questions must be answered in forecasts of the climatic future: How much carbon dioxide and other greenhouse gases will be emitted? By how much will atmospheric



CARBON IS EXCHANGED between the atmosphere and reservoirs on the earth. The numbers give the approximate annual fluxes of carbon (in the form of carbon dioxide) and the approximate amount stored in each reservoir in billions of metric tons. The existing cycles—one on land and the other in the oceans—remove about as much carbon from the atmosphere as they add, but human activity (deforestation and fossil-fuel burning) is currently increasing atmospheric carbon by some three billion metric tons yearly. The numbers are based on work by Bert Bolin of the University of Stockholm.

levels of the gases increase in response to the emissions? What climatic effects will the resulting buildups have, after natural and human factors that might mitigate or amplify those effects are taken into account?

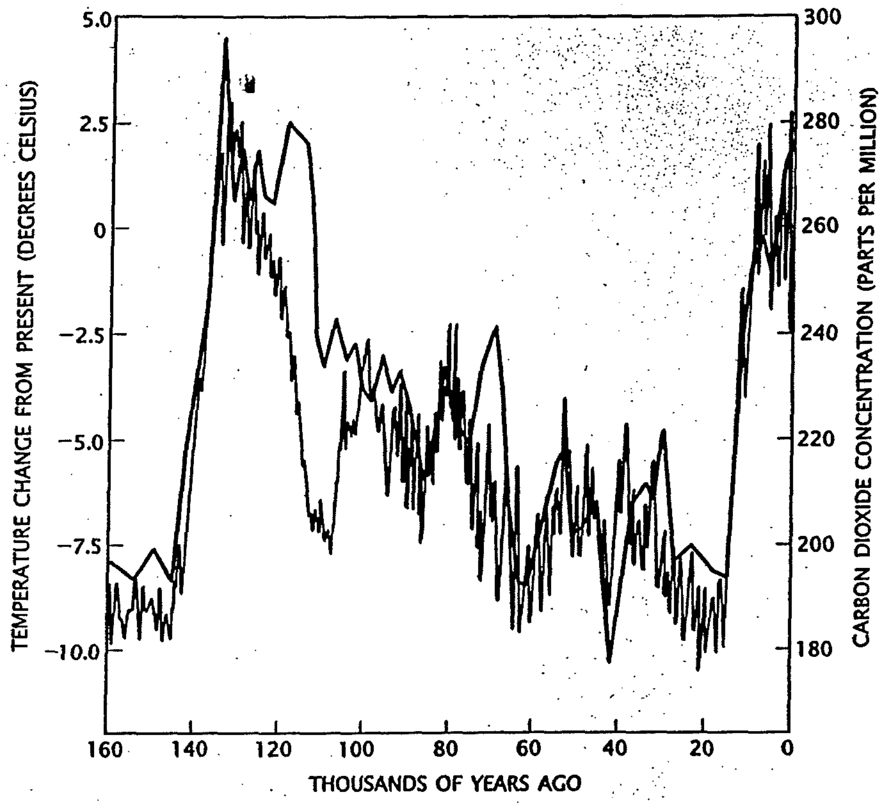
Projecting emissions is an intricate exercise in social science. How much carbon dioxide humanity as a whole will be emitting in the future depends primarily on the global consumption of fossil fuels and the rate of deforestation (which accounts for perhaps half of the buildup since the year 1800 and 20 percent of current emissions). Each factor in turn is affected by many others. Growth in fossil-fuel use, for example, will reflect population growth, the rate at which alternative energy sources and conservation measures are adopted and the state of the world economy. Typical projections assume that global fossil-fuel consumption will continue increasing at about its current pace—much slower than it grew before the energy crisis of the 1970's—yielding increases in carbon dioxide emissions of between .5 and 2 percent a year for the next several decades at least.

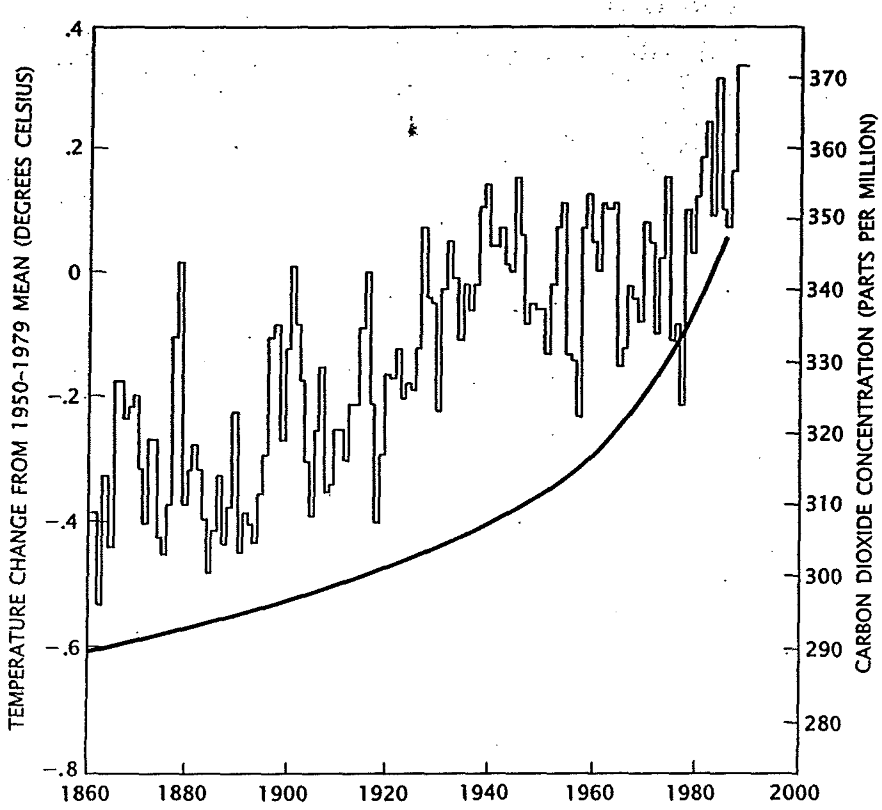
Other greenhouse gases, such as methane, the CFC's, oxides of nitrogen and low-level ozone, together could contribute as much to global warming as carbon dioxide, even though they are emitted in much smaller quantities: they are much better at absorbing infrared radiation. But predicting future emissions for these gases is even more complicated than it is for carbon dioxide. The sources of some gases, such as methane, are not well understood; the production of other gases, such as the CFC's and low-level ozone, could rise or fall sharply depending on whether specific technological or policy steps are taken.

