#4

COMMONWEALTH OF MASSACHUSETTS
DEPARTMENT OF PUBLIC UTILITIES

Re: Boston Edison Company's Construction Program and Capacity Needs, D.P.U. 19494

JOINT TESTIMONY OF PAUL L. CHERNICK AND SUSAN C. GELLER ON BEHALF OF THE ATTORNEY GENERAL

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Q: Have you testified previously as an expert witness?

A: Yes. I have testified before the Massachusetts Energy Facilities Siting Council and the Massachusetts Department of Public Utilities in the joint proceeding of Boston Edison's forecast, docketed by the E.F.S.C. as 78-12 and by the D.P.U. as 19494, Phase I. I also testified before the E.F.S.C. in proceeding 78-17, on Northeast Utilities' forecast, and in proceeding 78-33, on Eastern Utilities Associates' forecast.

Q: Ms. Geller, would you please state your name, position and office address.

A: My name is Susan Geller. I am employed by the Attorney General as a Utility Rate Analyst. My office is at One Ashburton Place, 19th Floor, Boston, Massachusetts, 02108.

Q: Please briefly describe your professional education and experience.

A: I graduated from Harvard University in June, 1974, with a B.A., magna cum laude, in Economics. In addition, I have a Master's Degree in Public Policy from the John F. Kennedy School of Government, Harvard University, and I have completed the course requirements and passed the qualifying examinations for the Ph.D. in Public Policy. My work experience includes:

I. INTRODUCTION

Q: Mr. Chernick, would you please state your name, position, and office address.

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A: I received a S.B. degree from the Massachusetts Institute of Technology in June, 1974 in Civil Engineering and a S.M. degree from the same school in February, 1978 in Technology and Policy. I have been elected to membership in the civil engineering honorary society Chi Epsilon, to membership in the engineering honorary society Tau Beta Pi, and to assicate membership in the research honorary society Sigma Xi. I am the author of Optimal Pricing for Peak Loads and Joint Production: Theory and Applications to Diverse Conditions, Report 77-1, Technology and Policy Program, Massachusetts Institute of Technology. During my graduate education, I was the teaching assistant for courses in systems analysis, for which I prepared course notes and taught classes in regression and other topics in modeling. My resume is attached to the end of this testimony as Appendix A.

Paul's Copy

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1. A summer internship at the Atomic Energy Commission where I collected and analyzed data for the Nuclear Reactor Safety Study (the "Rasmussen Study");

2. A research assistantship at the Harvard Business School where I helped prepare a seminar for business executives and public officials on the problems of producing electric power for New England (summer, 1974);

3. Volunteer consulting for Region I Office of the U.S. Environmental Protection Agency (spring, 1975); and

4. A research assistantship at the Kennedy School of Government, dealing with issues of technological safety (summer, 1975).

My resume is attached to the end of this testimony as Appendix B.

Q: Have you testified previously as an expert witness?

A: Yes. I testified jointly with Paul Chernick in Phase I of D.P.U. 19494 and in E.F.S.C. 78-12. I have also filed expert testimony in two cases before the Massachusetts Energy Facilities Siting Council, in cases involving long-range forecasts of New England Gas and Electric Association (E.F.S.C. 78-4), and Massachusetts Municipal Wholesale Electric Corporation (E.F.S.C. 78-1). One of these cases was decided without full evidentiary hearings; as a result, I was cross-examined only in the NEGEA case.

Q: For the convenience of the Department and the parties, will you please indicate who is responsible for which sections of this testimony.

A: Yes. Susan Geller is the principal author of §III, concerning the NEES forecast, of §VIII, concerning the NEGEA forecast, and of §IX, concerning the MMWEC forecast. Paul Chernick is the principal author of all of the remaining sections.

SECTION II: NORTHEAST UTILITIES

Q: What materials did you review in preparing this portion of your testimony?

A: I reviewed Northeast Utilities' (NU) Long Range Forecast of Electrical Loads and Power Facilities Requirements in Massachusetts, submitted to the E.F.S.C. on December 31, 1977; Electrical Energy Demand 1978-87 (January 1, 1978), which I will refer to as EED; portions of the Supplementary Material Relating to Electrical Energy Demand Forecast; NU's witness' testimony in E.F.S.C. 78-17 and NU's response to 57 Information Requests by the Attorney General in E.F.S.C. 78-17. Equation numbers in this section of the testimony refer to equations in EED; references to "AG-xx" are to NU's information responses.

Q: On what matters will you be testifying?

A: I will be commenting on virtually all major sections of NU's sales forecasting methodology : the economic/ demographic model, the residential model, the commercial model, and the industrial model.

Q: Do you have any general comments on the forecast methodology?

A: Yes, NU's forecasting techniques are generally more ambitious and sophisticated than those of any other electric utility in Massachustts. The extent of data collection, the disaggregation of classes, and the discussion of causal factors is definitely superior to the industry norm. The NU methodology incorporates some considerable conceptual improvements over traditional forecasting techniques. As a result, despite the sheer size of the methodology, it is relatively easy to identify the sections which contain serious flaws. Similarly, the areas which require more extensive documentation are also readily apparent. Thus, NU has not only made considerable progress in the development of a reasonable methodology, but has also produced an interim model which should be fairly amenable to critique and improvement. While most of my testimony will constitute a critique, with suggestions for improvement, this should not obscure the progress that NU has already made.

Q: Do you have any comments on the economic/demographic model?

A: Yes, I would like to comment on the following topics within the economic/demographic model:

1. the specification of the migr~~ant~~tion equations;
2. the specification of the non-manufacturing employment equations;
3. the specification of the manufacturing employment equations;
4. the growth multiplier function; and
5. the cost functions.

Q: What are your comments on the migration equations?

A: I have not been able to conduct a systematic comparison of the results of the alternative migration specifications (Equations 8a, 8b, and 8c) particularly because the output provided in response to question AG-1 is incomplete. However, I have noticed that some of the formulations have the "wrong" sign on the unemployment variable and that formulations with the expected sign often are of low significance. For example, for males 20-24 years old (M20), equation 8b has the right sign, and the t-statistic of .92, while 8a has the wrong sign but a t-statistic of 1.57. Similarly, for cohort F20, eq. 8b gives the right sign and a t of .77, while 8a has the wrong sign, but a t of 1.37. The F-tests and R^2 follow the same pattern: while neither fit is very good, the rejected equation fits better. For cohort F40, the situation is reversed: Eq. 8b is statistically significant, with the wrong sign, while eq. 8a is not significant, but has the expected sign. (The results for M40 are incomplete.)

If both formulations, and especially the more logically appealing 8b, frequently produce "incorrect" results with superior statistical validity, it might be wise to question NU's fundamental assumption that migration is largely a response to unemployment rates. Their own results argue for the importance of other factors.

Furthermore, the low t-statistics indicate that the relationships utilized may well be pure artifacts of chance.

Q: What would you like to say about the specification of the non-manufacturing employment equations?

A: Four aspects of equation 16 seem rather problematic: the derivation of the MIX variable, the validity of NU's application of the MIX weights, the timetrending of employment, and the statistical significance of the denied relations.

The MIX coefficients define the extent to which employment in each commercial category responds to local population (POP) and employment (CTEMP). These weights appear to be the result of NU's judgemental adjustments of Battelle's adjustments of the results of a Department of Commerce Input-Output model. It is not at all clear why the weights were not derived statistically from the Connecticut data NU has so carefully compiled. This would yield results more applicable to the service area; after all, the national mix of demand for commercial services may not be the same as the local mix, demand for some service (e.g., transportation and communications) may tend to fall outside the service area, and the average mix from the input-output model may be different than the marginal mix of recent and future years (in a linear model, this latter effect would result in inclusion of a constant term).

Secondly, it appears that population and employment inputs were not properly defined to be of equivalent magnitude so that the desired portion of the MIX variable is due to each variable. For example, if both POP and CTEMP in AG-5 are in thousands, POP will be more than twice as large as CTEMP and will account for 61% of the construction MIX, rather than the 41% prescribed by NU's estimate. Whether this problem arises can not be determined from the information presented.

Third, the time-trending of the employment ratios have the problems common to all such trending, as well as some special ones. Fundamentally, it is not clear that the factors which produced the historic trend in the data will continue into the future, or that the dependent variable can continue to respond. "Services and government" shows the strongest time trend among the outputs from equation 16 and accounts for 61% of NU's forecast growth in employment. Yet it seems likely that the boom in government activity of the late sixties and early seventies is apt to slow dramatically, at least per unit of population or employment. Also, the generally positive time-trends exaggerate the self-propelling tendency of equation 16: i.e., even if nothing else changed in the economy non-manufacturing employment would increase every year. This effect is the result both of the strong positive time trend and of the recursive nature of the assumption that employment in each year is a monotonically increasing function of employment in the previous year.

Finally, the time-trends for three of the seven non-manufacturing divisions are not statistically significant.

Q: What would you like to say about the manufacturing employment equations?

A: The form of equation 19 indicates the existence of a problem. It is tautological that

$$EMP_{e,t} = (1.0 + LG_{e,t}/100) * EMP_{e,t-1}$$

where $LG_{e,t}$ = local growth rate of employment in category e in year t

Apparently NU's efforts to derive LG were unsucessful, so that they found it expedient to add a factor of [$\alpha_e + \beta_e$ (ln time)] to correct their model to fit 1971-76 data. Since α is generally less than 1 and β is generally negative, this factor represents an increasingly downward correction over time. (Also note that the forecast results for 1976 on the last two pages of the "option summary" in the answer to AG-7 are generally much higher than actual.) This correction apparently barely compensates for the upward and increasing bias introduced by the estimates of LG in a time of recession; it seems unlikely that the correction will be adequate in the future as forecast national growth (NG) increases, forecast local labor cost falls, and as log(time) grows more slowly. Certainly, this approach is less satisfactory than an alternative approach which might attempt to estimate a reliable LG directly from NG, costs, and the like.

It is also not clear how the decision was made to omit time from equation 19 for SIC 20 and SIC 28, nor how the "calibration over the historic period" (question AG-7) was compared between models with and without the cost index.

NU can apparently no longer explain how the decisions were made. From the workpapers supplied, it appears that NU used the cost index (growth multiplier) when it produced superior back-casts (perhaps just for 1976) to the methodology without the index. In the many cases for which neither fit was very good, they then added the time variable. (Some decisions were apparently forced by data limitations.) Again, the approach is less consistent than the simultaneous estimation of all coefficients.

Q: What comments do your have on the growth multiplier ?

A: First, equations 20c and 21 combine to imply the relationships listed in Table NU-I <u>infra</u>. For example, if national growth is negative and costs are much lower locally, then the faster national employment <u>falls</u>, the faster local employment <u>grows</u>. This relationship is definitely counter-intuitive.

Second, the derivation of GM and particularly Φ in equation 21 is most mysterious. NU seems to be using NEPOOL/Batelle results (response to AG-8), but as Figure I <u>infra</u> shows, NEPOOL's curve for southern New England is not the same as the Φ function. NEPOOL'S function seems to be a set of straight lines, derived from a little data and a lot of judgment. How NU derived a non-monotonic fourth-order polynomial eludes me.

Q: What comments would you like to make regarding the cost functions?

A: With respect to labor costs (RLC), the major problems arise with respect to Eq. 23, which adjusts RLC as a function of local

<u>Table NU-I</u>

Local to National Cost Ratio	Relationship between Local Growth and National Growth if NG>0	NG<0
over 1.08	LG=-.1NG	LG=2.1NG
1.07 to 1.08	LG=0	LG-2NG
.92 to .93	LG=2NG	LG=0
under .92	LG=2.1NG	LG=-.1NG

and national unemployment rates. There is no documentation of the coefficients in Eq. 23, either in EED or in the (garbled and incorrect) response to question AG-10. Yet