Given a plausible scenario for future carbon dioxide emissions, how fast will the atmospheric concentration increase in response? Atmospheric carbon dioxide is continuously being absorbed by green plants and by chemical and biological processes in the oceans. The rate of carbon dioxide uptake is likely to change as the atmospheric concentration changes; that is, feedback processes will enter the equation. Because carbon dioxide is a raw material of photosynthesis, an increased concentration might speed the uptake by plants, counteracting some of the buildup. Similarly, because the carbon dioxide content of the oceans' surface waters stays roughly in equilibrium with that of the atmosphere, oceanic uptake will slow the buildup to some extent. (The slower the buildup is in the first place, the more effective, proportionally, oceanic uptake is likely to be.)

It is also possible, however, that an increased concentration of carbon dioxide and other greenhouse gases will trigger positive feedbacks that would add to the atmospheric burden. Rapid change in climate could disrupt forests and other ecosystems, reducing their ability to draw carbon dioxide down from the atmosphere. Moreover, climatic warming could lead to rapid release of the vast amount of carbon held in the soil as dead organic matter. This stock of carbon—at least twice as much as is stored in the atmosphere—is continuously being decomposed into carbon dioxide and methane by the action of soil microbes. A warmer climate might speed their work, releasing additional carbon dioxide (from dry soils) and methane (from rice paddies, landfills and wetlands) that would enhance the warming. Large quantities of methane are also locked up in continental-shelf sediments and below arctic permafrost in the form of clathrates—molecular lattices of methane and water. Warming of the shallow waters of the oceans and melting of the permafrost could release some of the methane.

In spite of all these uncertainties,





CARBON DIOXIDE AND TEMPERATURE are very closely correlated over the past 160,000 years (*top*) and, to a lesser extent, over the past 100 years (*bottom*). The long-term record, based on evidence from Antarctica, shows how the local temperature (*color*) and atmospheric carbon dioxide rose nearly in step as an ice age ended about 130,000 years ago, fell almost in synchrony at the onset of a new glacial period and rose again as the ice retreated about 10,000 years ago. The recent temperature record shows a slight global warming (*color*), as traced by workers at the Climatic Research Unit of the University of East Anglia. Whether the accompanying buildup of carbon dioxide in the atmosphere caused the half-degree warming is hotly debated.

many workers expect uptake by plants and by the oceans to moderate the carbon dioxide buildup, at least for the next 50 or 100 years. Typical estimates, based on current or slightly increased emission rates, put the fraction of newly injected carbon dioxide that will remain in the atmosphere at about one half. Under that assumption, the atmospheric concentration will reach 600 parts per million, or about twice the level of 1900, by sometime between the years 2030 and 2080. Some other greenhouse gases are expected to build up faster than carbon dioxide, however.

What effect will a doubling of atmospheric carbon dioxide have on climate? The historical record offers no clear quantitative guidance. Nor can climate—the product of complicated interactions involving the atmosphere, the oceans, the land surface, vegetation and polar ice—be physically reproduced in a laboratory experiment. In exploring the future of the earth's climate, my colleagues and I rely on mathematical climate models.

The models, which have been built at Princeton University's Geophysical Fluid Dynamics Laboratory, the Goddard Institute for Space Studies, here at NCAR and elsewhere, consist of expressions for the interacting components of the ocean-atmosphere system and equations representing the basic physical laws governing their behavior, such as the ideal gas laws and the conservation of mass, momentum and energy. Given values for, say, the input of energy from the sun and the composition of the atmosphere, a model calculates "climate"—temperature and, in sophisticated models, pressure, wind speed, humidity, soil moisture and other variables.

To keep the task computationally manageable, the calculations are done at discrete points in a simplified version of the real world. In the most complicated models—global-circulation models (GCM's), which were first developed for long-term weather forecasts—the atmosphere is represented as a three-dimensional grid with an average horizontal spacing of several hundred kilometers and an average vertical spacing of several kilometers; climate is calculated only at the intersections of the grid lines. In spite of the simplification, running such a GCM for only one simulated year can take many hours on the fastest available supercomputers.

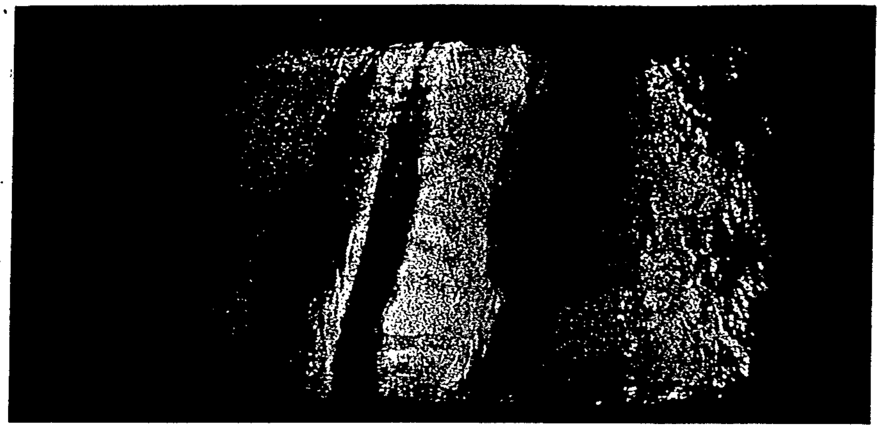
To study the effect of a trace-gas buildup, a modeler simply specifies the projected amount of greenhouse gases and compares the model results with a control simulation of the existing climate, based on the present atmospheric composition. The results of the most recent GCM's are in rough agreement: a doubling of carbon dioxide, or an equivalent increase in other trace gases, would warm the earth's average surface temperature by between 3.0 and 5.5 degrees C. Such a change would be unprecedented in human history; it would match the five-degree warming since the peak of the last ice age 18,000 years ago but would take effect between 10 and 100 times faster.

The shortcomings of computer models limit the reliability of such forecasts. Many processes that affect global climate are simply too small to be seen at the coarse resolution of a model. Such climatically important processes as atmospheric turbulence, precipitation and cloud formation take place on a scale not of hundreds of kilometers (the scale of the grid in a GCM) but of a few kilometers or less. Since such processes cannot be simulated directly, modelers must find a way of relating them to variables that can be simulated on the model's coarse scale. They do so by developing a parameter—a proportionality coefficient—that relates, say, the average cloudiness within a grid cell to the average humidity and temperature (something the model can calculate).

This strategy, known as parameterization, has the effect of aggregating small-scale phenomena that could act as feedbacks on climatic change, either amplifying or moderating it. Clouds, for example, reflect sunlight back to outer space (tending to cool the climate) and also absorb infrared radiation from the earth (tending to warm it). Which effect dominates depends on the clouds' brightness, height, distribution and extent. Recent satellite measurements have confirmed two-decade-old calculations showing that clouds currently have a net cooling effect; the earth as a whole would be much warmer under cloudless skies. But climatic change might cause incremental changes in cloud characteristics, altering the nature and amount of the feedback. Present models, crudely reproducing only average cloudiness, can say little that is reliable about cloud feedback—or about the many other feedbacks that depend on parameterized processes.

Another shortcoming of present models is their crude treatment of the oceans. The oceans exert potent effects on the present climate and will surely influence climates to come. Their enormous thermal mass will act as a "thermal sponge," slowing any initial increase in global temperature while the oceans themselves warm up. The magnitude of the effect will depend on ocean circulation, which in turn may change as the earth warms. In principle, a climate model should couple a simulated atmosphere with oceans whose dynamics are simulated in equal detail. The computational challenge is staggering, however, and in most GCM's applied to greenhouse warming the dynamics of the oceans are simplified, treated at coarse resolution or left out.

In addition to limiting the reliability of global forecasts, the simplified treatment of the oceans also prevents the models from giving a definitive picture of how climate will change over time in specific regions. Ideally one would like to know not only how much the world as a whole will warm but also whether it will, say, get drier in Iowa, wetter in India or more humid in New York City. Yet, as long as the oceans are out of equilibrium with the atmosphere, their thermal effects will be felt differently at different places. An area in which there is little mixing between surface waters and cold, deep waters might warm quickly; high-latitude regions where deep water is mixed up to the surface might warm more slowly. These thermal effects could in turn affect wind patterns, thereby altering other regional variables, including humidity and rainfall. (Regional forecasts are also compromised in many models by simplified representations of vegetation, which ignore climatically important processes such as the release of water vapor by plants and their effect on surface albedo, or reflectiveness.)

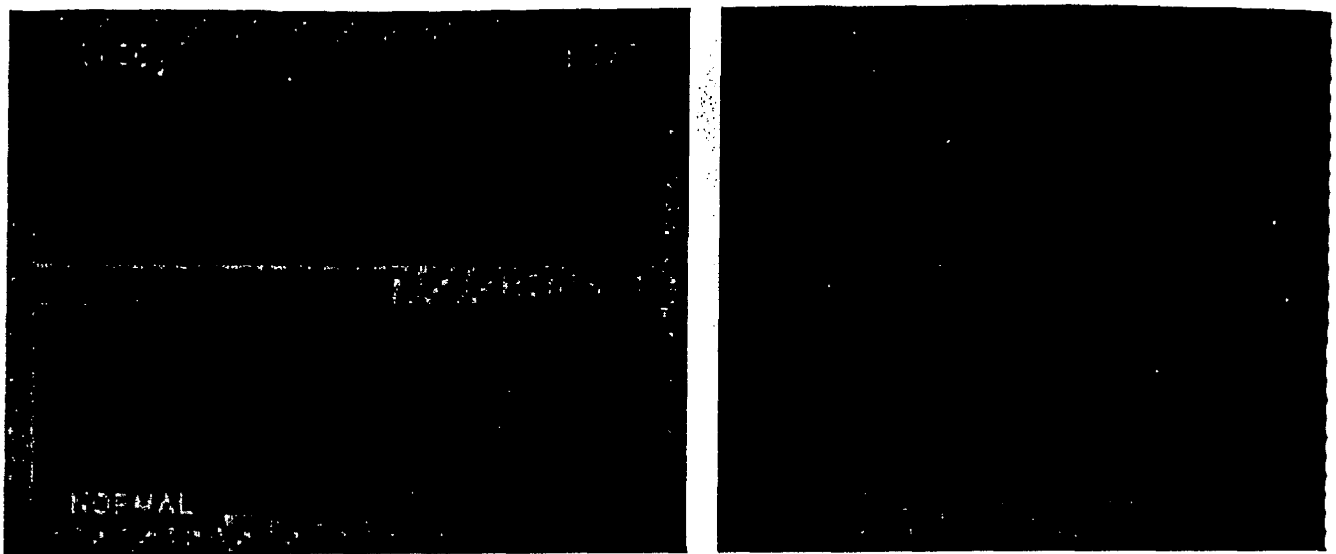


ICE CORE—a segment of a two-kilometer core drilled from the Antarctic ice sheet at the Soviet Union's Vostok Station—contains trapped bubbles of ancient air. Analysis of the bubbles and of the ratio of hydrogen isotopes in the ice, which varies with local temperature, enabled Claude Lorius and his colleagues at the Laboratory of Glaciology and Geophysics of the Environment, near Grenoble, to reconstruct a 160,000-year record of trace gases and temperature (*see top illustration on opposite page*).

Nevertheless, climatologists have grounds for considerable confidence in their models' forecasts of global surface-temperature change. Individual model elements can be verified by comparing them with the results of a more detailed submodel—a smaller, finer-scale simulation—or with real data. Cloud parameterizations, for example, can be tested against actual measurements of the relation of temperature and humidity to cloudiness within an area corresponding to a cell in the model.

The skill of a model as a whole, and in particular its ability to account for relatively fast processes, such as changes in atmospheric circulation or average cloudiness, can be verified by checking its ability to reproduce the seasonal cycle—a twice-yearly change in hemispheric climate that is larger than any projected greenhouse warming. In spite of parameterization, most GCM's map the seasonal cycle of surface temperature quite well, but their ability to simulate seasonal changes in other climatic variables, including precipitation and relative humidity, has not been studied as thoroughly.

During the course of decades (the expected time scale for unmistakable global warming), other, slower processes that do not affect the seasonal cycle come into play: changes in ocean currents or in the extent of glaciers, for instance. Simulations of past climates—the ice ages or the Mesozoic hothouse—serve as a good check on the long-term accuracy of climate



SNAPSHOTS OF A GREENHOUSE WORLD come from a climate model used by the author and Starley L. Thompson at the National Center for Atmospheric Research. The model traced surface temperatures over the year for an atmosphere with twice the present level of carbon dioxide (*top*); the findings were compared with the results of a yearlong simulation for

models. To such tests of overall validity can be added simulations of the climates of other planets, such as Venus, where a dense greenhouse atmosphere maintains a surface temperature of about 450 degrees C.

The record of the past 100 years provides the only direct test of the models' ability to simulate the effects of the ongoing greenhouse-gas increase. When a climate model is run for an atmosphere with the composition of 100 years ago and then run again for the historical 25 percent increase in carbon dioxide and doubling in methane, does it "predict" the observed half-degree warming? Actually most models yield a somewhat larger warming, of at least a degree.