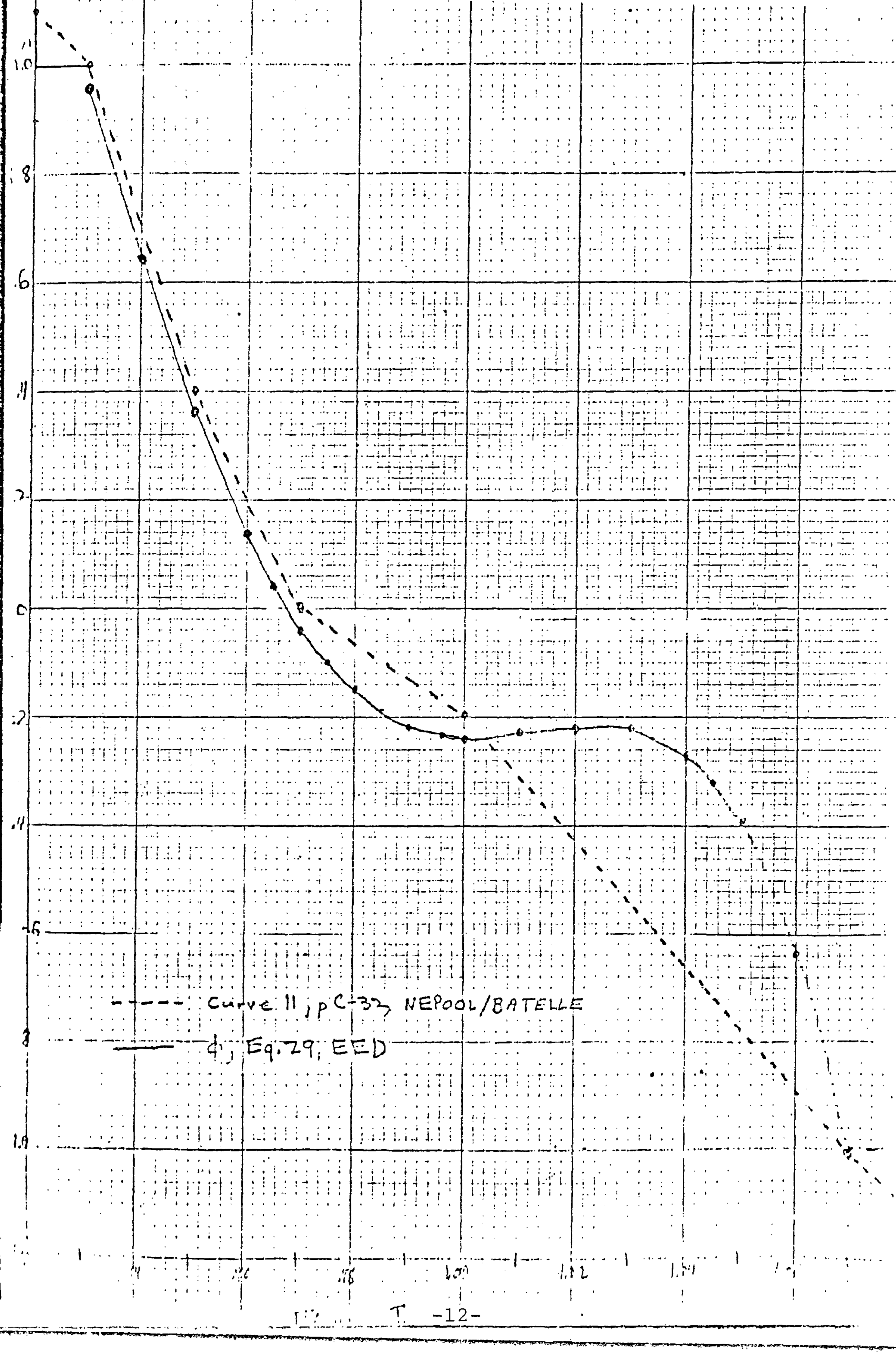


FIGURE NU-I

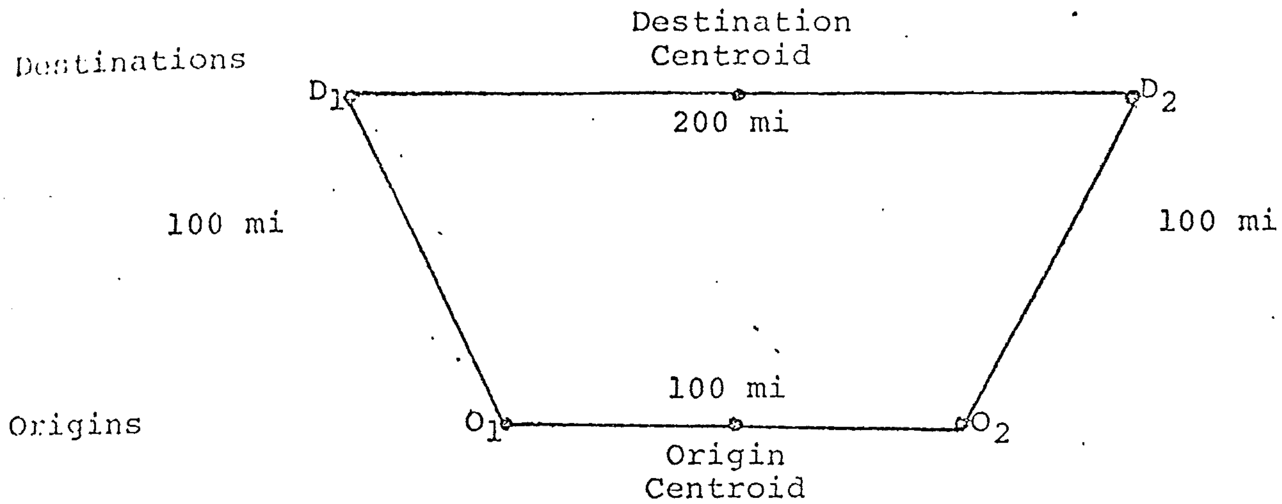
this equation adjusts all SICs' labor costs downward, by as much as 10% or more in the forecast period. Furthermore, equation 23 adjusts RLC more rapidly when RLC<1 (local costs are cheaper than national costs) than when RLC>1. NU's reasoning on this matter is utterly opaque, and their response to AG-10 answers nothing.

With respect to transportation costs, the major problems concern measurement of distances. While the measurements of distance from New England to other regions are somewhat crude, the real problem arises within New England. NU assumes that all shipments from any part of their service territory originate at the Connecticut employment centroid and terminate at the New England employment centroid. This will tend to underestimate transportation costs within New england, as illustrated in Figure NU-II, infra.

Q: Are taxes measured better than transportation costs?

A: No, they are very poorly measured. Utility taxes, which probably affect few industrial customers directly, are included in the measure, as are insurance taxes, only a portion of which are paid by manufacturing firms. But real estate taxes, which may be very important costs, are excluded. It may not be possible to accurately measure tax costs to business: it is not clear that a bad measure is more useful than none.

FIGURE NU-II



SUPPOSE:

Shipments originate equally from 0_1 and 0_2
Shipments from each origin are equally divided between D_1 and D_2

THEN:

Average shipment length = $1/2 \times 100\text{mi} + 1/2 \times \sqrt{3} \times 100\text{mi} = 136.6\text{mi}$

BUT:

Distance between centroids = $\frac{\sqrt{3}}{2} \times 100\text{mi} = 86.6\text{mi}$.

Figure NU-II: Why centroids are poor measures of distance when regions are close together.

Q: What about energy costs?

A: NU uses that 1971 ratio of Connecticut electric prices to national electric prices. This is the only year in which the ratio was less than unity. It would appear to be more appropriate to use at least the weighted average of 1970 to 1975, which is 1.087.

Q: If NU could correct the problems you have outlined, would their cost index methodology be adequate?

A: I think not. First of all, the "Other Cost" category contains between 58.2% and 90.2% of each SIC's costs. Assuming that the four disaggregated cost categories could be carefully measured and that a reasonable growth modifier function could be formulated, the exercise is pretty pointless if most costs evade both measurement and projection. Furthermore, NU's undocumented assumption that "Other Costs" are equal to the national average is suspect: those other costs are for construction, services, raw materials, and the like, which must pay local wages, taxes, fuel costs, and transportation expenses.

Q: Do you have any comments on the residential model?

A: Yes. Two aspects particularly concern me: the assumed effective date of DOE appliance efficiency standards, and the calculation of refrigerator efficiency standards. These concerns can be briefly stated.

On the basis of discussions with DOE officials (p. 108 EED), NU assumes that the 1980 appliance standards will not be met until 1985. In my conversations with the DOE official responsible for the program, he indicated that he expected them to be met in 1980-1981. NU's assumption has a sizable impact on 1987 residential consumption, and is not, on the face of it, valid. The DOE standards seem to fall well within currently feasible technology (and even within the range of current designs), and NU's assumption is inappropriate.

NU also refers to a 32% reduction target for frost-free refrigerators (p. 130 EED). Actually, the target (now 28%) applies to all refrigerators; as sales shift from manual to the more energy-intensive frost-free refirgerators, the latter must become even more efficient so that the sales-weighted mix will meet the FEA (DOE) standards. It does not appear that NU's calculations are intended to meet that standard.

Q: What problems exist in NU's commercial model?

A: The commercial model suffers from weaknesses in both concept and execution. Conceptually, it relies on many assumptions, including:

1. There is a constant relationship of 425 sq. ft. of floor space per employee;
2. 1% of pre-1960 building stock is removed in each year 1971-1976;

3. Vacancy rates for commercial property are generally negligible;

4. Intensity of commercial activity per square foot and per employee is constant overtime and between business types;

5. "potential electricity use" is a meaningful, measurable variable which applies to buildings of all vintages in a particular year; and

6. A building's saturation of electricity as a percentage of total energy use is determined only by vintage.

These are generally quite doubtful assumptions; any analysis based on them should be carefully examined for internal consistency and sensitivity to alternative assumptions. Until adequate data is collected to permit more disaggregated analysis of the commercial sector, NU's approach may be reasonable, but only if it yields sensible results and is not overly dependent on highly uncertain or imaginary factors. Unfortunately, the commercial model fails on both these grounds.

Q: What problems arise in the execution of NU's commercial methodology?

A: There are peculiarities in at least five distinct steps: the estimation of demolitions, the estimation of floor space in new buildings, the separation of incremental electric use of new buildings from that of old buildings, the estimation of "potential electricity use", and the effects of future conservation.

On page 148 of EED, NU states that demolitions are 1% of the remaining stock of buildings constructed prior to 1970. While EED does not appear to specify the base stock assumed, the 1973 to 1976 demolitions given in Table 63 imply a 1970 stock of pre-1960 buildings of between 25,842,000 and 25,867,000 sq. ft. for WMECO. If that is the case, then the 1971 demolitions should have been 258 or 259, not the 807 NU lists, while the 1972 demolitions should have been 256, not the 783 NU lists. (Incidentally, the demolition figures for the Connecticut service area are all consistent with one another.) Within NU's methodology, if demolitions are smaller, then so are the amounts of new buildings. Using the consistent demolition figures for 1971 and 1972 given above, new floor space for those years would be 696 and 1076 sq. ft., respectively. The electrical use in new buildings (column 7, Table 63) then implies use per square foot of:

1971: 53,049 ÷ 696 = 76.22 KWH/sq. ft. (vs. NU's 42.61)
1972: 83,917 ÷ 1076 = 77.99 KWH/sq. ft. (vs. NU's 52.35)

However, these values are greater than the "Potential Electric Use" (PEU) that NU assumes for the corresponding years and would imply impossibly high electric penetrations of 140% and 137% respectively. This might tend to cause one to question the validity of the concept and derivation of PEU, or the extension of Connecticut data to WMECO, or the calculation of new floor space, or the derivation of floor space from employees. Instead NU seems to have departed from their original demolition assumption so as "to produce more reasonable results" (response to AG Info. Request 37).

This alteration in the methodology raises a related issue: whether the rate of demolition assumed has an important effect on calibration of the commercial methodology. As the previous discussion indicates, reducing the 1971 and 1972 demolitions to the level NU says it assumes results in impossible electric penetrations. On the other hand, if demolitions in (for example) 1976 are set at twice the level estimated in Table 63, the electric penetration would be 53% rather than 68%. If the "electric penetration" is to be meaningful, "demolitions" must also capture changes in old space vacancy rates; correct estimation of this unmeasured and volatile variable is crucial to the commercial calibration.

Q: What problem arises in the estimation of new floor space?

A: Unlike demolitions, where NU departs from its methodology to avoid nonsensical results, the methodology for estimating new floor space is followed to absurd conclusions. In 1975, the WMECO new floor space is estimated at -1936 sq. ft.

NU attributes this anomaly to "vacant or idle space in existing buildings" (response to question AG-38). But the electricity use in this space is calculated to be 17% larger than average; in fact, an electric penetration rate is calculated for these negative buildings, just as if they were new construction. Further, the whole concept of "electric penetration" is meaningless if the "new buildings" (column 4) are really the difference between new construction and the increase in vacant space. For example, if the 1973 "new building" figure of 1129000 were actually the difference between 2,000,000 sq. ft. of new construction and an 871,000 sq. ft. increase in vacancies, then the electricity use in new buildings would be greater by

$$871 \times 57.0 \times 14.04 \div 53.8 = 12956 \text{ MWH.}$$

Adding this to the estimated 46763 MWH in Table 63 yields 59719 MWH used in the 2,000,000 sq. ft. of new buildings, or 29.86 KWH/sq. ft., for a penetration of 52% as opposed to the 72% reported for that year.

As stated previously, NU should have intended to capture vacancies in the demolition column; otherwise, the entire approach of the commercial model is untenable. Of course, if vacancies are part of "demolitions", as is proper, then the concept of negative new floor space is clearly problematic. In any case, inability to estimate vacancies makes accurate calibration of NU's model impossible.

Q: What problems arise in the separation of incremental electric use in new and old buildings?

A: If one accepts the contentions that underlie NU's methodology, (see p. 16-17, supra) then the basic calculation laid out in the response to question AG-37 (with some clarification from p. 148 of EED) is appropriate.

$$\left[\begin{array}{l}\text{incremental}\\ \text{use in existing}\\ \text{buildings}\end{array}\right] = \left[\left(\begin{array}{l}\text{previous}\\ \text{years}\\ \text{use}\end{array}\right) - \left(\begin{array}{l}\text{use in}\\ \text{demolished}\\ \text{buildings}\end{array}\right)\right] \times \left[\frac{\text{current PEU}}{\text{previous year PEU}} - 1\right]$$

Note that whenever PEU falls, incremental use by existing buildings must be negative. (Furthermore, while this formula eliminates any growth in the use in demolished buildings, it does not net out the previous year's use by those buildings. In fact, it does not appear that previous use by demolished buildings is subtracted at any point in the methodology.)

I have attempted to reproduce the calculation of column 6 of Table 63 in EED. My results are given in Table NU-II infra, and do not correspond well to NU's figures. There is no systematic difference in the results, suggesting that NU used some other approach entirely. This suspicion is reinforced by the observation that neither

TABLE NU-II

year	Sq. Ft. (a)	Kwh/Sq. ft. (b)	Mwh (c)	Surviving MwH (d)	PEU growth % (e)	Existing buildings incremental Mwh (f)
1971	807	14.04	11330	658143	1.30	8563
1972	783	14.22	11136	729167	4.59	33448
1973	253	14.88	3763	840538	.70	5899
1974	251	14.98	3760	893924	-10.45	-93442
1975	248	13.41	3327	841224	- 3.50	-29459
1976	248	12.94	3184	895544	1.21	10833

Column notes:

(a) from column (3), Table 63
(b) (14.04 ÷ 53.8) x (PEU/sq. ft., t-1) from column 10, Table 63
(c) (column a) x (column b)
(d) (column 5, t-1, Table 63) - (column c)
(e) $PEU_t \div PEU_{t-1}$, column 10, Table 63
(f) (column d) x (column e); this should equal column G, Table G3

Table NU-II: Attempt to reproduce column 6, Table 63, EED for Massachusetts service area.

state service area has a negative value for column 6 for 1975, when PEU was declining. Also, even ignoring the MWH's attributable to demolitions, I cannot reproduce NU's results for most years.

Attempting to derive columns 7, 9 and 10 in Table 63 is no more successful (see Table NU-III). Three of the electric penetrations are within three percentage points of NU's figures, but two others are impossible (110% and -91%).

Q: What problems arise in the estimation of Potential Electricity Use?

A: The estimation of historic PEU depends on estimates both for commercial fossil fuel consumption and for end-use efficiency. The former is subject to errors in data collection and changes in definitions and methodology over time, while the latter probably varies by both fuel type and year. Hence, much of the variation in PEU in Table 62 may be due to problems of measurement and calculation, rather than actual differences in energy use. While there is a generally upward trend in the PEU/sq. ft. data from 1965 to 1973, the growth is quite uneven.

For example, while a best-fit line from 1965 to 1973 has a slope of about .57 KWH/sq. ft./year, the same technique yields a slope of .125 when applied to data from 1965-71 and .011 from 1966-71 data. Considering the nature

Table NU-III

ear	growth in total sales(1)	increment in existing buildings(2)	decrement due to demolitions(3)	increment in new buildings(4)	new building electric use(5) Kwh/sq. ft.	Electric penetration(6) percentage
	(a)	(b)	(c)	(d)	(e)	(f)
971	71830	8563	11330	74597	59.92	110
972	103998	33448	11136	81686	50.96	89
973	53383	5899	3763	51247	45.39	79
974	-53133	-93442	3760	44069	22.07	43
975	54177	-29459	3327	86963	-44.92	-91
976	37519	10833	3184	29870	33.67	67

Notes:

(1) from column 5, Table 63 EED
(2) column f, Table NU-II
(3) column c, Table NU-II
(4) (a)-(b) + (c)
(5) (d) ÷ (column 4, Table 63 EED)
(6) (c) ÷ (column 10, Table 63 EED)

of the underlying data, and the sensitivity of the trend line to the time period, the .57 KWH/sq. ft./year should be viewed with considerable scepticism. Furthermore, NU extrapolates this questionable trend from a period of low and generally falling real energy prices into a period of much higher and (as is commonly anticipated) rising energy prices, coupled with falling real costs of conservation.

Q: What problems arise in NU's estimation of the effects of future conservation programs and efforts?

A: The ASHRAE-90-75 standards are applied to some extent in new buildings. However, NU estimates a 35.6% reduction in energy use under ASHRAE 90-75 (p. 154 EED) while the A.D. Little study they cite lists reductions of 41.6% to 61.5% for various building types; NU's adjustment seems rather conservative. In addition, no explicit allowance is made for retrofitting any energy-saving technology in existing buildings, nor for changes in building operation. Considering the recent changes in the applicable codes in Massachusetts, for example, the latter assumption is clearly inadequate.

Q: Do you have any summary comments on the commercial model?

A: Yes. I would emphasize three points. First, the calibration of the commercial model is irreproducible. Second, the electric penetration rate and PEU projections are based on very shaky data and methodology. Third, the impacts of more efficient energy use are applied in a limited fashion and applied only to new construction.

Q: What comments do you have on the industrial model?

A: While NU's general approach looks reasonable, there are some puzzling and disturbing aspects. These involve the general specification procedure for the industrial equations (Equation I1), the handling of price in specification and forecast, and the "other sales" equation (equation I2).

Q: Please describe the specification of equation I1.

A: Actually, NU uses 9 different specifications for the 14 SIC's. Since various specifications include or omit four variables (employment ratio, conservation, price, and time), there are at least 16 possible specifications for each SIC. The response to AG-44 indicates that two conservation dummies were actually used, so 24 specifications were possible, not counting the use of special dummies.

It is not at all clear how NU selected one of these many specifications for each SIC. The answer to AG-44 does little to clarify that issue, since NU's description of the specification process consists of:

> That standard R^2, T-statistic, and Durbin-Watson statistics combined with common sense and good judgment resulted in the industrial model specification.

Nor is the voluminous computer output provided very helpful. In SIC 20, for example, neither the selected equation, nor any other multiplicative specification is presented in the output; the models shown are additive. In SIC 22, only the selected equation and five others are presented, out of the many possibilities; the last two alternatives appear to have better t and R^2 statistics than the selected specification. I did not review the other 12 SIC's: the absence of input data and the spotty definitions of variable names made the material difficult to interpret; in any case, it was not possible to determine why particular specifications were attempted and selected.

Finally, it would seem that use of the industrial production index without some modification, such as that provided by the local employment measure, would be most undesirable. Omission of employment (similarly, price) from some SIC specifications is especially hard to justify.

Q: Please explain how electricity price is handled in the specification?

A: The price variable is a ratio of a typical electric price to an average wage rate. This formulation omits the possibility of substituting capital or alternative energy sources ~~of~~ for electricity; the real price of electricity would be a better measure.

Another serious difficulty arises in that only ~~the~~ current year prices are considered. Considerable research indicates that adaptations to price changes are rather slow, and that the eventual reaction to a price change may be five to ten times the reaction in the first year following the change. Therefore, the coefficients listed in column 5 of Table 69 are only short-run elasticities and can not be expected to capture either the future effects of past price changes, nor all the effects of future price changes. NU should at least have attempted to define some specifications with lagged price effects.

Another problem arises in the way that the price ratios are forecast at constant levels (apparently 1976). Since NU is forecasting dramatic decreases in wage rates (at least relative to national levels), it would be appropriate to incorporate price projections in the forecast.

Q: What comments would you like to make concerning the "other sales" equation.

A: As presented on page 1974 of EED, this equation projects "other" sales as the 1976 "other" sales multiplied by the current year's growth in Connecticut's share of national production growth. But then the 1987 "other" sales of 1531.9 GWH (Table 70, EED) would have to be the result of a 51% increase in projected Connecticut industrial output from 1986 to 1987; I doubt that NU is predicting such a high growth rate.

Furthermore, 24% of the 1976-1987 industrial sales growth is in the "Other" category (Table 70). But nearly half of this projection, which is untempered by price or by conservation, is due to the use of 1976 as a starting year. The 1976 "Other" sales are 74% higher than the 1975 sales; using this base figure may introduce a serious bias into the "other" forecast. Additionally, "other" sales grow faster than the average rate for industrial sales. NU reports no effort to backcast their "other" equation, nor to develop a best-fit specification. Therefore, the large growth in "other" sales should be viewed with considerable scepticism.

Q: Has NU properly dealt with electricity price elasticity in this forecast?

A: No. In no portion of the forecast is any future price increase, nor any future impact of any past price increase, contemplated in any way. Electric price appears in a short-run application in calibration of the industrial model, but both the price-related and conservation-related impacts in the industrial sector are assumed to be historical events, determining starting points for current forecasts, but not affecting future growth rates.

Q: Does this conclude your testimony on the NU forecast?

A: Yes, it does.

III. NEW ENGLAND ELECTRIC SYSTEM

Q: On what documents do you base your evaluation of the NEES forecast?

A: My evaluation of the NEES forecast is based on a review of the initial 1975 forecast, the two forecast supplements, the report by the National Economic Research Associates entitled "Residential, Commercial, and Industrial Demand for Electricity and Growth of Peak Demand, A Report to the New England Electric System" (the NERA report) and the information filed in response to the Attorney General's information requests in EFSC 78-24. The NEES forecast of total sales supplied to NEPOOL is based on the NERA report. My testimony will be a critique of the NERA methodology.

Q: Do you have any general criticisms of the NERA model?

A: Yes. The NERA report provides inadequate documentation of the methodology; therefore, the methodology is largely unreviewable. However, the information available indicates major deficiences in the methodology.

First of all, there are problems with the data base. The source and characteristics of the data base are inadequately described. It appears that the econometric analysis and therefore NEES' 1977 forecast is based on pre-oil embargo, pre-1973 data, a period of falling electric prices.

Second, the regression analyses are poorly documented. The report fails to specify the models used, the alternative models estimated, the criteria for model selection and the goodness-of-fit tests. In particular, the report fails to specify the R^2 adjusted for the number of independent variables. It also fails to supply the variances or the significance levels of the estimated coefficients. Instead, for the residential and commercial models, the tables carry the following footnote: "Coefficients of all included variables meet differing levels of statistical significance," a useless tautology.

Third, the report fails to document in detail the assumptions made about future values of independent variables and the supporting evidence for these assumptions. In particular, the NERA forecast makes the following unsubstantated assumptions about the relative rates of increase of electric and fossil fuel prices (see response to A.G. Information Request 7):

Real Price of Electricity
Annual % Change

Sector	1975-1980	1980-1985
Residential	+2% to +3%	0 to +2%
Commercial	+3% to +5%	0 to +3%
Industrial	+3% to +5%	0 to +3%

Real Price of Alternative Fuels
Annual % Change

Sector	1975-1985
Residential	not appropriate
Commercial	+7%
Industrial	+5% to +7%

This assumption of a much faster increase in the fossil-fuel price than in the electric price must have a significant impact on the electric usage projections; therefore, it merits more careful analysis.

Fourth, NERA fails to document in any internally consistent manner how, or even whether, these econometric models are used for purposes of prediction. The following statement is made in the introduction to the NERA report (page I-1):

> It is commonplace today to cite the need for even more elaborate . . . which is not necessarily to say more accurate. . . forecasting methodologies.

Apparently, the NERA forecasters doubt the reliability of their own models. Where the models appear to be used, further adjustments are made to inflate the initial growth rate forecast. These adjustments are unsubstantiated and, in several cases, clearly unreasonable manipulations of econometric estimates.

Q: Do you have any criticisms specific to the residential forecast?

A: My criticisms pertain to the following two major problem areas: the relevance of the data base and the misuse of the econometric results for prediction purposes. The information that is provided about the forecasting methodology indicates an unreliable and inflated forecast.

Q: What is your criticism of the data base?

A: The econometric estimates and the non-econometric selection of adjustments to growth rates are all based on data from pre-embargo years, a period of falling electric prices.

Q: Please explain your objections to the residential prediction method?

A: The NERA residential model identifies two components of consumption, "usage related to appliance decisions" and "net usage." The two components are modelled and forecasted separately. In projecting the first component, it appears that NERA does not use the econometric models of electric versus utility gas appliance saturations (displayed on Tables II-1 through II-5), except perhaps to develop a "1975 estimated level." Instead, according to the NERA report (page II-14):

> When estimating the saturations of electric ranges, electric dryers and electric water heaters, we assumed that the number of households with gas ranges and gas dryers will remain at its 1975 estimated level.

It appears that NERA makes the implausible assumption that after 1975 all new ranges, dryers and water heaters will be electric.

Table II-6 of the NERA report then presents a new set of equations to be used in deriving the saturation projections of these three appliances. One of these equations, "Electric Cooking Net of Utility Gas," has a large positive electricity price coefficient of +1.62. If NERA does use this equation for prediction purposes, it takes rising electric prices into account by assuming that the number of electric ranges rises as the price of electricity rises.

In the case of space heating saturations, the econometric estimates were also scrapped. NERA projects a range for future penetration rates, 50% to 70%, based on only three historical data points (42% in 1970, 63.5% in 1973 and 65.6% in 1974), with the only justification being that "during the period 1970-1974, electric space heating penetration. . . increased on the whole consistently [emphasis added]." Perhaps the 1971 and 1972 data points that are omitted would contradict NERA's claim of an upward trend during the 1970 to 1974 period. At any rate, the existence of an upward trend during 1970-1974, a period of falling electric prices, is irrelevant for purposes of predicting consumer choice in the 1980's. NERA does use the econometric model to forecast net usage. This constitutes an invalid use of econometric estimates. If "net usage" was calculated by "netting out" from total usage, a "usage related to appliance saturations" that is calculated using NERA's econometric models of appliance saturations, then "net usage" is not independent of these models and cannot be used in isolation. If NERA has no confidence in their appliance saturation models, then the "net usage" model is also invalid.

Q: What criticisms do you have of the NERA commercial model?

A: First of all, the econometric analysis relies on data from the continental United States, rather than on data specific to New England, Massachusetts, or the NEES territory. In addition, the sample size is so limited as to cast doubt on the validity of the regression results. The NERA report itself refers to the model as "tentative" (see page III-1).

Secondly, the coefficient of the income variable, "Percent Households with Cash Income $<$ \$3,000", has the wrong sign. The opinions of the NERA analysts to the contrary notwithstanding (see page III-4 and III-6), it seems more reasonable to expect the sign of that coefficient to be negative.

Finally, NERA's use of the model for predictive purposes lacks validity.

Q: Why does the projection of commercial sales lack validity?

A: In two cases, to take into account economic activity factors and the effects of rising prices of alternative fuels, NERA makes adjustments to an initial forecast. These adjustments are not econometrically valid.

Q: What are your objections to NERA's treatment of the economic activity variables?

A: For two economic activity variables, the Urban Index and the Ratio of Retail Employment to Total Commercial Employment, NERA makes no attempt to project values. Instead, NERA arbitrarily adds 0.5 to 1.0 percentage points to account for these factors, with the following tenuous argument (page III-8):

> First, if the two economic activity variables are left out of the commercial model, income elasticity increases from roughly 1.3 to 1.8. This, of course, would produce significantly more rapid projected growth in commercial sales for projected increases in income. Second, small changes in the two variables produce major impacts on growth. For example, a change in percent of commercial activity represented by the retail sector of only 0.7 percent -- for example, from 34 percent to 33.3 percent -- produces an estimated 1 percent increase in commercial sales. Similarly, a change in the urban index of seven points -- for example, from 150 to 157 -- produces an estimated 1 percent increase in commercial sales.

In their first argument, the NERA authors fail to point out the changes in the other coefficients (price elasticity, for example) and the changes in the reliability of the regression estimates that occur when the two economic activity variables are omitted. The second argument is also misleading. The NERA report offers no historical evidence of trends or variation in these two variables in the NEES service territory. In the future, these variables may very well change in the opposite direction or not change at all.

Variables such as these which, by NERA's own admission, can have such a large effect on the projection of commercial energy use, merit more careful analysis.

Q: What is your objection to NERA's treatment of the effects of alternative fuel prices?

A: The NERA study does not include a competitive fuel price variable in the commercial model. NERA apparently tested models with such a variable, but in none of these was the coefficient significant. In support of their model, the report justifies its failure to detect a significant effect of changes in fossil fuel prices on electricity use (page III-3 and III-4):

> In theory, we would also expect the price of competing energy sources to have a positive effect on the level of commercial sales, again, all else equal. However, within the context of these data, we have been unable to identify any significant

> effect for such variables. One might speculate on the reasons for this failure to find a significant cross-elasticity effect in the commercial demand for electricity. For example, it seems not unreasonable to argue that the bulk of commercial demand for electricity historically -- and within the service areas covered by our data base -- has been in uses for which commmpetitive fuels are not available, i.e., lighting, air conditioning and ventilation. In other words, one simply might not observe significant amounts of electricity used in space heating and water heating applications within a commercial sector (and, of course, this is true of clothes dryers and ranges). Available data support this view. For example, while 40 percent of residential consumption nationally is related to choice of electrical energy for space and water heating, cooking and clothes drying, the corresponding figure nationally for commercial consumption is 8 percent. Within areas covered by our data, the figure must be far less.

Then, contrary to the prior arguments, the NERA report proceeds to add in fuel substitution effects, thus raising the growth rate projections to 5.5 to 7.5 percent for 1975 to 1980 and 5.0 to 8.0% for 1980 to 1985. NERA added in these fuel substitution effects using a statistically invalid manipulation of econometric models. In a stewpot approach to econometric forecasting, NERA tacks on a price coefficient from an apparently in-house, unpublished model "of the commercial sector that utilizes a very similar set of economic and demographic factors as explanatory variables" prepared by Kent Anderson "of NERA" (page III-8). According to the NERA report, Anderson derived an electric price elasticity of -.81 and a cross-price elasticity of +.81. According to the response to the Attorney General's Information Request 7 the NERA study simply appended the cross-price elasticity to the original model, thus producing a model with an own-price elasticity of -0.39 and the cross-price elasticity of +0.81.

Q: What are your criticisms of the NERA industrial sales growth projection?

A: There are major deficiencies in the NERA industrial forecast:

First, the econometric model is based on national data, instead of data specific to the NEES service territory. The industrial class is disaggregated for only 5 industry groups that account for only 31% of the NEES territory's total industrial sales. The NERA report gives no further information on the source and characteristics of the data, in particular, the time period.

Second, the econometric models fit the data poorly. According to Table IV-1 the output elasticity is not statistically significant (at the 10% level). In only one out of the six industry groups is the alternative fuel price elasticity significant (at the 5% level). Of the three independent variables included in the model, the electricity price appears to be the most important variable; it is significant at the 5 or 10 percent level in 5 out of the 10 cases.

Third, the NERA report makes unsubstantiated claims that the results of their econometric analysis are conservative. The following statement appears on page IV-5 of the report:

> Although we have some concerns that the estimated price elasticities are too high in absolute terms because of limitations of data which prevent us from further disaggregating our analyses, we believe these results represent a reasonable basis for projecting industrial sales growth within the NEES service territory, if one wishes to remain on the conservative side.

Perhaps NERA assumes that problems with data necessarily lead to conservative estimates. In this case, considering that the elasticity of the aggregated group of industries is much lower, in absolute terms, than those of the disaggregated groups, -.26 compared to a range of -.56 to -.98, NERA's claim of conservatism appears particularly unappropriate.

Fourth, it is unclear whether NERA actually uses the econometric model in deriving the industrial demand projection. The report describes a series of subjective adjustments to unspecified base growth rates. These adustments are based partly on NERA's interpretation of outdated (1974 or earlier) and nationwide studies of manufacturing industries; and partly on sheer speculation.

In addition, NERA appears to assume for the NEES industrial sector, a growth rate drawn from projections (done in 1972 through 1974) of the growth in Gross National Product, a figure which includes growth in the commercial and service industry sectors as well as in manufacturing.

In justifying the subjective adjustments to the forecast, the only evidence that is specific to the NEES service territory is NERA's interpretation of confidential and therefore unreviewable interviews of 9 NEES industrial customers made in January, 1975. Furthermore, the description of these subjective adjustments is unclear and internally inconsistent. Not only does the NERA report not derive the forecast directly from the econometric analysis. Judging from the series of subjective adjustments made, it appears that NERA does not believe the results of the econometric analysis that it presents as the basis of its forecast.