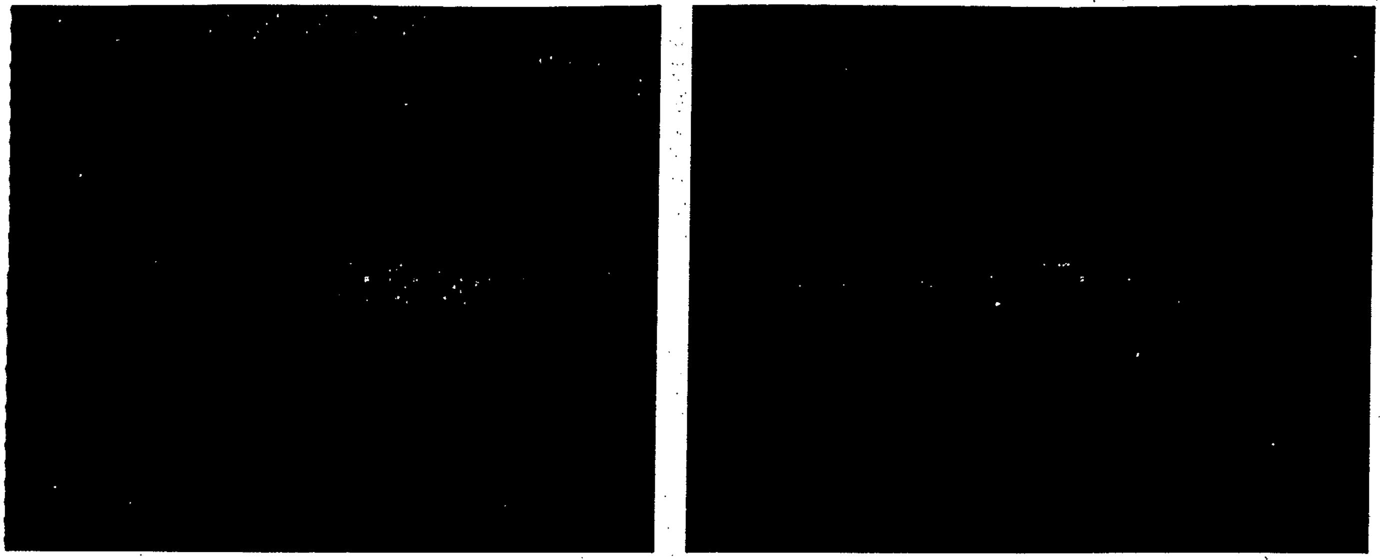
If the observed temperature increase really is a greenhouse warming and not just "noise"—a random fluctuation—one might account for the disparity in various ways. Perhaps the models are simply twice too sensitive to small increases in greenhouse gases, or perhaps the incomplete and inhomogeneous network of thermometers has underestimated the global warming. Conceivably some other factor, not well accounted for in the models, is delaying or counteracting the warming. It might be that the heat capacity of the oceans is larger than current models calculate, that the sun's output has declined slightly or that volcanoes have injected more dust into the stratosphere than is currently known, thereby reducing the solar energy reaching the ground.

It may be significant that the transient cooling interrupting the warming trend began around 1940 and was most pronounced in the Northern Hemisphere, coinciding in time and place with a sharp increase in emissions of sulfur from coal- and oil-burning factories and power plants. The sulfur, a major cause of acid rain, is emitted as a gas, sulfur dioxide, but is transformed into fine sulfate particles once in the atmosphere. The particles can travel long distances and serve as condensation nuclei for the formation of cloud droplets, and so they may make some clouds denser and brighter, increasing their cooling effects. In addition, if no soot is bound to the sulfate, it forms a reflective haze even in cloudless skies. Sulfur emissions could be one factor that has held a greenhouse warming down somewhat in the Northern Hemisphere, especially since World War II.

The discrepancy between the predicted warming and what has been seen so far keeps most climatologists from saying with great certainty (99 percent confidence, say) that the greenhouse warming has already taken hold. Yet the discrepancy is small enough, the models are well enough validated and other evidence of greenhouse-gas effects on climate is strong enough, so that most of us believe that the increases in average surface temperature predicted by the models for the next 50 years or so are probably valid within a rough factor of two. (By "probably" I mean it is a better-than-even bet.) Within a decade or so, warming of the predicted magnitude should be clearly evident, even in the noisy global temperature record. But waiting for such conclusive, direct evidence is not a cost-free proposition: by then the world will already be committed to greater climatic change than it would be if action were taken now to slow the buildup of greenhouse gases. Of course, whether or not to act is a value judgment, not a scientific issue.

Why worry about changes in climate on the scale predicted by the models? Changes in temperature and precipitation could threaten natural ecosystems, agricultural production and human settlement patterns. Particular forest types, for example, grow in geographic zones defined largely by temperature. The belt of spruce and fir that now spans Canada grew far to the south at the end of the last ice age 10,000 years ago, hugging the edge of the ice sheet. As the climate warmed by one or two degrees every 1,000 years and the ice retreated, the forest belt migrated northward, at perhaps one kilometer a year. Forests probably could not sustain the much faster migration required by the projected warming, and many ecosystems cannot migrate in any case: they exist only in preserves, which might become marooned in a newly inhospitable climate zone.

Human activities could be affected directly if a warming speeded the evaporation of moisture, reducing stream runoff; in the western U.S. a temperature increase of several degrees C could decrease runoff in the Colorado basin substantially even if precipitation held steady. As water ran short, faster evaporation would in-



the present atmosphere (*bottom*). The red areas were more than six degrees C warmer than the model-calculated normal for that time of year under existing conditions; the light blue areas were more than six degrees colder. The weather anomalies steadily changed position, shape and size, but heating always predominated in the greenhouse simulation.

crease the demand for irrigation, adding to the strain on water supplies. At the same time, water quality might suffer as the same waste volume was diluted in lower stream volumes.

What is more, several climate models predict that summer precipitation will actually decline in midcontinental areas, including the central plains of the U.S. The late Dean F. Peterson, Jr., of Utah State University and Andrew A. Keller of Keller-Bliesner Engineering in Logan, Utah, estimated the effects on crop production of a three-degree warming combined with a 10 percent drop in precipitation. They found that based on increased crop water needs and a reduction in available water, the viable acreage in arid regions of the western states and the Great Plains would fall by nearly a third. (A western drying might also result in an increased frequency of wildfires.)

Coastal areas, meanwhile, might face a rise in sea level. Most workers expect a global temperature increase of a few degrees C over the next 50 or 100 years to raise sea level by between .2 and 1.5 meters as a result of the thermal expansion of the oceans, the melting of mountain glaciers and the possible retreat of the Greenland ice sheet's southern margins. (Ice could actually build up in Antarctica owing to warmer winters, which would probably increase snowfall.) The rising sea would endanger coastal settlements and ecosystems and might contaminate groundwater supplies with salt. In spite of many local factors that make it difficult to isolate a consistent global signal, one group of workers recently claimed to have found a uniform worldwide rise in sea level of about two millimeters a year in long-term tide-gauge records. That rise is somewhat larger, however, than one would have expected from the warming seen so far.