Q: Does this conclude your testimony on the NEES forecast?

A: Yes.

IV. PUBLIC SERVICE COMPANY OF NEW HAMPSHIRE

Q: What materials did you review in preparing this portion of your testimony?

A: I obtained the forecast PSNH prepared late in 1977; this forecast is incorporated in the 1978 NEPOOL Load and Capacity Report. The forecast document consists of:

1. Final Forecast Review (11/2/77), presenting final sales forecast,

2. Residential Forecast (10/7/77), the forecaster's response to top management questions and instructions,

3. Residential Forecast (9/6/77),

4. Industrial Forecast (12/20/77),

5. General Service Forecast and Total Price Sales Forecast (10/12/77),

6. Sales to Other Utilities (10/11/77),

7. Development of Monthly Net Price Output and Winter Prime Peaks for the Period 1977-1978 Thru 1988-1989 12/15/77, which is based on time 1 above and presents the peak forecast used in the 1978 L and C Report,

8. Lost and Unaccounted for MWH (1/1/77), and

9. An untitled memo which includes "Summary of Fuel Price Projections" (10/27/77).

In addition, I reviewed the PSNH Preliminary Sales Forecast of August, 1978, which contains actual 1977 sales and partial 1978 sales.

Q: Do you have any general comments on the PSNH forecasting methodology?

A: Yes. In general PSNH disaggregates sales in considerable detal, to the level of individual appliances, industrial SIC's and commercial divisions. PSNH then generally assumes greatly overstated growth rates based on clearly biased projection techniques to produce a drastically inflated sales forecast.

Q: On what specific aspects of this forecast will you be commenting?

A: This testimony will consist of an overall description of the methodology, followed by specific analysis of the residential customer forecast, the space heating penetration forecast, the appliance consumption projections, the "Industrial" class forecasting methodology, the "General" class forecast methodology, the "Other Utilities" forecast, and the peak demand forecast.

Q: Please describe PSNH's overall methodology?

A: The sales forecast is the sum of:

1. The residential sales forecast which is the product of residential customer number times the summation over appliance type of saturation times average use, plus an "Other Use" category;

2. The industrial forecast, which is the sum of sales to:
   a. nine nonmanufacturing industries, each forecasted as the product of projected KW demand and projected hours use;
   b. thirty eight large manufacturing customers, projected as nonmanufacturing;
   c. a group of 300 smaller manufacturers, held constant in the future; and
   d. new manufacturing customers since 1969, trended as a fraction of total manufacturing sales.
3. The general service forecast, forecast as the product of:
   a. general customer number, projected as a generally declining fraction of residential customer number; and
   b. sales per customer forecast to increase linearly.
4. The street lighting forecast, for which I have seen no documentation;
5. The other government authority forecast, which is the sum of sales to:
   a. the Navy Yard and Pease AFB, held constant at about the last three year's average sales;
   b. University of New Hampshire at Durham, projected at .8%; and

6. Other Utilities, which is the sum of Concord Electric Company's forecast of their purchases and PSNH's forecasts of sales to New Hampshire Electric Cooperative, Exeter and Hampton, Wolfeboro, Ashland, and New Hampton.

A fraction of annual use is then attributed to January. A monthly load factor is applied to the January use to determine annual peak demand.

Q: Please describe the deficiencies in the residential customer forecast?

A: There are major problems in both the population forecast and in the projection of the ratio of customers to population.

Strangely enough, PSNH uses New Hampshire population forecasts, rather than town or county forecasts, despite the fact that much of the state is served by other utilities. Absolutely no attempt seems to have been made to disaggregate the PSNH territory population from the state population in any year. Since the PSNH territory population as a fraction of state population may change in the future in very different ways than it has in the past, this error may seriously distort the PSNH forecast. Furthermore, it is hard to believe that PSNH has been unable to obtain an exogenous state-wide forecast since 1975.

Even given the limited data they utilize, PSNH seems to have produced an unrealistic state-wide population forecast. First of all, the claim that the PSNH forecast is conservative, because it lies below the N.H. Office of Comprehensive Planning (NHOCP) estimates for 1976 and 1977, is simply irrelevant. Since the customer forecast is based on trending historical ratios and applying the results to current estimates, the data of interest relates to growth rates: consistent over or under-estimations of population produce consistent changes in customer-to-population ratios. In setting their customer-to-population ratio PSNH uses Department of Commerce estimates, so it is imperative that the population forecast be consistent with those figures. Yet, PSNH's population forecast growth rate exceeds the NHOCP forecast growth rate, which has historically exceeded the Federal estimates (and NHOCP's own estimates) in growth rate, as well as absolutely. Given the data presented in figure PS-1 (from PSNH's forecast), their state population forecast growth rates appear grossly overstated, rather than conservative.

Q: How does PSNH forecast residential customer number, given state population?

A: PSNH uses an equation of the form

$$\text{Customers}_t = a\left[\text{Population}_t\right]^b \times \left[\text{Customers}_{t-1}\right]^c$$

where a,b,c are estimated coefficients

FIGURE 1

PUBLIC SERVICE CO. OF NEW HAMPSHIRE

Book No. ____ Page No. ____

Subject: POPULATION FORECAST FOR NEW HAMPSHIRE

File No. ____

Sheet ____ of ____ Sheets

Computed By ____ Date AUG 31 1977 J.C.J. Checked By ____

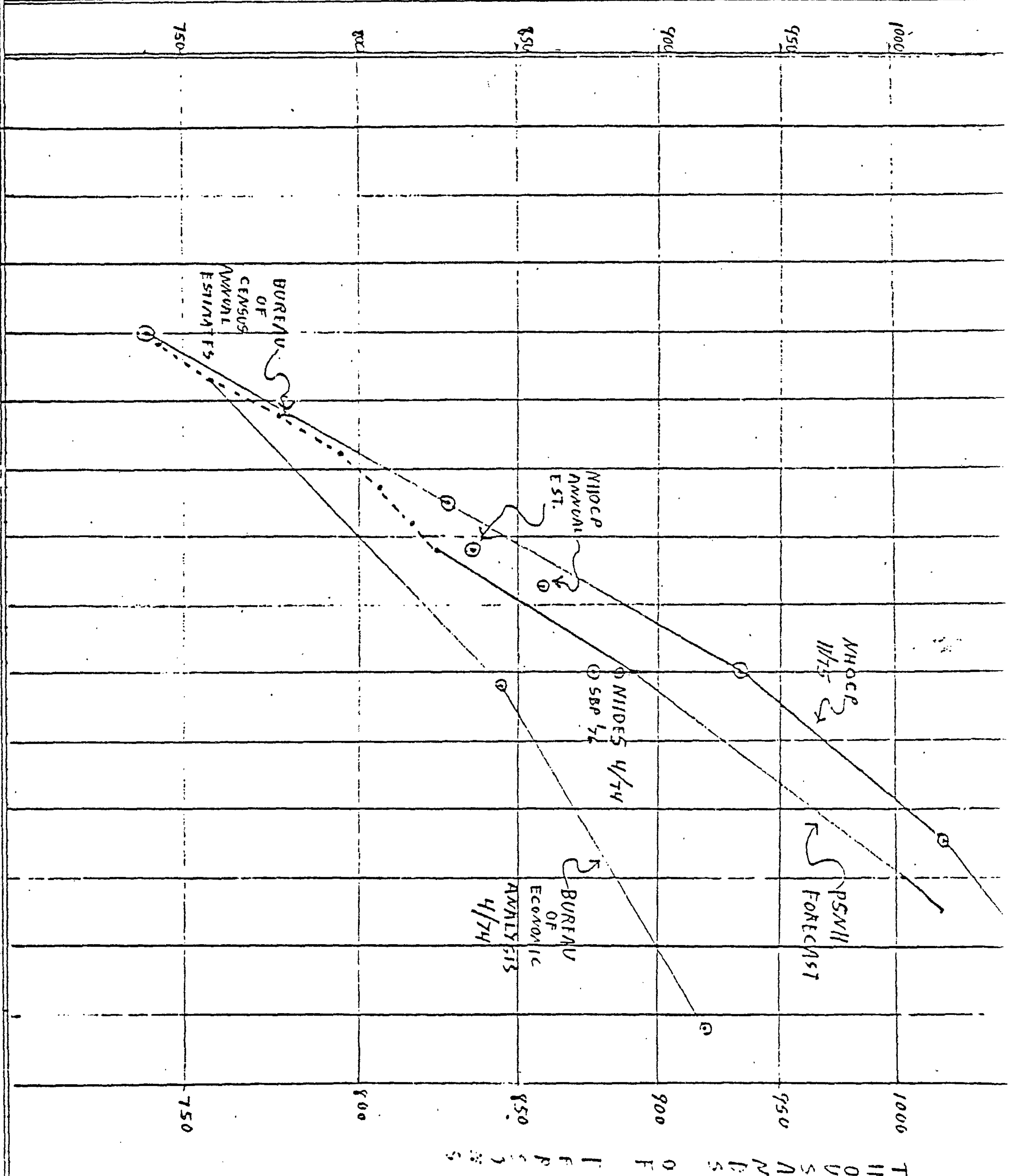


Figure PS-1

t indicates current year

t-1 indicates previous year

The reasoning behind this approach is utterly opaque. Of the obvious causal variables for customer number, only total population is represented: neither age-specific population, per capita income, youth employment rates, nor any measure of PSNH's population as a fraction of the state appears in the equation. Even time, which might serve as a proxy for some relevant variables, is excluded. This formula compounds growth in the customer-to-population ratio, implying that family size will fall faster and faster over time. Actually, as large families are phased out, the ratio should tend to stabilize. (Surely, the trend cannot continue past unity or in PSNH's case, some larger number, reflecting the unspecified share of state population in the service territory). In addition, since b + c = 1.2233, customer number must increase over 20% faster than population; this relationship was estimated on the basis of an annual population growth rate around 1.96%, yet it is extended to a period of forecast 2.44% population growth, a 24.5% higher rate. Since the change being modelled is more dependent on time than on population, this extrapolation from low growth to high growth will be likely to overestimate the customer growth rate, in addition to the errors caused by compounding customer number and by using an excessive population growth rate.

Q: Once PSNH has derived a customer number, how is the rest of the residential forcast derived?

A: PSNH estimates the consumption per customer by forecasting a saturation and an average consumption for each of 14 appliances, to which are added lighting and other uses.

The saturation projections are apparently entirely subjective, sometimes loosely based on historical trends. (See our testimony in D.P.U. 19494, Phase I, on trending saturations.) There is no disaggregation by dwelling type, nor between new market penetrations and acquisitions by existing customers. Apparently, no correction is made in the heating, water heating, range, or dryer categories for the gas shortages of the mid-1970's, which may have artificially increased electric market share. Furthermore, special problems are evident in the space heating and dryer saturation forecasts.

Q: What are those special problems?

A: In the case of electric space heating , a number of new electric space heating customers is forecast, based on PSNH's Marketing Division forecast of additional electrically heated dwellings. PSNH's data indicates that Marketing has historically overestimated by an average of

28.3%. Therefore, the forecast is lower than marketing's projection, but only by 20.8%. This greater confidence in marketing's judgment seems particularly ill-founded considering the caveat that:

> The Marketing Division Forecast is contingent upon an increase in manpower in order to assist people in making their decisions concerning selection of spaceheating systems and energy.

To the extent that the forecast relies on the assumption that the New Hampshire Public Service Commission will allow PSNH to spend ratepayers' money (or even stockholders' money) on promotional advertising and incentives to builders and homeowners, even while such practices are being discouraged or prohibited elsewhere, it seems overstated. In fact, the saturation trends of all appliances for which gas is an alternative energy source would be distorted by that assumption. (In any case, the historic saturations of the other appliances with gas alternatives were inflated by the high electric heating penetrations; if PSNH's forecast trends are based on those inflated past penetrations, they are overstated.)

Curiously, PSNH projects rising heating penetrations until 1984, when Seabrook 2 is scheduled to come on line, and falling slightly thereafter. Regardless of how and why the projection was derived, PSNH is forecasting that the falling trend since 1973 will be reversed, even in the face of rising electric prices, regulatory reforms and improved gas availability.

In the case of dryers, PSNH reports a strong variability in their survey results, which suggests some problems in the sampling or data gathering techniques. However, PSNH says that they simply ignored the low 1973 saturation survey result and "the trend from 1971 to 1976 was extrapolated". But, as Table PS-1 shows, the .92 annual percentage point increase of the 1971-1976 period is increased to a 1.48% annual growth in the 1976-1987 period; the projected rate of increase also rises slightly within the forecast period, from 1.4 to 1.6 percentage points. Therefore, even when PSNH attempts to explain its saturation forecasts, the projection belies the documentation. Additionally, PSNH reports in the text that the 1973 dryer saturation was 40.9%, yet it is reported in the saturation tables as 44.9%; it is not possible to determine how much of the other data, which PSNH presents as "actual", has been similarly altered.

Q: How appropriate are PSNH's forecast of usage per appliance?

A: Average use projections are poorly developed and grossly overstated. In general, all the projections suffer from PSNH's failure to distinguish changes in new dwellings from those in existing dwellings, to distinguish between apartments and single-family homes, to allow for family size effects, or to systematically apply efficiency standards to new units and retrofitting to old units.

In addition, we have specific comments on space heating, water heating, refrigerators, dryers, freezers, televisions, air conditioning, lighting and other use.

Q: Please comment on PSNH's space heating use projection.

A: The space heating use is held constant at the 11000 kwh/yr. estimate for 1975. PSNH acknowledges that future homes will be smaller than current ones and that supplemental heating systems (presumably solar and wood) would decrease usage, yet no such decrease is forecast. PSNH does not seem to realize that future housing stock will almost certainly be better insulated, weather-proofed, situated, and designed than past ones; that apartments use far less heating energy than houses; that the smaller families that PSNH projects will result in longer daily time periods when homes are unoccupied and less heated; that wood and solar heat will have far greater application in future homes than past ones (indeed, electric heat will frequently be the "supplementary" source); or that existing dwellings can be better insulated, weather proofed and equipped with automatic-setback thermostats, wood stoves, and solar heating. Therefore, a sizable decrease of the average space-heating use would appear to be appropriate.

Q: What errors does PSNH make in its forecast of electric water heater use?

A: PSNH forecasts increases in average water heater use, "reflecting the increase in dishwasher and clothes washer saturation being offset by increased efficiency in waterheaters." However, taking into account the efficiency improvements expected in new water heaters, dishwashers and clothes washers, as well as family size, should decrease average waterheater use by over 20% by 1987, resulting in residential sales 5% lower then forecast, even with PSNH's saturation projections. In addition, insulation retrofit on existing heaters and pipes, better placement of water pipes in new construction, and improved basement insulation and flow-reduction devices in all dwellings will further reduce electric use per waterheater. Also, water use by clotheswashers and dishwashers will tend to respond to family size decreases.

Q: What deficiency exists in the refrigerator use forecast?

A: According to PSNH, average refrigerator use was 1592.33 KWH/yr. in 1976. Assuming a 15-year life, some 38% of existing refrigerators will be replaced from 1981 to 1987; also, 19% of PSNH's projected 1987 customers are new since 1980. Therefore, some 57% of PSNH's refrigerators would be covered by federal standards of 28% efficiency improvements, for a net 16% improvement and an average use of 1338 kwh/refrigerator. PSNH forecasts 1934.39 kwh/refrigerator, a difference of about 600kwh/unit or 6750 – ? kwh/yr/customer. This is 6.4% of the residential forecast for 1987. According to NEPOOL, another reduction of 40kwh would result from smaller family size.

Q: Do the same effects occur for dryers?

A: Yes. Federal standards of 4% would apply to about two thirds of PSNH dryers, for a 2.8% reduction. In addition, family size charges would produce another 8.8% decrease, for a total effect of 11.4% reduction. PSNH assumes a decrease of only 2.5%.

Q: And for freezers?

A: Freezer average energy consumption is scheduled to decrease 22% for new units, ~~on~~ or about 15.4% for PSNH's average unit. PSNH forecasts a decrease from 1146 to 1083 kwh, only a 5.5% decrease.

Q: Does PSNH do better with televisions?

A: No. The federal standards of 35% reductions for color sets and 65% reductions for monochrome sets will probably be exceeded by the average sets 1987, as circuitry advances from transistors to integrated circuits. PSNH forecasts improvements of only 11.2% in color televisions and of only 5.8% in monochrome sets.

Furthermore, as the number of sets per household increases from 1.167 to 1.32, average hours use per set (and kwh/yr) should fall, especially because the number of people per household will be falling.

Q: What comments do you have on window air conditioner average use?

A: PSNH lowers average use by less than 2%, despite federal standards of 22% improvement and improvements in weather proofing in homes.

Q: How does PSNH forecast ~~forecast~~ lighting use?

A: They hold their estimate constant. Efficiency improvements (particularly through greater use of fluorescent bulbs), smaller home size, and smaller family size should all contribute to a decrease in this category.

Q: How does PSNH forecast other use?

A: Other use is basically the residuals in PSNH's estimates of individual appliances saturations and average use, plus such additional uses as microwave ovens, fossil heating auxiliaries, central air conditioning, and small kitchen, personal and entertainment appliances. PSNH apparently regressed this random error term against per capita income and then projected it into the future on a per household basis. The purpose in trending an error term is not quite clear. Beyond its retrospective use as a catch-all, the other use has a legitimate forecasting role, reflecting the use of particular appliances. However, many of these uses should decrease over time, or cause these uses to decrease. For example:

(1) microwave ovens will replace far more energy than they use; PSNH forecasts that 76% of ranges will be electric, so most of the displaced energy will be electric;

(2) fossil heating auxiliaries will decrease in saturation as electric heating saturation increases (PSNH assumption), and decrease in average use as house size falls and weather proofing improves;

(3) central air conditioning is subject to DOE appliance efficiency standards; and

(4) home sound equipment will become more efficient as the conversion continues from tubes to transistors to integrated circuits.

Considering the nature of PSNH's "data" for their trend, and the factors described above, it seems excessive to increase Other Use at over 7% per year.

Q: Does this conclude your testimony on PSNH's residential model?

A: Yes.

Q: Please describe PSNH's basic approach to the Industrial class?

A: PSNH disaggregates the Industrial class (which includes large commercial customers) into a number of categories. For most of those, sales are projected as the product of forecast KW billing demand and forecast hours of use for each category. Each of these forecasts are, at PSNH's discretion, based on a sort of exponential trend analysis, on an historic average of 1970-1976, on a 1973-1976 average, or on some totally fabricated growth rate. Thus, PSNH started with an inadequate trending technique and apparently manipulated the trends to produce any desired result. This "technique" was applied to nine non-manufacturing categories and 78 large industrial firms.

For the industrial firms, PSNH manages to "trend" a .9% historic growth rate into a 3.47% growth rate. This is achieved by breaking the class into three groups.

> Group I: Both KW demand and hours of use showed some increase, however erratic, in the period 1970 to 1976. Sales are forecast as the product of the two trended variables.
>
> Group II: While KW demand generally rose, hours of use generally fell. Hours of use were held constant at 1970-1976 average levels, (not recent ones) while KW demand was trended.
>
> Group III: PSNH simply projected a growth rate from "recent indicators", apparently subjectively.

Various non-manufacturing divisions are trended in a similar variety of ways.

Q: How were the other portions of industrial sales projected?

A: The total sales to 300 small industrial customers is held constant (at a level 6% higher than 1976) in the future, despite the fact that these sales fell in four of the last six years, for an overall decline of 2.5% annually from 1970 to 1976.

Sales to new industrial customers is projected as a fraction of total sales to manufacturing. Since this total includes the new customers, the formula doubly compounds the growth rate. Some time in the year 2020, this relation predicts infinite sales to new customers. In addition, the ratio of new sales to total sales is further increased by two percentage points per year, which, in a sense, approximates the historical experience. But the 1970-1976 experience is equally supportive of a two percentage point annual change in new sales as a fraction of old sales, or of a linear 19.5 GWH per year increase in new sales (which fits with $r^2 = .99$). While PSNH's rather imaginative method produces 657.6 GWH of new sales in 1987 and a 15.08% growth rate from 1977 to 1987, taking new sales as a fraction of old sales yields 432.9 GWH in 1987 and a 10.37% growth rate, while linear growth yields 342.6 GWH and an 8.00% growth rate. Both PSNH's method and the new-as-a-fraction-of-old method described above are greatly inflated by PSNH's projection of much faster future growth in sales to old customers (2.87% from 1976-87) than occurred in the past (.90% from 1970-76).

Q: Does any of the evidence available to you suggest that PSNH utilized reasonable, consistent or appropriate statistical or economic methods in producing their projections?

A: No. The only consistency is that PSNH repeatedly manipulates formulae and data to produce projections which are not supported by their own data.

Q: How does PSNH forecast sales to their General Service customers?

A: PSNH determines the number of General Service customers (apparently small commercial enterprises) by some type of trending of the 1960 to 1976 data for the ratio of General to Residential customers, with a 1987 result of .1113. However, extrapolating the negative compound growth rates of 1960 to 1976 or 1966 to 1976 produces a 1987 ratio of .1067, while the 1972 to 1976 growth rate yields a 1987 ratio of .1007. Some of the discrepancy arises from PSNH's increase of the ratio in 1977 and 1978; not until 1981 does their ratio forecast fall below 1976 actual levels.

Sales per customer is projected to increase linearly at almost six times the average 1973 to 1976 increase. PSNH claims that this growth rate is moderate and reflects conservation, but neither building efficiency standards, future equipment efficiences, nor price elasticity are addressed explicitly.

Thus, General Sales are the product of the inflated Residential customer forecast, an oddly high General-to-Residential customer ratio, and a highly suspect trended use per customer. This results in a greater forecast MWH sales increase to this class, in each year 1978-1987, than in the commercial rebound year of 1976, and greater percentage increases in all those years, except two, than in 1976.

Q: Is the separate projection of General customer number and sales per customer a reasonable approach for this class?

A: Probably not, due to the diversity generally found in the commercial sector.

Q: How are sales to Other Government Authorities forecast?

A: A few large users are held constant or increased slowly, while the rest are increased as a class at 10% year, greater than their 1970 to 1976 growth rate. No accounting seems to be made for any further conservation in this sector.

Q: How were sales to other utilities forecast?

A: From the materials available to us, it appears that the long-term historical trends were subjectively modified for each utility, except for Concord, which forecast its own purchases. For all companies, annual growth in sales increases over time. Apparently, either no conservation was assumed for the customers of these utilities, or a greater share of their purchases are projected to come from PSNH: it is not clear what assumptions are made regarding other sources of supply.

Q: What other factors affect PSNH's peak forecast?

A: PSNH appears to be forecasting a small increase in the fraction of annual sales which occur in January, apparently reflecting the assumption that space-heating usage will continue to rise. However, monthly load factor, which has been steadily increasing since 1974, is held constant at a level lower than the 1975, 1976, or 1977 actual values. Using 1977's January fraction and monthly load factor, 1987 peak would be 7.7% lower than PSNH's forecast, a difference of 168 MW, even using PSNH's forecasts of sales and losses. This calculation does not include the effects of load management (including controlled water heating, which PSNH apparently is promoting), time of use rates, or the generally higher load factors predicted in the industrial sector.

Q: Does this conclude your testimony on PSNH's forecast?

A: Yes.

TABLE PS-1: PSNH DRYER SATURATION FORECAST

	DRYER KS9
70	0.367
71	0.431
72	0.440
73	0.449
74	0.548
75	0.467
76	0.477
77	0.491
78	0.505
79	0.519
80	0.534
81	0.549
82	0.564
83	0.579
84	0.594
85	0.609
86	0.624
87	0.640

V. CENTRAL MAINE POWER

Q: What materials did you review in preparing this portion of your testimony?

A: We were able to obtain some of Central Maine Power's (CMP) responses to Data Requests from Louise McCarren in a CMP rate case before the Maine Public Utilities Commission, docketed as FC #2322. Apparently, there was no official forecast document, but one of the responses is to the question

> "Please state the methodology used, including all the variables considered. . . (and) a detailed account of the statistical techniques used to assure the accuracy of the growth projections. . ." (1VLM-7)

The responses to this question and follow-up questions appear to present CMP's best explanation of its methodology. The forecast peaks coincide with those in the 1978 NEPOOL Load and Capacity Report.

Q: Please describe the CMP forecast methodology?

A: The sales forecast is composed of Residential, Commercial, Industrial, and Other Sales. Sales in each category are then allocated between months. Losses and Company Use are added to each month's sales to determine territory output, which is multiplied by a monthly load factor to determine monthly peak.

Q: Do you have any general comments on the sales forecast?

A: Yes. Considering the size and forecast growth rate of CMP, the sales forecast is wholly inadequate. As CMP stated:

> In summary, the basis for the forecast is a computer program which produces a forecast of monthly sales, net for load energy and peak loads. The key variables input to the program are as follows:
>
> 1. Residential nonseasonal customers;
> 2. Nonseasonal subclass saturation rates;
> 3. Residential seasonal customers;
> 4. Residential customer average usage by subclass;
> 5. Commercial sales growth rate;
> 6. Industrial sales growth rate;
> 7. Other sales growth rate;
>
> 8a. Lost and Unaccounted For growth rate;
> 8b. Other Company Use Factors; and
> 9. Monthly Load Factors.

In other words, CMP is only really interested in what happens after the sales forecast is developed. The sales forecast shows the result of this neglect. CMP indicates that "Ordinary least square (sic) regression analysis is the statistical technique which is put to greatest use." This technique is obviously inadequate for most purposes: in any case, we cannot determine any situations in which CMP actually used linear regression. The lack of detail and disaggregation in the various classes and the absence of a formal forecast document are also indicative of CMP's cavalier attitude towards forecasting.

It is also interesting that Dr. Joel Brainard of the Brookhaven National Laboratory reviewed CMP's forecast for the rate case mentioned above and testified that ". . . if you use the company's data and their methodology, you will not get the numbers they claim you'll get. . . The error in some cases is quite large." Our review confirmed this finding in several cases.

Q: Please describe the Residential forecast methodology?

A: The Residential class is divided into five subclasses: general, waterheating, spaceheating, all-electric, and seasonal. A Maine population projection was multipled by CMP's fraction of state population and divided by a projection of household size to yield households in CMP's territory. Since CMP apparently uses data on consumption per dwelling, rather than consumption per customer (a peculiar approach, which must cause some data problems), they then presumably scaled the household count upward to include vacant units. Unfortunately, CMP says they "divided by one plus. . . the vacancy rate", when they should have intended to divide by one <u>minus</u> the vacancy rate. It is not clear what in fact they did.

CMP then seems to have determined the number of new "customers" (actually dwellings, since some 11% were assumed to be vacant) in each year and to have apportioned them between the four non-seasonal subclasses by use of utterly undocumented, and very high, penetration rates for electric space and waterheating. In addition, the seasonal customer class is increased by 300 customers annually.

Each subclass "customer" number is then multiplied by an average consumption per dwelling. These average consumptions must capture the effects of dwelling type, dwelling size, family size, appliance saturations (other than space and water heating), average appliance consumption, efficiency standards, retrofitting of insulation, woodburning, building design, and electricity price. None of these factors was explicitly considered. In fact, average consumption for non-seasonal space-heating and all-electric customers was simply projected to increase at 140 KWH annually, presumably based on some data from the late 1960's, when average consumption was rising. Since 1972, average consumption for these two classes has actually <u>fallen</u> by an average of 718 KWH annually; a linear time trend on this period would predict declines of 588 KWH annually. Of course, no historical trend can capture future appliance efficiency and the like.

Similarly, General Residential customers' average consumption was assumed to increase at an amazing 200 KWH annually, waterheating customers at 190 KWH annually, and seasonal customers at 100 KWH per year. While we do not have historical data for these subclasses, it is unlikely that appliance penetrations could counter-balance greater appliance efficiency, insulation retrofit on water heaters and pipes, and all the other conservation measures available to residential customers, so as to produce these large increases. Of course, since CMP has no saturation data for most appliances, it would be difficult to model these effects in detail.

Q: Please describe the commercial methodology?

A: Based on an analysis of historical data, CMP claims that 3.4% annual long-term growth in Maine nonmanufacturing employment is "appropriate". We have not seen the data they used; given their performance in other sectors, CMP's credibility in determining "appropriate" growth is not high.

CMP then multiplied Maine nonmanufacturing employment by KWH per employee to yield Maine commercial sales. The KWH per employee figure is said to be based on a time trend; CMP provides no details, as usual. In any case, this factor increases at 250 KWH/employee/year in most forecast years, and occasionally at 240 KWH/year.

Even if CMP's projection of sales per employee is based on a perceived trend, it is apt to be incorrect for three important reasons. First of all, commercial sales and nonmanufacturing employment have not historically referred to the same establishments. CMP apparently used sales to certain rate classes as a proxy for commercial sales until 1975, at which time they reclassified sales on the basis of SIC. This reclassification may have moved many large commercial customers (e.g., shopping centers) into the commercial class for the first time, dramatically inflating the sales-to-employee ratio and distorting historic trends, which may have already been affected by classification problems. Secondly, no correction has been made in the historical trend to reflect higher electric prices, equipment efficiency, or improved building design. Indeed, given CMP's predilections, the forecast growth may exceed historic rates. Third, CMP does not disaggregate commercial sales in any way, either in historic data or in the forecast.

CMP determined its share of Maine commercial sales by use of the equation:

$$GWHC = \left[.801 * CWHM\right] - 45.2$$

where GWHC is CMP commercial sales in GWH

GWHM is Maine commercial sales in GWH

Obviously, this formulation will increase CMP's share of Maine sales over time. We have no information as to how CMP chose this peculiar function, nor from what data (if any) it is derived, nor whether the other Maine utilities are projecting corresponding decreases in their share of total commercial sales.

Finally, CMP's methodology does not seem to produce its commercial forecast, perhaps because of further reclassifications in 1978. But even the growth rates are not taken from the methodology, CMP holds growth constant at 7.0% from 1983 to 1990, when the methodology should produce steadily declining growth, due to the linear growth in sales/employee and the asymptotic nature of the CMP fraction equation. CMP's methodology yields a 6.4% growth rate in 1990, for example, rather than the 7.0% CMP uses. In fact, it appears that CMP used a higher growth rate than the methodology would produce <u>in every forecast year</u>.

Q: Please describe the industrial sales forecast methodology?