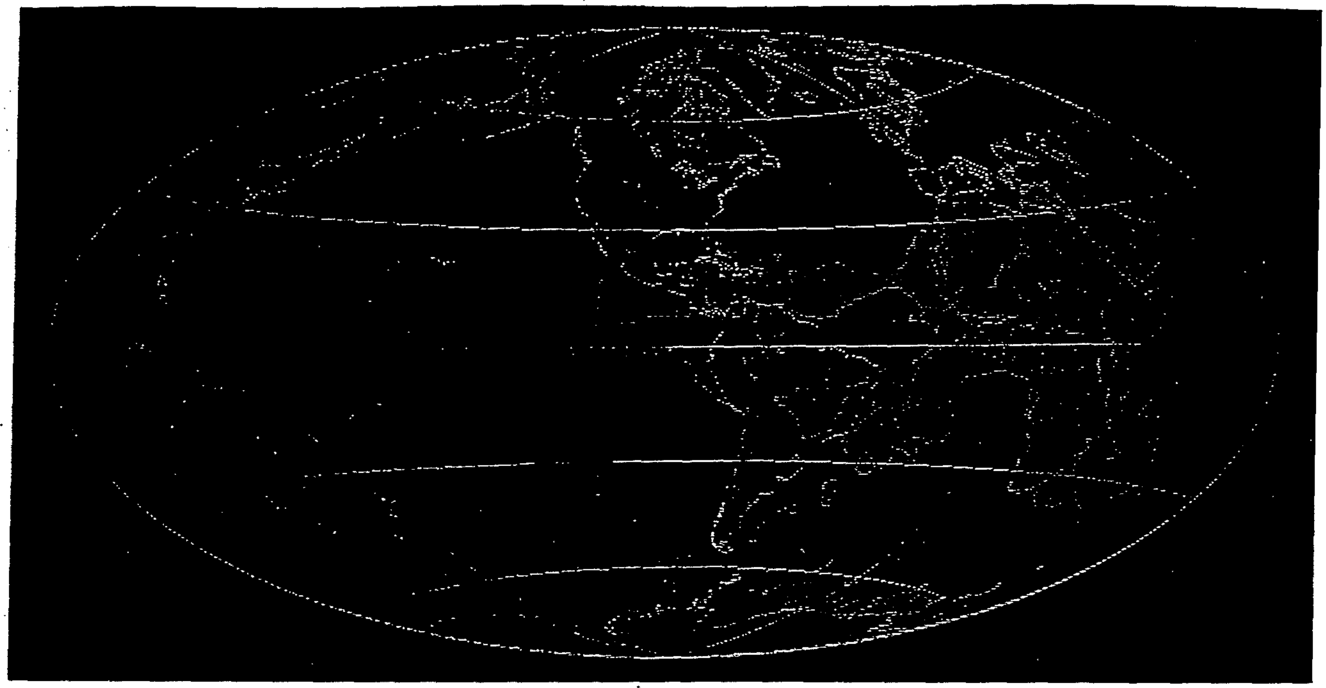
Clearly these direct effects of climatic change would have powerful economic, social and political consequences. A decline in agricultural productivity in the Middle West and Great Plains, for example, could be disastrous for farmers and the U.S. economy. By cutting into the U.S. grain surplus, it might also have serious implications for international security.

To be sure, not everyone would lose. If the corn belt simply moved north by several hundred kilometers, for example, Iowa's billion-dollar loss could become Minnesota's billion-dollar gain. But how could the losers be compensated and the winners charged? The issue of equity would become still more thorny if it spanned borders—if the release of greenhouse gases by the economic activities of one country or group of countries did disproportionate harm to other countries whose activities had contributed less to the buildup.

In the face of this array of threats, three kinds of responses could be considered. First, some workers have proposed technical measures to counteract climatic change—deliberately spreading dust in the upper atmosphere to reflect sunlight, for instance. Yet if unplanned climatic changes themselves cannot be predicted with certainty, the effects of such countermeasures would be still more unpredictable. Such "technical fixes" would run a real risk of misfiring—or of being blamed for any unfavorable climatic fluctuations that took place at the same time.

Many economists tend to favor a second class of action: adaptation, often with little or no attempt to anticipate damages or prevent climatic change. Adaptive strategists argue that the large uncertainties in climate projections make it unwise to spend large sums trying to avert outcomes that may never materialize. They argue that adaptation, in contrast, is cheap: the infrastructure that would have to be modified in the face of climatic change—such as water-supply systems and coastal structures—will have to be replaced in any case before large climatic changes are due to appear. The infrastructure can simply be rebuilt as needed to cope with the changing environment.

Passive adaptation relies mostly on reacting to events as they unfold, but some active adaptive steps could be taken now to make future accommodation easier. An American Association for the Advancement of Science panel on climatic change made a strong, potentially controversial but, I believe, compelling suggestion for active adaptation: governments at all levels should reexamine the technical features of water systems and the economic and legal aspects of water-supply management in order to increase the systems' efficiency and flexibility. As the climate warms and precipita-



CLOUDS AFFECT SURFACE TEMPERATURES because they both reflect sunlight, preventing it from warming the earth, and absorb infrared radiation from the surface, contributing to the greenhouse effect. In this image, based on satellite data gathered in April, 1985, clouds had a net cooling effect in some regions (*blues and green*) and a heating effect in others (*red*). On the whole, clouds cool the planet more than they warm it, but the characteristics of clouds and their effect on climate might change unpredictably in a greenhouse world. The image was provided by V. Ramanathan of the University of Chicago.

tion and runoff change, water shortages may grow more common and needs for regional transfers more complex. Even if climate did not change, more flexible water systems would make it easier to cope with the normal extremes of weather.

The third and most active category of response is prevention: curtailing the greenhouse-gas buildup. Energy-conservation measures, alternative energy sources or a switch from coal to natural gas and other fuels with a lower carbon content could all reduce carbon dioxide emissions, as could a halt to deforestation. Stopping the production of CFC's, already notorious because of their ability to erode the stratospheric ozone layer, would eliminate another component of the buildup. A far-reaching proposal for an international framework for reducing emissions was put forward in 1976 by Margaret Mead and William W. Kellogg of NCAR: a "law of the air," which would keep emissions of carbon dioxide below a global standard by assigning polluting rights to each nation.

Proposals for immediate action are controversial because they often entail large immediate investments as insurance against future events whose details are far from certain. Is there some simple principle that can help us to choose which preventive or adaptive measures to spend our resources on? I believe it makes sense to take actions that will yield "tie-in" benefits even if climatic changes do not materialize as forecast.