A: Industrial sales are forecast as a whole, without any disaggregation by industrial type, such as SIC code. CMP forecasts 17.7% growth in industrial sales in 1977, followed by a constant (if sloppily calculated) 4.0% growth thereafter, until 1990. CMP claims that "the industrial sales growth rate is based on the long term trend exhibited between 1965 and 1975." This long term trend was actually 3.0% annual growth. In the longer term, 1965-1977, CMP industrial sales grew at 2.6%, which slowed to 2.5% from 1969 to 1977, to 2.1% since 1972, and 1.0% since 1974. How a 17.7% jump and long term 4.0% growth can be "based on" these historic trends is not at all clear. No effort is made to incorporate electricity price or national or local industrial output into the forecast.

Q: Please describe the Other sales forecast?

A: Other sales is projected to grow at 5% annually from 1978 to 1990. This growth appears to be chosen to approximate the overall forecast growth in the major classes. However, these sales have historically fluctuated with no real pattern: since 1966, other sales have grown at a compound rate of 1.6% annually, but they have <u>fallen</u> since 1973 at -5.5%. Almost any moderate growth rate, positive or negative, could be supported in some fashion by this erratic record, but not one as high as 5%. Zero growth might be the most reasonable assumption.

Q: Does this conclude your testimony on the CMP forecast?

A: Yes.

VI. UNITED ILLUMINATING

Q: What materials have you reviewed in preparing this portion of your tesimony?

A: The only document available to us was the Report to the Power Facility Evaluation Council (1/1/78) by United Illuminating (UI).

Q: Please describe the UI forecast?

A: The energy forecast consists of sales to four sectors (residential, commercial, industrial, and street lighting) plus losses. An annual load factor is then applied to predict peak demand. UI, like NEES, uses a bandwidth forecast; unlike NEES, UI explains many of the assumptions used in developing each limit of the band.

Q: Please describe the residential model?

A: UI's method starts with exogenous population forecasts, which are divided by UI's projection of average household size (partially based on regional planning agency projections) to yield customer number. Since the low band for household size assumes a linear continuation of the recent decline (therefore increasing the proportion~~ed~~al rate of decline), while the high band more than doubles that rate, the bandwidth does not appear to be symmetrical around recent experience. Of course, the high and low band for customer number should ideally be based on alternate age-specific headship ratios, rather than trends, but from the material UI presents, their low band is a moderate to high assumption, not a low one.

Q: How does UI forecast electricity use per residential customer?

A: Saturation rates for 12 appliance types are projected for high and low bands. A single average consumption value (which varies over time) is used for each appliance. Significantly, no distinction is made between saturation or average use by houses and apartments, nor is any forecast made of future housing stock. This ommission makes it difficult to determine how fast some appliances' average consumption should be forecast to change.

UI phases in the DOE efficiency standards evenly over the expected lifetime of the appliance. Thus, a 12% reduction for an appliance with a 10 year life would be phased in at 1.2% a year for 10 years. However, those appliances in the service area which are new, rather than replacements, will <u>all</u> be covered by the efficiency standards, which will therefore come in faster for those appliances than for appliances with stagnant or falling saturation.

Q: Are UI's saturation projections reasonable?

A: Not in general. First of all, there is no documentation supporting the new market or old market penetration rates, which in turn predict sizeable saturation increases. No account appears to be taken of the affect of family size on penetrations for any appliances, including clothes washers, dishwashers, dryers, or freezers, where family size would appear to be an important causal variable. Nor are personal income or electric price incorporated in any saturation projection. There is also no indication that UI has even considered the possibility that saturation increases will slow as saturations rise. (See our testimony in Phase I of this proceeding for additional discussion of this issue.) There are also special problems in several of the individual appliance projections.

For space heating, two recent penetration rates (apparently for new markets) are reported: 25% for 1976 and 36% for nine months in 1977. Based on this, UI's bandwidth of 25% and 55% seems to be set too high. A narrow bandwidth of 20% to 40% might be reasonable, or a wide one of 10% to 55%; but UI uses a very tight lower band and a quite broad upper band.

Backcasting UI's dryer penetration figures for the high-band indicates that there were <u>no</u> electric dryers in their service territory in 1971. Since that is quite unlikely, UI's high-band penetration rates must greatly exceed historical rates. The penetration rates for the multiple-unit appliances of room air-conditioners and color televisions seem equally excessive, but it is difficult to reproduce UI's methodology, due to some vagueness in the definition of the penetrations. In addition, room air conditioning and central air conditioning rates do not seem to recognize the mutually exclusive nature of these appliances, resulting in 110% air conditioning penetration on the high-band.

In this case of electric ranges, UI assumes that 1.5% to 3% of existing customers with gas ranges will replace them with electric ranges each year. Using a 15 year range life, these rates imply that 22.5% to 45% of retired gas units will be replaced by electric ones. This would appear to be a very rapid shift, considering the cost of conversion and the considerable performance advantages of gas stoves.

Various problems arise in the other appliances, such as 100% new market penetration for dishwashers, and more than 100% saturation of refrigerators. In general, the low band seems to represent moderate growth (only for water heaters does saturation fall) while the high band represents an extreme growth scenario: the band again does not appear to be symmetrical.

Q: Does UI properly forecast consumption per appliance?

A: No. UI says that:

> while growth in the number of residential appliances varies with conditions of income,

> relative owning and operating costs, dwelling-type mix and life styles change, the utilization of those appliances will generally follow a trend with a relatively small deviation. The principal contributors to changes in the amount of electricity consumed by appliances tend to be technological and to be a function of size, convenience features or imposed efficiency improvements which together with the boundaries imposed by weather and human behavioral characteristics, result in relatively homogeneous use patterns for residential appliances. (page 6-8).

As noted above, UI's saturation forecasts do not explicitly consider income, housing mix, or any of the other factors they correctly list as being relevant to saturation rates. However, they do implement their assertion that appliance usage changes only due to technological factors. If that were so, short-run price and income elasticities would be very small, and UI's 3.5% decline in residential electric use from 1973 to 1974 is inexplicable. It is absurd to suggest that appliance size, convenience features, efficiency improvements, and human behavior are independent of prices, dwelling type, income, and lifestyles. As a result of this position, UI seriously over-estimates future use by appliances. In addition, UI errs in its application of the DOE standards and in neglecting all retrofitting considerations.

Q: How do these errors affect the forecast of space heating use?

A: The average use is held constant, despite lower family size (which affects both home size and hours of use); changes in dwelling mix; superior insulation, design and general weather tightness in new construction; automatic setback thermostats; improved insulation and weather tightness in existing units; and supplementary heating, such as wood or active and passive solar. Air conditioning use should also be affected by these changes, or their warm-weather analogs, such as shading and lighter-colored western walls.

Heating use is also subject to such behavioral changes as lower thermostat setting, earlier drape closings, greater care in opening doors and using exhaust fans. The 22% decrease in heating usage from 1972 to 1976 must be largely of this nature; behavior may adapt further to rising electric rates.

Q: You mentioned some problems in the air conditioning use projections. Are there others?

A: Yes. As the number of room air conditioners per household rises, each should use less electricity, due to a combination of fewer hours use and lower cooling load.

Q: How reasonable is UI's forecast of water heating use?

A: UI forecasts only a 3% decrease in average water heating use despite a 15% DOE standard, and the effects of insulation on existing heaters, piping, and basements, better placement of pipes in new construction, smaller family size, change in dwelling mix, and hot water savings in showers, dishwashers, and clothes washers. UI's errors in water heating are similar to those of other companies, such as PSNH.

Q: Are there similar problems in other appliances?

A: Yes. Refrigerator average use declines only 12.5% in UI's model, while the DOE target is 28%, and color television use is not adjusted to reflect increased number per household. No usage is adjusted for family size.

Q: Is the lighting and miscellaneous category projected in a reasonable manner?

A: No. Many sources of future conservation are ignored by UI. Dehumidifers and monochrome televisions are covered by DOE standards, and the latter appliance saturation and use should fall further as color TV saturation increases. Additional efficiency improvements should also occur in lighting, electronics, and large motors (as in clothes washers). Falling family and home size should decrease lighting and many appliances. And many new products (such as microwave ovens) will simply displace range use, often with much higher efficiency. UI's projected increase in this largest category seems inappropriate, considering the conservation occurring in other appliances.

Q: How does UI forecast commercial sales?

A: They use high and low forecasts of commercial employment, which is then multiplied by KWH per employee to yield KWH sales. This process is repeated for each of six commercial categories, which are summed to derive total sales.

Q: Are the employment forecasts UI uses appropriate?

A: The low-band forecast appears to be based on the most recent (1976) projection by the Connecticut Department of Planning and Energy Policy. It is not clear why UI used these figures as the low end of their range, rather than the middle. The high end of the range is provided by an old federal forecast from 1967, revised in 1972. The relevance of this work is most unclear, considering the economic and demographic changes in the decade separating the 1967 forecast from 1976 one.

Additionally, NU's 1978 forecast includes non-manufacturing employment forecasts derived from the NEPOOL model for Connecticut and for NU's portion of the state. The remainder of the state (most by UI territory) is projected to grow at only 1.13% from 1976 to 1987; even UI's low-band forecast is for 1.21%. NU's 1979 forecast reports that their improved demographic model now produces even lower employment growth for the state. The tendency of recent studies therefore seems to be towards a much lower range of commercial employment growth than UI predicts.

Q: Does UI's forecast of annual KWH consumption per commercial employee appear reasonable?

A: No. According to the data UI presents in their forecast, use per employee has grown at about .5% since 1973. Their low-band forecast is for 1.9% growth, and the high-band is for 4.6% growth, much higher than historic levels. Even the low-band appears excessive considering the impact of new building codes; more efficient space conditioning, waterheating, refrigeration, and electronics equipment; improved weather tightness and reduced lighting in existing structures; increased gas availability; expanded energy management systems; and the improved economics of cogeneration. The high band appears to be completely unrelated to past, present, or future developments. UI does not document the development of either band.

Q: How does UI forecast industrial sales?

A: Earnings in 7 industries and "other" manufacturing is projected and multiplied by projected KWH/$ earnings to yield KWH sales.

Q: Does UI forecast earnings in an appropriate manner?

A: No. For the high-band, UI uses the same old 1972 forecast that was used in the commercial high-band forecast, for a growth rate around 2.86%. This is arbitrarily lowered slightly to 2.19% in the low-band forecast. In the high band, UI assumes that the 1972 projections for the 1980's will be correct, despite numerous intervening events. In any case, this is a quite archaic source. Surely, more recent and relevant economic forecasts are available.

UI's data also contradicts the text of the document. Pages 6-26 speaks of "losses in primary metals and miscellaneous manufacturing", yet these industries are projected as growing, in Table II-17, even in the low-band. "Other" manufacturing grows at 2.2% in the low case and 2.5% in the high one.

Finally, it does not appear that earnings, which are a function of wage rates and labor intensity (in the wrong direction) are proper measures of industrial output.

Q: Does UI properly forecast electricity use per dollar of earnings?

A: UI states that:

> "The 1975 and 1976 data as base points assumed that most of the short-run (utilization type) conservation measures have already been implemented and no further reductions for this reason are anticipated.

> Future values are assumed to be affected by improved technology on the one hand and fuel switching on the other hand. (page 6-27)."

However, Detroit Edison found that a sample of its industrial customers achieved 3.74% reduction in KWH use and 6% in KW demand due to a brief program which <u>started</u> in August of 1976. Very simple physical changes were involved, primarily in lighting, and all investment had pay-back periods of under 2 years. UI's industrial customers may implement similar changes more slowly, due to UI's lack of a similar consulting program, but there is little reason to believe that the results would be less significant. As electricity prices rise and minor routine modification and replacement of equipment take place, more opportunities for economical conservation are bound to arise. UI seems to ignore all such possibilities and assumes that for all SIC's, the limited effects of improved technology will be dominated by fuel switching from fossil fuels to electricity, even on the low band. On the high band, electric intensities increase even faster.

In part, UI's energy intensity projections are based on the twin assumptions that fossil fuels are getting scarce, and that industry will use electricity to replace those fuels as heat sources. The first assumption is periodically true, sometimes for natural gas and sometimes for oil. However, it is also true sometimes for electricity. Federal policy now seems to favor the use of natural gas, which is in quite adequate supply, despite the dislocations of a few years ago. DOE also seems favorably disposed towards fossil-fueled cogeneration. Thus, a user of oil who wishes to replace an oil-fired heat source has several options, including using gas, cogenerating with gas or oil, or using electricity. It is difficult to understand why industrial customers would choose to pay for electricity which will, at the margin, be produced by burning oil at about 30% efficiency, when they can burn the same oil (or gas) at 80% to 90% efficiency to produce the needed heat (and electricity, if desired).

Q: How does UI's projection of total industrial sales compare to past growth?

A: The five-year period 1968-1973 exhibited 3.37% growth; this is the highest 5-year growth in the data that the company reports. Yet, even the low band forecast is for 3.7% growth, while the high band is for 6.5% growth. Despite the reversal in electric price trends, growth in industrial electric use is projected to exceed historic rates.

Q: Do you have any comments on UI's peak forecasts?

A: Yes. UI forecasts peak demand by forecasting a load factor (63% in the low band, 62% in the high band) which is applied to the forecast of sales plus losses to yield the peak. UI neglects two important factors in the load factor forecast. First, UI projects that the ratio of industrial sales to total sales, rather than declining as it has since 1972, will increase in the future. Industrial customers can be expected to have higher load factors and lower weather sensitivity than other classes, and the growth of this class should improve load factors, just as its past decline has contributed to deterioration in load factors. Secondly, UI explicitly excludes all effects of peak load pricing or load management, even though Connecticut has been a leading state in this field.

Q: Does UI in any way, in any section of its forecast, explicitly recognize the effect of higher electricity prices on consumption?

A: No.

Q: Does this conclude your testimony on the UI forecast?

A: Yes.

VII. EASTERN UTILITIES ASSOCIATES

Q: What materials did you review in preparing this portion of your testimony?

A: I reviewed the Long Range Forecast of Electric Needs and Requirements for the EUA System 1976-1985 (May 1, 1976); the First Supplement 1977-1988 (December 31, 1976); the Second Supplement 1978-1988 (December 31, 1977), which constitutes the current forecast reflected in the 1978 NEPOOL forecast; EUA's testimony in EFSC 78-33, which dealt with the Second supplement; and EUA's answers to 43 information requests in that case (which will be referred to as AG-xx).

Q: What aspects of the residential forecast will you discuss?

A: I will consider issues related to demography, housing stock, customer counts, new customer consumption, electric space heat penetration, existing customer consumption, "Unforeseen Appliances", and price effects.

Q: Is EUA's overall approach to residential forecasting appropriate?

A: EUA's basic residential methodology represents a reasonable beginning framework for a small company. Unfortunately, several important considerations are omitted, and others are severely misrepresented in the methodology.

Q: How sophisticated is EUA's demographic analysis?

A: EUA appears to utilize state and regional population projections and some sort of time trend of household size to derive future residential customer counts. The use of exogenous population forecasts would appear to be reasonable, under the current circumstances. It is probably unrealistic to expect EUA to forecast population independently, given the small size of its service territories and the potential impact of migration.

On the other hand, the time-trending of household size is not a particularly valuable or reliable technique. Age-specific population projections and headship rates, for which forecasts are generally available (see NEPOOL, page D-2)[1/], can be used to derive household number in a more sophisticated and realistic manner than EUA's method.

In addition, the basis of EUA's time-trending is questionable. For both Blackstone and Fall River, the curves EUA fitted to the historical data underestimate household size for every year since 1970 (including 1970 for Blackstone). This is apparently because the family size trends are flattening out in EUA's service territories faster than the selected functional form can follow it.

[1/] References to NEPOOL are to the Report on a Model for Long-Range Forecasting of Electric Energy and Demand, June 30, 1977.

For example, for Blackstone, average family size fell by .97% annually from 1945 to 1965, but only .17% annually from 1965 to 1968. Regression of Fall River data for 1965 to 1978, using EUA's functional form, yields the equation

$$\text{average house hold size} = \frac{\text{Year}}{.33034 \times \text{year} - 1.4465}$$

where year = 0 in 1900
Correcting for the 1978 starting point, this trend projects an average household size of 3.1602 for Fall River in 1988, decreasing residential households to 45938 in that year and reducing the number of new customers in the decade by a third.

EUA's explanation of the household-size forecast for Brockton indicates that some round-about and subjective method was utilized (AG-4). To the extent that it was derived from the Blackstone and Fall River data, the trend is overstated; to the extent it is based on anything else, it is undocumented.

Unfortunately, neither EUA nor the forecasts of other New England companies have found a way to deal with the decreased appliance saturations, penetrations, and electric consumption which would result from smaller household size. Until the necessary data is gathered and analyzed, EUA's projected decrease of family size by 3.4% is somewhat problematical, since these other unrecognized effects will tend to counter-balance the increase in household number.

Q: Does EUA deal appropriately with issues related to housing stock?

A: Not at all. EUA does not disaggregate housing stock in any way, such as single-family, multi-family, and second residences. Since appliance penetrations and consumption may vary widely between housing type, this omission may seriously distort the forecast, unless future housing stock is very similar to the current stock.

Q: Does EUA forecast use by new customers in a reasonable manner?

A: The basic algorithm for new customers seems conceptually sound, but exhibits several practical flaws. The lack of a derivation for penetration and average use figures is one such weakness. Of course, the efficiency improvements should be revised to the actual federal targets by appliance, and more stringent standards should be anticipated during the 1980's. The failure to modify space heating average use to reflect smaller dwellings, better insulation, weather tightness, and generally better design results in serious overestimates. The same arguments would naturally apply to air-conditioning: winterization measures will reduce cooling requirements, as will smaller house size and such specific features as awnings, light-colored roofs and shading.

Perhaps the most serious problem with the new customer methodology, however, is the handling of the "Base Use" consumption, which in EUA's nomenclature includes refrigerators, dishwashers, clothes washers, televisions, fossil-fuel heating auxiliaries, lighting, and miscellaneous. The first four categories are covered by federal efficiency standards. Table EU-I applies the Federal standards to NEPOOL's estimates of refrigerator and TV usage in Massachusetts. In addition, clothes washers and dishwashers will use less hot water, and dishwashers will use less electricity for drying; the combined energy savings due to these latter improvements is projected to be comparable to those from refrigerators[2/] although the electric share of the savings will vary with the electric penetration of water heating. In any case, the efficiency standards should reduce Base Use by at least 20%. If Base Use for new customers is reduced by 10% in 1979 and 20% thereafter from EUA's estimates (which increase over time for some reason) the reduction in 1988 energy use for Brockton Edison is 12.5 GWH, about 1% of Brockton's residential energy (see Table EU-II).

Q: Do EUA's electric heating penetrations appear reasonable?

[2/] Federal Register 7/15/77, page 36649.

TABLE EUA-I

The Effect on Base Use of Federal Standards for Three Appliances

APPLIANCE	(1) % OF BASE	(2) FEDERAL EFFICIENCY IMPROVEMENT TARGET (%)	% BASE REDUCED
Refrigerator	38.8	28	10.9
Color TV	11.8	35	4.1
B/W TV	4.8	65	3.1
Total	55.4		18.1

Notes: (1) from NEPOOL, p. G-28, for 1975

(2) from Federal Register, 7/15/77, 4/11/78, and 10/12/78

(3) product of two preceding columns ÷ 100

TABLE EUA-II

The Effect of Federal Applicance Standards on New Base Use ^ Brockton

YEAR	EUA BASE USE (kwh)	REDUCTION %	NEW CUSTOMER NUMBER	TOTAL ENERGY REDUCTION (Mwh) ~~(kwh)~~
1979	3357	10	1379	463
1980	3480	20	1471	1023
1981	3606	20	1616	1165
1982	3738	20	1664	1244
1983	3874	20	1677	1299
1984	4015	20	1683	1351
1985	4161	20	1686	1403
1986	4312	20	1683	1451
1987	4469	20	1700	1519
1988	4632	20	1720	1593
TOTAL				12511

A: Not really. Table EU-III presents apparent penetration figures by company for 1971 to 1977; these numbers include conversions, changes in vacancy rates and the like, but they offer a rough indication of the popularity of electric heat. Note that apparent penetration rose sharply from 1971 to 1974 and has been falling ever since. EUA predicts increases in electric penetrations to the levels of the early seventies; in the case of Brockton, the penetration forecast surpasses all historical values. Bear in mind that these comparisons are to historical values inflated by conversions, a gas shortage, and promotion by the utilities.

In Table EU-IV, I compare EUA's predicted heating conversions and penetrations for 1978 to the actual results for 1977. The prediction for the EUA system is about 40% higher than the actual number of new electric heating customers observed.

Q: Is EUA's methodology any better for existing customers?

A: No, not really. For example, conversion rates and saturation increases are expressed as a fraction of customers, rather than as a fraction of customers without the appliance (or the electric version). The causal mechanism underlying these penetrations into existing markets involves the purchase of the appliance by people who do not have it now, and it is difficult to see how

TABLE EUA-III

Apparent past electric heat penetration rates

		Blackstone	Brockton	Fall Ri[illegible]
1971	a. △heating customers	42	93	132
	b. △total customers	1656	1626	488
	c. apparent penetration %	2.5	5.7	27.[illegible]
1972	a.	90	362	154
	b.	707	2081	460
	c.	12.7	17.4	33.[illegible]
1973	a.	70	294	135
	b.	172	1581	333
	c.	40.7	18.6	40.[illegible]
1974	a.	128	609	42
	b.	292	1949	93
	c.	43.8	31.2	45.[illegible]
1975	a.	75	299	-34
	b.	230	1310	181
	c.	32.6	22.8	——
1976	a.	23	271	3
	b.	423	1406	213
	c.	5.4	19.2	1.[illegible]
1977	a.	48	198	6
	b.	580	1286	151
	c.	8.3	15.4	4.[illegible]

TABLE EUA-IV

Comparison of 1977 actual increase in electric heating customers and prediction based on 1978 penetration and conversion

	Blackstone	Brockton	Fall River
New customers, 1977	580	1286	151
Predicted heating penetration rate 1978	.08	.15	.10
Predicted new heating customers, 1977	46	193	15
Existing customers 1977	66123	85656	44263
Predicted heating conversion rate, 1978	.0005	.0005	.0005
Predicted existing customers converting to electric heat, 1977	33	43	22
Total predicted additional heating customers, 1977	79	236	37
Actual additional heating customers, 1977	48	198	6
% of error in prediction	65	19	517

these factors can be forecast without both determining the number of households which might switch and estimating the fraction which will. Since no derivation or historical data is provided for these factors, it is not clear whether the authors understood what they were doing. The forecast numbers seem to be the products either of this fundamental misunderstanding, or some remarkable coincidence.

Secondly, EUA fails to recognize that old electric appliances will be replaced by new, more efficient appliances. For example, in 1980, EUA projects 10% annual replacement of water heaters. If the saturation of electric water heaters is 20% and the old units average 6000 KWH/year, the reduction in usage due to this turnover is (using Brockton as an example):

.10 x 20 x (6000-4874) = 22.5 KWH/year/customer

or about 2 GWH due to one appliance in one year. This correction should be made to the five listed appliances (space heating is a bit different) and to Base Use as well. If refrigerators are 38.8% of an average Base Use of 3,000 KWH for existing customers, if 10% of the refrigerators are replaced in each year and if the efficiency improvement is 28%, then in 1980, the reduction in usage due to refrigerator replacement is about

3000 x .388 x .10 x .28 = 32.6 KWH/customer

or about 3 GWH for that year (for Brockton). Note that this is about the size of EUA's projected Base Use increase for that year, which apparently reflects some sort of historical trend.

EUA's asserts that this error is "compensated for by an under-estimation with new customers", since some new customers bring some appliances with them. This factor is apt to be minor for several reasons. First of all, the number of new customers is relatively small, compared to the existing customer counts. Second, new customers are alleged to have very high penetration rates; unless only people who already have many appliances move to EUA's service territory, this indicates that most appliances will be new. Third, EUA assumes high Base Use for new customers, and applies no efficiency standards to Base Use; clearly, this is an <u>over-estimation</u>. Fourth, about a third of the customer increase is the result of decreased family size; these bifurcated families will not generally have appliances to take with them. Fifth, very few people carry water heaters or space heating systems when they relocate. Sixth, saturations of appliances and especially the electrical versions of ranges and dryers tend to be low in Boston Edison's service area, compared to EUA's new customers; people who have gas ranges (as well as those who rent an apartment with an electric range) will not have an electric range to bring with them.

In addition to appliance efficiency, EUA also ignores the impacts of conservation in existing space heating, water heating, and air conditioning applications through such measures as insulation, weatherization, and temperature set-backs. Surely, not all the walls, ceilings, floors, hot water pipes, and tanks in EUA's service areas are optimally insulated, nor have all customers installed heat traps, automatic set-back thermostats, water-saving shower heads, storm windows, and the like. Failure to account for these factors is a serious oversight.

Q: What are your comments on the "Unforeseen Appliance" in the residential forecast?

A: EUA's "Unforeseen Appliance" category is quite an innovation in forecasting. I am disappointed that the forecast does not include an "Unforeseen Conservation" factor as well. After all, solar heating and hot water and passive cooling are much more technically and economically attractive then the electric car, which the "Unforeseen Appliance" is apparently modeled on. EUA has never presented any evidence that such an "Unforeseen Appliance" has ever appeared so rapidly, let alone that it is a regular decennial occurrence. Thus, in addition to increasing Base Use, EUA has thrown in a 19% saturation of a highly unlikely 4000 KWH/year appliance, with accounts which for over a quarter of annual residential growth in EUA's Massachusetts service territory by 1988, and over a third in Rhode Island.

Q: How does EUA estimate the effects of electricity price on consumption?

A: EUA seems to ignore the effects of price increases, both historic and projected. This oversight is significant both because electric price is an important determinant of demand and because price impacts can be estimated quite easily and conveniently. Short-run and long-run elasticities have been estimated from various national and regional data sets by a large number of investigators; most studies are fairly consistent in deriving short-run residential elasticities in the -.1 to -.2 range, and long-run elasticities in the -1.0 to -1.2 range, although there is some spread around these figures. The large differences between the short-run and long-run effects indicate that much of the impact of the price increases of the early to middle 1970's are yet to be felt. EUA's response to IR AG-14 indicates a failure to understand these effects, especially lags and non-substitution price effects (efficiency, use, size, and purchase decisions).

Given the existing data base, local elasticity estimation is desirable but hardly essential; a small company, such as EUA, may simply apply elasticities representing the consensus of national or regional studies. The actual application of the elasticities can be quite straight forward, and can be conducted independently of population, household, housing, and appliance models.

Q: Does this conclude your comments on EUA's residential model?

A: Yes.

Q: Is EUA's commercial methodology appropriate and reasonable?

A: EUA projects commercial sales as a fraction (sometimes greater than unity) of residential sales. This method has both advantages and disadvantages.

On the positive side, EUA's commercial methodology is relatively simple and straight forward in application. It also responds to both local population and residential conservation measures. On the negative side, the methodology requires a forecast of the residential/ commercial ratio, is inversely proportional to household size, and does not reflect commercial conservation measures.

Q: What problems arise in forecasting the residential/ commercial ratio?

A: First of all, since no uniform, consistent methodology is applied, the ratio forecast is essentially judgmental. Once the residential forecast is determined, a subjective forecast of the ratio is indistinguishable from a completely subjective forecast of commercial sales.

Secondly, this ratio is sensitive to the definition of rate classes, such as whether master-metered apartments are counted as residential or commercial sales. Unless the classes are properly distinguished, the ratio is meaningless. Page VIII-5 of the second supplement illustrates the impact on the ratio of the Fall River customer reclassification.

Q: What problems arise with the sensitivity of the commercial forecast to household size?

A: As noted above, the increase in per capita residential consumption forecasted by EUA is partly due to the decrease in family size. Since commercial sales are projected as a function of residential sales, this implies that greater commercial sales result from smaller family size. At least 3% of 1988 commercial sales would seem to originate in falling household size; actually, since new households use more electricity than existing households, and since declining household size generates new households, the impact must be substantially more than 3%.

It is not at all clear that commercial activity or electric use is more closely related to household number than to population; in fact, population is generally preferred as an explanatory variable, when the data is available.

Q: Please explain why the commercial forecast is not sensitive to conservation and price effects.

A: EUA forecasts increases in penetrations of electric heating and appliances, in both new and old dwellings, as well as increased Base Use and Unforeseen Appliance Use. These effects more than counteract the limited residential conservation introduced by more efficient appliances (note that the residential model seriously underestimates the impact of efficiency standards, as described above). This represents a very limited view of the potential for commercial conservation from lighting reduction and replacement, more efficient appliances, improved ventilation systems, weatherization, and improved building design. With reference to the last point, see the discussion of the ASHRAE 90-75 standards in NU section II, supra. Massachusetts is also changing lighting and ventilation standards for old and new buildings; the lighting code will require a 40% reduction in average commercial lighting levels.

Commercial establishments may have many of the same conservation technologies available to them as are available to residential customers, but with greater flexibility, expertise, incentives, opportunity, and regulatory pressure. In addition, large commercial establishments have a conservation option not generally available to householders: cogeneration. This and other conservation measures may be encouraged by a variety of rate reforms, such as flat rates, peak-load pricing, and fair purchased-power and back-up-power rates for cogenerators. Therefore, assuming that commercial conservation will only be as great as residential conservation is very conservative. Assuming, as EUA apparently does, that conservation will be dominated by new electric uses is extremely unlikely.

Q: Can you determine from EUA's filing the extent of commercial conservation or price effects embodied in the forecast?

A: No, I can not. EUA fails to distinguish between changes in commercial activity (sales, floor space, employment, etc.) per household, historical energy use per unit of activity, electric penetration of the commercial energy market, and conservation. The forecast ratio (if one is used) should be derived from a quantitative analysis of all these factors; it is not clear that any of them were explicitly considered.

Q: What comments would you like to make on EUA's industrial forecast?

A: As filed, the industrial forecast is unreviewable. EUA generally has not provided or explained the historical data, the interview results, or the subsequent manipulations on these data and results. For example, EUA supplies some sort of trending computation for Brockton in IR AG-17; it is not a linear regression and in fact, is so poorly documented as to be incomprehensible. In any case, both historical growth rates and interview results have serious limitations as forecasting techniques.

Q: Please explain the limitations of historical growth rates for forecasting industrial sales.