Pursuing energy efficiency is a good example of this tie-in strategy. More efficient fossil-fuel use will slow the carbon dioxide buildup, but even if the sensitivity of climate to carbon dioxide has been overstated, what would be wasted by taking this step? Efficiency usually makes economic sense, and a reduction in fossil-fuel use would curb acid rain and urban air pollution and lessen the dependence of many countries on foreign producers. Developing alternative energy sources, revising water laws, searching for drought-resistant crop strains, negotiating international agreements on trade in food and other climate-sensitive goods—all these steps could also offer widespread benefits even in the absence of any climatic change.

Often such steps will nonetheless be costly and politically controversial. Regulations or incentives to foster energy-efficient technologies might burden some groups—coal miners and the poor, perhaps—more than others, and the costs may be proportionally greater for poor countries than for rich ones. Actions to prevent a greenhouse warming will have to be coupled with domestic- and foreign-policy measures that attempt to balance fairness and effectiveness. Still, I believe it is better to fight poverty and foster development through direct investment rather than through artificially low energy prices that neglect the costs of the resulting environmental disruptions.

Some people argue that the free market, not government regulation or tax incentives, should dictate increases in energy efficiency, say, or the elimination of CFC's. But it cannot be logically argued that the market is "free" when it does not include some of the potential costs of environmental damage caused by goods or services. Moreover, even political conservatives agree that an economic calculus must give way to a strategic consciousness when national or global security is at stake.

Security is indeed at stake here, as the implications of a global temperature rise of several degrees or more over the next century make clear. Adding to the predicted threats are surprises that may be lurking in the greenhouse century: a sharp positive feedback in the greenhouse-gas buildup from accelerated decay of soil organic matter, dramatic changes in regional climates because of a shift in ocean circulation, or the outbreak of new diseases or agricultural pests as ecosystems are disrupted. In my value system—and this is a political and not a scientific judgment—effective tie-in actions are long overdue.

I am often asked whether I am pessimistic because it will be impossible to avert some global change: at this stage, it appears, no plausible policies are likely to prevent the world from warming by a degree or two. Actually I see a positive aspect: the possibility that a slight but manifest global warming, coupled with the larger threat forecast in computer models, may catalyze international cooperation to achieve environmentally sustainable development, marked by a stabilized population and the proliferation of energy-efficient and environmentally safe technologies. A much larger greenhouse warming (together with many other environmental disruptions) might thereby be averted.

The developed world might have to invest hundreds of billions of dollars every year for many decades, both at home and in financial and technical assistance to developing nations, to achieve a stabilized and sustainable world. It is easy to be pessimistic about the prospects for an international initiative of this scale, but not long ago a massive disengagement of NATO and Warsaw Pact forces in Europe also seemed inconceivable. Disengagement now seems to me to be possible, even likely. Perhaps the resources such an agreement would free and the model of international cooperation it would provide could open the way to a world in which the greenhouse century exists only in the microchips of a supercomputer.

FURTHER READING

THE GREENHOUSE EFFECT, CLIMATIC CHANGE, AND ECOSYSTEMS. Edited by Bert Bolin, B. R. Döös, Jill Jäger and Richard A. Warrick. John Wiley & Sons, 1986.

AN INTRODUCTION TO THREE-DIMENSIONAL CLIMATE MODELING. Warren M. Washington and Claire L. Parkinson. University Science, 1986.

CLOUD-RADIATIVE FORCING AND CLIMATE: RESULTS FROM THE EARTH RADIATION BUDGET EXPERIMENT. V. Ramanathan et al. in *Science*, Vol. 243, No. 4887, pages 57–63; January 6, 1989.

GLOBAL WARMING: ARE WE ENTERING THE GREENHOUSE CENTURY? Stephen H. Schneider. Sierra Club Books, 1989.

POSSIBLE CLIMATE CHANGE DUE TO SO_2-DERIVED CLOUD CONDENSATION NUCLEI. T. M. L. Wigley in *Nature*, Vol. 339, No. 6223, pages 365–367; June 1, 1989.

Conservation Law Foundation
Docket No. 5330
Exhibit CLF-PLC-R7

PLC, Inc.

Critique of ESRG Report: Valuation of Externalities

M E M O R A N D U M

TO: Hydro Quebec File

FROM: Paul Chernick
Emily Caverhill

DATE: February 5, 1990

RE: CRITIQUE OF ESRG REPORT: VALUATION OF EXTERNALITIES

We have reviewed Chapter 9 of the ESRG report, and have the following comments on their treatment of externalities:

1. Valuation of Externalities

ESRG does not appear to be valuing externalities at all, as the chapter titles would suggest. Indeed in a 19/1/90 revision to chapter 9 provided to us by ESRG, it states:

> "We have not used damage cost estimates nor have we used abatement costs as a surrogate for damage costs; thus, we have not chosen to value externalities as such." (emphasis in the original).[1]

2. Use of Abatement Costs

ESRG (1989) appears to prefer to use abatement costs as a measure of the cost of internalizing pollution control, rather than as a measure of the value of an externality. Since this is the case, We do not think ESRG is implying that average abatement costs have any relation to the value of the externality. In their report, ESRG does not make this distinction explicit; however, their revised version of Chapter 9 seems to make this distinction. Indeed, average costs have little to do with the valuation of externalities; it is the marginal abatement cost which is important.[2]

The use of marginal versus average abatement costs is important to the extent that ESRG is using its results (which are based on average costs) to recommend a supply plan for Vermont. First, as We understand it, the valuation of externalities will be used primarily for the comparison of DSM programs to marginal supply options; the important externalities are the externalities of the marginal supply option since they are the externalities

---

[1]ESRG 19/1/90 revision of Chapter 9, page 9-13.

[2]See Chernick and Caverhill (1989) for a detailed explanation.

avoided if the DSM program is adopted. Second, the cost of control (or cost of abatement as ESRG prefers to call it) can be used as a surrogate for the direct cost of the emissions if one makes the assumption that societal preference for emissions reduction is implied through regulations requiring control equipment. Here, the relevant cost is the most expensive cost society has demonstrated it will pay (the marginal cost).

3. Analysis of Direct Costing Studies.

ESRG made some errors in its interpretation of the direct costing studies it reviewed, which appear to have influenced its assertion that direct costing studies could not be used for valuing externalities. Based on those results, ESRG concludes that the direct cost estimates have too much variability and uncertainty to be useful. Direct costing reports do have variability in the results; but much of the variability is reconcilable if the studies are corrected to a comparable basis. Further, the direct costing studies suggest much higher values for externalities than ESRG concludes are useful in Vermont supply planning.

ESRG presents a fairly detailed literature search of direct costing studies which appears at various points to have overlooked several of the obvious differences in the reports. Each of the reports reviewed by ESRG provide power plant or regional air emissions estimates and estimates of the damages due to those emissions. While none of the studies were designed to calculate a cost/lb pollutant, this figure can be derived from the information given in the reports, as noted by ESRG. However, in order to correctly compare the results of the reports, the emissions and valuations assumptions and results must be adjusted to reflect a consistent basis. The spread in the results of the various studies as presented by ESRG is very wide; however, this spread is dramatically reduced when adjustments are made to bring the studies onto a comparable basis. The necessary adjustments include:

1. Accounting for differences in value judgments. For instance, the studies reviewed use different values for a lost life, lost day of health, or degraded visibility. These differences stem from different assumptions about the proper factors to include in this valuation which can include lost productivity, medical costs, and pain and suffering.

2. Resolving differences in input information. The studies reviewed by ESRG have different bases, such as different sizes of the affected population, crops, etc.

The studies also use different methods of estimating materials damage, extent of degraded visibility and other damage.

3. Resolving differences in the interpretation of input information. For instance, the handling of impacts which are not easily quantified, such as assuming a value of zero for the effects on wildlife or other effects.

ESRG makes vague references to the differences in the studies in its report. However, ESRG does not appear to reconcile the differences in the studies even though this reconciliation is critical for comparison of the results to one another.[3]

ESRG reviewed the direct costing studies: Hohmeyer (1988), and ECO (1984, 1986, 1987). We will briefly outline the reconciliations required for each report in order to present the results of the reports on a comparable basis. We have not reconciled the reports figures in this analysis; an analysis of this type is presented in Chernick and Caverhill (1989).

Hohmeyer (1988) provides summary total costs of pollution in West Germany on a number of systems (crops, forests, human health and materials). Hohmeyer allocates the costs between several pollutants on the basis of their relative "MAK" values, which are apparently a measure of the pollutants relative toxicity to humans.[4] He assumes that the value of a pound of emissions of SO_2, particulates, and VOCs have the same impact or value, and that NO_x is worth 25% more per pound. ESRG estimates a cost/lb for some pollutants by assuming their relative toxicities (as represented by their MAK values) approximate their relative contribution to the total damage, and comparing these figures to pollutant emissions in West Germany (also provided by Hohmeyer). These results are only useful to valuing individual pollutants to

---

[3]ESRG (1989) is a summary document, which provides only a brief description of its approach, and no derivation of most of the numerical results. I am necessarily critiquing what ESRG appeared to do to derive its results.

[4]Hohmeyer's method breaks down completely on global warming, for which he is unable to identify any costs affecting the DBR, other than the cost of raising dikes. Some other costs could probably be quantified, but most of the effects of global warming are difficult or impossible to value in monetary terms.

the extent that these assumptions are acceptable.[5] Even given its limitations, this method is acceptable for estimation of the cost/lb of pollutants in West Germany.

In order to transfer the results for use in the United States and New England, several adjustments must be made. Hohmeyer appears to use values for loss of human life and for illness which are lower than recent estimates used in the US; he appears to include only losses in work-related productivity which ignore increased medical and "psycho-social" costs,[6] and the value of illness and death of non-working adults, children and the elderly. Hohmeyer's assumptions are imbedded in his background sources for externalities associated with fossil fuels. Grupp (1986) cites values of DM100,000 to DM1 million, or approximately $40,000 to $400,000, for the value of a life, but appears not to use this value. Instead, he values lives based on loss of productivity of $50,000 DM/year, and includes working and non-working humans. We cannot determine from his paper his value per life, as such, but it is understated because it excludes medical costs and pain and suffering. Hohmeyer uses values of about $500,000/death and $250,000/illness (where the average illness prevents 10 years of employment) in the nuclear externalities section of his report, also based only on productivity loss. Hohmeyer recognizes that his results are understated; ESRG also points out that Hohmeyer's costs are understated, but still compares Hohmeyer's results to those of studies using values for mortality which are several times higher without noting the obvious differences.

The ESRG study also carelessly combines inconsistent estimates within the Hohmeyer (1988) study. Hohmeyer presents tables of emissions for the electric utilities plus district heating (which comes to 34% of total emissions, weighted by his assumed toxicities), and of damages just from electric utilities, which he estimates to be 28% of the total. ESRG appears to have divided 28% of total damages by 34% of total emissions, and produced results 18% lower than Hohmeyer's data would indicate. The 18% difference is not as critical as other problems with ESRG's interpretation of Hohmeyer's results.

---

[5]It is not clear whether the sources on which he relied made the same assumptions regarding the relative toxicities of pollutants, or indeed whether those sources directly estimated aggregate pollutant effects.

[6]"Psycho-social" costs are pain, suffering, dread and the like.

ESRG also reviewed three of ECO Northwest direct cost studies (ECO, 1984, 1986, 1987). ECO performed the series of studies for the Bonneville Power Administration, to estimate and value the environmental effects of combustion turbine, pulverized coal, and other electricity generation technologies. ESRG used the emission and cost figures in these studies to estimate direct cost/lb of pollutant; ESRG's analysis appears to be simplistic and did not reveal the underlying commonalities and differences in the studies, nor the obvious factors causing most of the difference in the estimates calculated from the studies' data.

Chernick and Caverhill (1989) points out the major differences between the respective ECO reports and adjusts them to a level basis in order to estimate a cost/lb for SO_2, NO_x and particulates.[7] These differences include:

a. Size of the affected population. Damages to human health, materials and visibility vary directly with population density (as they are generally calculated) so the size of the affected population has a significant impact on total damage estimates. This is especially true currently because the damages which are independent of population or vary inversely with population density (e.g., damages to wildlife) are poorly understood and generally given very low or zero value. Some of the ECO studies were performed for sites in Eastern Washington or the Rockies, with population densities as low as in virtually any part of the lower 48 states. Even the most densely settled areas studied (in ECO 1984 and part of ECO 1987) have significantly lower population densities than most sites in the Northeast.[8]

b. Value judgments. The ECO studies used different assumptions regarding the value of human health, with the 1984 study assuming \$0.3 - 1.0 million per life and \$75/lost work day and the 1986 and 1987 studies a more reasonable \$3 million, but excluding all morbidity costs.[9]

---

[7] CO_2 is also mentioned in this section, but the ECO reports do not address CO_2.

[8] See Chernick and Caverhill (1989) for a description of how the different damages are generally estimated, and for a discussion of the population densities.

[9] ECO (1987) assumed that the estimates it used, which were taken from wage risk differentials, included the value of morbidity. This assumption is methodologically suspect, since it assumes that the ratio of morbidities to mortalities is the same for occupational hazards and for air pollution-induced illness.

c. Excluded effects. The 1987 study appears to carefully estimate the health effects from the generic coal plant from first principles, but it basically excludes effects for visibility and ecosystems. On the other hand, the 1984 study uses a much more sophisticated analysis of visibility effects. As a result, even for fairly low-population areas in the Northeast, a portion of the value from the 1984 study (almost half of which are due to reduction in visibility) should be added to the results from the 1987 study for the high-population case (which are almost all human health effects) in order to approximate the total direct costs. This estimate might still understate potential impacts on wildlife. The value for high-population areas in the Northeast would be much higher than even the high-density case in the 1987 report.

d. Use of the reported data. The summary emissions table in the 1987 study reports two emissions estimates in grams/second and tons/year which do not agree; the grams/second estimate is taken directly from a consultants report attached to the ECO report as an appendix, and the tons/year should have been derived from this estimate (and based on the capacity factor of the plant). The resulting tons per year are in the millions of tons per year range, not in the units of tons reported. ESRG did not notice this oversight, and instead used the wrong figure to estimate a totally implausible value for the cost/lb of pollutant. ECO (1987) is widely viewed as one of the best direct cost studies in the US, and one of few in existence; the careful analysis of its estimates is important to a direct cost analysis.[10]

The ESRG report did not discuss the reasons for the differences in the results of the ECO studies, and treated the 1984 and 1987 studies as alternative estimates; these studies are more accurately characterized as estimates of two different effects of air pollution, health and visibility. Even if the methodologies used in these reports to estimate the direct effects were completely comparable, the differences in valuation of health effects would still have to be reconciled between the two reports in order to compare the results. ESRG did not make either of these corrections. ESRG also described the lowest-

[10]ESRG appears to discard this ECO (1987) result from the western Washington plant as an "outlier." The value ESRG discarded was certainly implausible. However, unusual results in modelling studies can usually be explained as unusual valuation decisions or interpretation errors, as was the case here.

density and highest-density results in ECO (1987) as if they were alternative estimates. These estimates are not directly comparable, rather they were clearly intended to be different estimates based on different (and known) population densities.[11] Finally, since we are most interested in a representative value for New England and not the Northwest, even the highest-density case results of the ECO (1987) study are too low for most of our region, considering the relative population densities surrounding many proposed generating facilities.[12]

4. ESRG Abatement Cost Estimates

ESRG Table 9.2 presents a range of estimates of abatement and damage costs. ESRG uses the appendices to Chapter 9 to support these figures; however, moving between Table 9.2 and the appendices is difficult due to cursory presentation of the studies' assumptions and results. For instance, some of the figures which appear in the appendices are not represented in the ranges presented in Table 9.2, with no justification for their exclusion. Certainly, even the high figures presented in Table 9.2 are lower than the marginal costs of abatement (as identified in Chernick and Caverhill, 1989). ESRG does not discuss the figures in Table 9.2 in enough detail for us to determine exactly where the high and low estimates came from, but some of the figures appear low. For instance, the reported high abatement cost for CO_2 should represent the cost of CO_2 scrubbers, which is in the range of 10 to 25 cents per pound carbon, or closer to \$55/ton CO_2; ESRG's high figure does not even reflect their own estimate (cited from NYSEP) of CO_2 scrubbing technology at \$28.11/ton CO_2 and is closer to an average cost of planting trees on the margin.[13] By design, this makes ESRG's average cost of

[11]The lowest-density case shows a slight increase in crop damage over the high-density case, but this effect is completely swamped by the magnitude of the population-related effects discussed above. Ecosystem damages are very low, and are the same for all cases.

[12]The problems discussed above affected ESRG's recommendations only to the extent that ESRG concludes that direct costing is not feasible at this time.

[13]ESRG admits that the NYSEP CO_2 scrubber cost is too low in that it ignores operating costs which are likely to be substantial; this figure also appears to ignore the extra energy required to run the scrubbing equipment and the CO_2 emitted in generating that energy.

CO_2 abatement (the one used in analyzing the environmental effects of the different cases) too low.

The low values presented for CO_2 appear to be based on studies which use resources unavailable for CO_2 sequestration. For example, ESRG cites BPA (actually Buchanan, 1989) from which ESRG estimates a cost of \$10.71/ton CO_2. Buchanan uses the cost of trees which are currently planted for lumber, not future more expensive plantings. Thus, his value is at the low end of the plausible range. ESRG also uses tree planting-costs allegedly from EDF (this may be ESRG's low-end estimate). The actual EDF analysis (Dudek, 1989) assumes a planting cost at the low end of the plausible range, but very high land-rental costs, bringing the levelized cost to \$7.90 to \$11.88/ton (for ESRG's estimate). Thus, the source of one of ESRG's "low" estimates actually generates a "high" estimate when all costs are accounted for. Dudek's estimates of costs and sequestration rates are also optimistic. See Chernick and Caverhill (1989) for an analysis of tree planting costs and an initial estimate of CO_2 scrubber costs.

Since the values for methane and N_2O are both dependent on the value for CO_2, the high-end methane and N_2O values should reflect this adjusted high CO_2 value. Further, ESRG has not yet responded to discovery on its source document for its estimates of the relative contributions of methane and N_2O to the greenhouse effect, which are significantly lower than EPA (1989) reports. If this additional adjustment is made, the per-ton values for methane and N_2O would increase.

The high values for SO_2 and NO_x presented in Table 9.2 appear to be reasonable estimates of the high cost control measures for those pollutants, and they agree with the very conservative estimates of marginal control costs presented in Chernick and Caverhill (1989). However, the high values do not represent the marginal cost of control represented by moving between low cost and high cost controls.

The values ESRG uses for CO_2, SO_2 and NO_x appear to be very important to the differential environmental effects. For example, comparing the figures in the worksheets for the base load cases MINBGLFO (min HQ) and 0HQBGLFO (no HQ), 98% of the difference in the environmental costs is due to three pollutants: SO_2 (56%), CO_2 (28%), and NO_x (14%).[14] If the values of these

[14]These figures were taken from workpapers received on disk through discovery. The figures in these workpapers did not appear to agree exactly with the final results filed by ESRG; however, the total figures were on the same magnitude.

pollutants were adjusted to reflect conservative estimates of the marginal cost of abatement, that is, the SO_2 value increased by 4 times, the CO_2 value by 3 times, and the NO_x value by 20%, then the differential environmental cost between these two cases would more than double.

5. Use of the Results: The Environmental Credit

ESRG Table 3.3 lists an "Environmental Credit" for residential and commercial DSM programs. The original Table 3.3 used a uniform 9% credit, while the "revised" table shows different values, and a range of environmental credit percentages for different programs. A derivation of these values was not provided making review difficult.