A: Historical growth rates are deficient for forecasting for at least four reasons:

1. Future national (or even world) growth rates for output, shipments, employment, or value added in various industries may not be the same as past growth rates.

2. Local growth rates in the future may not bear the same relationship to national growth rates that they did in the past.

3. Technical change may alter historic relationships between industrial activity and electric consumption; e.g., conversion from vacuum tubes to integrated circuitry in both control equipment and products.

4. Increasing energy prices and rate reforms may further alter the ratio of output to electric use by encouraging more efficient equipment, greater care in the maintenance and use of equipment, cogeneration, etc.

Q: Why are interviews inappropriate for forecasting industrial electric consumption?

A: Again, there are several reasons:

1. It is not apparent that industrial customers make any concerted effort to realistically project the output, let alone the electric use, of particular facilities a decade in advance.

2. Forecasts which customers do prepare may be optimistic planning documents to facilitate growth under favorable conditions or to impress the home office with the plant manager's zeal.

3. Projections delivered to the utility may be tailored to the utility's expectations, either out of a general cooperative spirit or in hopes of such specific results as construction of new transmission facilities or special consideration in rates or service for a potentially significant customer. In particular, a customer is unlikely to mention major conservation or cogeneration plans as there is no reason to antagonize the utility earlier than necessary. This may be especially true when the utility is in a position to interfere with the project, e.g., Boston Edison's actions to halt MATEP. Similarly, customers may not wish to publicize plans to close a plant.

4. The results of the interview process may be manipulated in many ways by the utility, intentionally or unintentionally, including:

   a. the selection of companies to be interviewed;
   b. the interviewer's attitudes and comments;
   c. the phrasing and sequence of questions;
   d. the numerical interpretation of qualitative responses; and
   e. the weighting of results from various companies.

   In fact, unbiased interview or survey techniques are difficult to design and implement, even for impartial and well-trained social scientists.

5. If it is to be a reviewable public document, a forecast based on interviews must present a great deal of detail on the methodology for gathering, interpreting and processing the data, as well as summaries of the data collected and the sources. It would also be important for the utility to present forecasts both of industrial output (or activity) and of electricity consumption per unit of output. Reasonable levels of documentation also may create problems with confidentiality of individual customer plans; companies may either refuse to participate or come to view the survey as a public relations forum.

Q: Are there any other problems in the forecast methodology?

A: There is at least one more serious error. EUA's limited statistical analyses were conducted on a data set from which certain data had been removed. That data represented industrial customers who have gone out of business. EUA is implicitly assuming that none of their current customers will close down in the future and, additionally, that all customers will grow (on the average) at the same rate as the successful customers of the previous decade. That analysis does not even properly represent past experience.

Q: Do you have any comments on EUA's peak forecast?

EUA considers only one type of load management in two customer classes. Other candidates for control include commercial display lighting (such control may soon be mandated at the federal level), retrofit of currently uncontrolled water heaters in all classes, and some industrial and commercial processes (such as heating and

chilling equipment). Furthermore, the extra water heaters EUA projects should not be contributing to peak demand.

In addition to controls imposed by the utility, customers will tend to switch activities out of the peak period to avoid peak rates. Again, this applies to all classes and may involve both rescheduling of activities (such as clothes drying) and greater care in those activities which continue to fall on peak.

Q: Have you seen revised estimates of EUA's forecast?

A: Yes. In D.P.U. 19738, EUA presented new estimates, including a 1987-1988 winter demand that is over a hundred megawatts below that contained in the Second Supplement. Figures EU-I and EU-II show the evolution of EUA's forecast (excluding sales to NEES' Tiverton Division) over time. Figure EU-I shows each forecast as a single line, while Figure EU-II shows the forecast for each year as a single line; the data is the same for both tables. The 1987-1988 peak forecast has fallen 211 MW, or 18.3%, between the time it was first projected in January 1977 and the most recent estimate.

Q: Have you been able to determine whether EUA's new forecast has corrected the problems you discussed above?

A: No. EUA has not yet released its methodology;in fact, the February 1979 forecast is said to be basically an estimate of the effect of anticipated revisions. However,

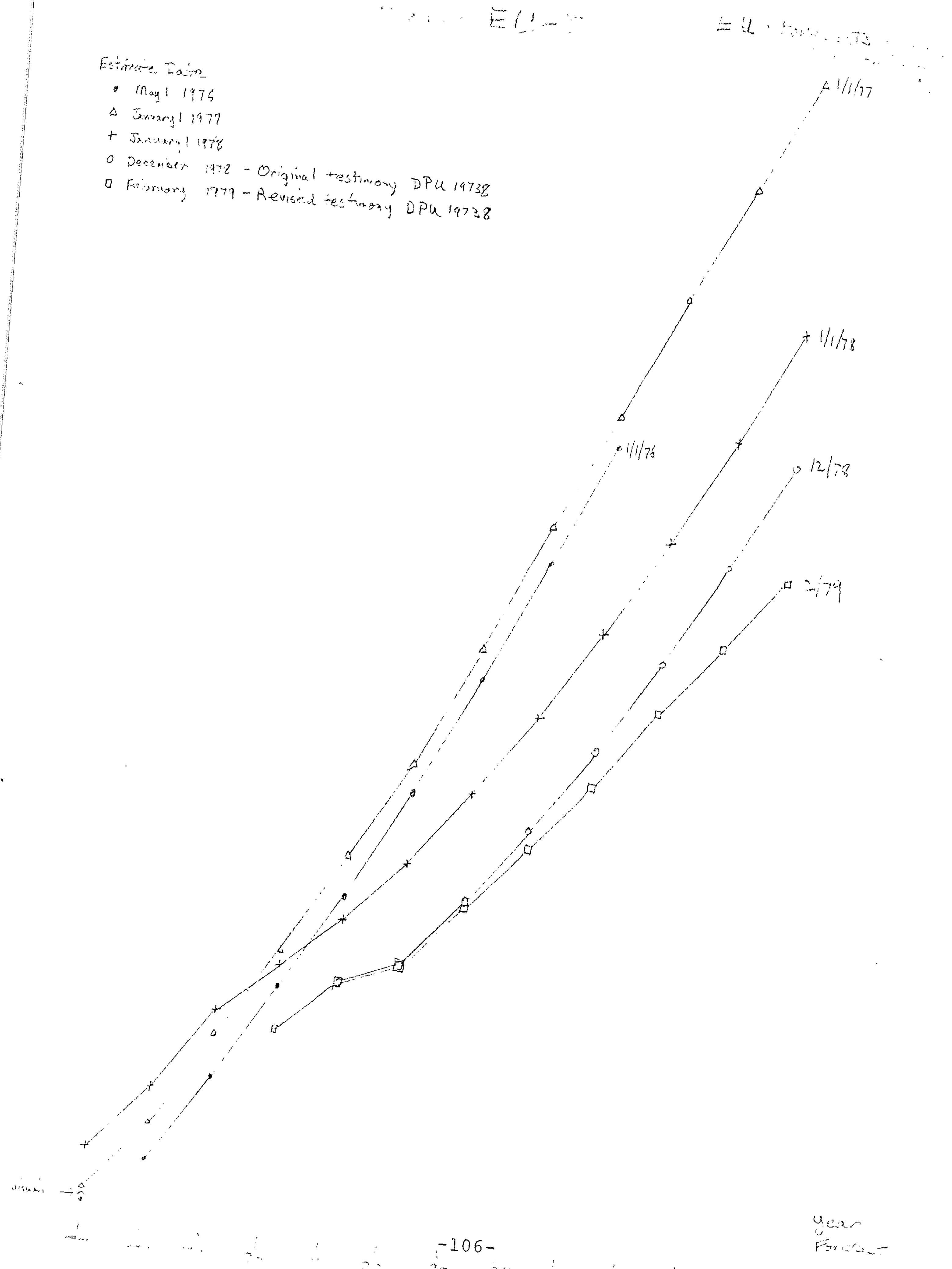
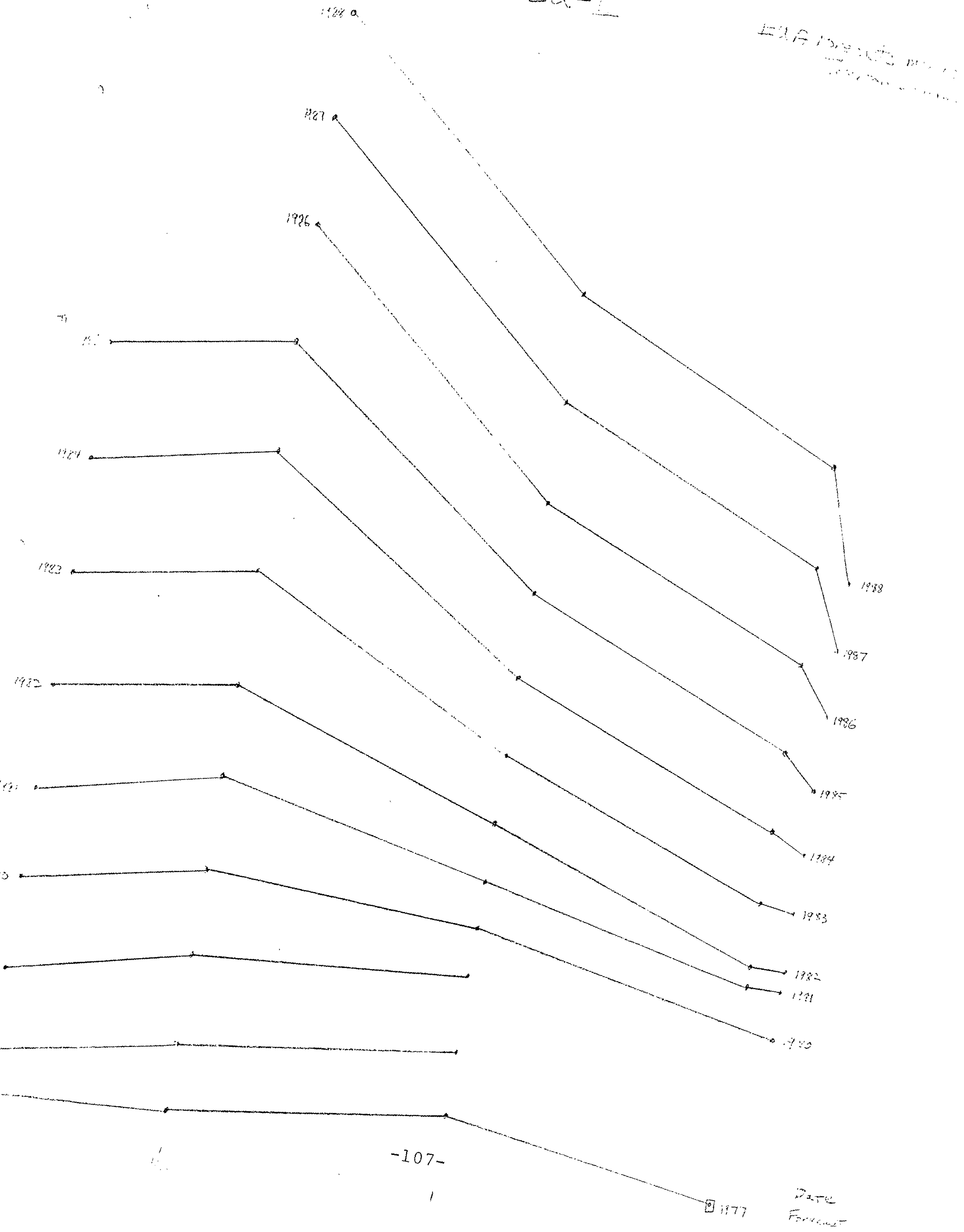

Estimate Date
May 1 1976
January 1 1977
January 1 1978
December 1978 - Original testimony DPU 19738
February 1979 - Revised testimony DPU 19738
1/1/77
1/1/78
1/1/76
12/78
2/79
Year
Forecast

Figure EU-I



1988
1987
1986
1985
1984
1983
1982
1981
1980
1977
Date Forecast

EUA's statements in D.P.U. 19738 indicate that Unforeseen Appliances will still be included in the forecast and that only minimal adjustments will be made to reflect replacement of existing appliances. While electric heating conversions will be revised downward in the short run, average electric consumption of new dwellings will apparently not be changed, nor is there any indication that efficiency standards will be applied to Base Use. Neither the industrial nor commercial forecasts seem to be under revision.

Q: Do you believe that the new EUA forecast is reasonable?

A: From the available evidence it still appears to be overstated.

VIII. NEW ENGLAND GAS AND ELECTRIC ASSOCIATION

Q: On what documents do you base your evaluation of the NEGEA forecast?

A: My evaluation of the NEGEA forecast methodology is based on a review of the initial 1975 forecast, the two forecast supplements, the direct testimony of S. Robert Fox filed in DPU No. 19738, the direct and rebuttal testimony of Richard K. Byrne filed in EFSC No. 78-4, and the information filed in response to the Attorney General's information requests filed in the above two proceedings. I have also reviewed the briefs filed and the transcript of the EFSC No. 78-4, as well as the direct testimony of Richard K. Bryne and the information request responses filed by NEGEA in EFSC No. 77-4.

Q: Do you have any general criticisms of th NEGEA forecast methodology?

A: Yes. It appears that NEGEA did not base its forecast on any systematic consideration of underlying causal factors, such as price of electricity, price and availability of alternative fuels, conservation technologies, and economic conditions. Rather NEGEA claims that these factors have been taken into account subjectively and that they are contained in some unquantified and unquantifiable way in the responses of interviewees.

Heavy reliance upon survey data has major shortcomings. There is a problem of reviewability inherent in all subjective judgments; survey data also has intrinsic biases. The use of subjective opinions of many individuals does not guarantee a better forecast than the use of the subjective judgment of NEGEA's staff alone, especially considering (1) that the interviewees cannot be expected to expend the substantial time required for careful, well-thought-out and objective responses to complex questions concerning future energy use and demand and (2) that the responses are subject to interpretation by (and the biases of) the company. NEGEA may also have relied on interviewees for opinions beyond the scope of their expertise. For example, the residential forecast depends on the ability of town selectmen, land developers, bankers, etc. to predict the future energy picture and federal energy policies accurately enough to assess the likelihood of a move to an "all-electric economy". In the case of the industrial forecast, while it may be reasonable to expect an industrial customer to produce an accurate 3 to 4 year forecast of energy use, it seems unreasonable to accept the ten year forecasts of industrial customers, which depend on numerous assumptions about future technological and economic conditions. In addition, the NEGEA forecast depends on commercial and industrial customers to predict their contributions to system peak. For customers with fluctuating demands, this is an unreasonable methodology.

Reliance on the subjective judgments of many people magnifies the requirements for documentation. Both to make the interview data meaningful (to avoid adding apples and oranges) and to permit review, interviews must be conducted in a systematic and carefully documented manner. Written records must be kept of the questions asked, the background of the respondents, the responses to the questions, and the dates of the interviews. The questions must be carefully formulated and standardized to insure consistency in the responses. In addition, the interview must be structured to elicit from respondents the assumptions about causal factors, and the reasoning that underlie their responses. According to NEGEA's expert, Mr. Byrne, no such systematic records have been kept of the interview process.

The documentation of the interview results indicates deficiencies in NEGEA's derivation of numerical data and projections from the interview process. In addition, the documentation of the use of this numerical data is incomplete and contradictory. In general the justification of assumptions made is weak, and in several cases, the evidence that NEGEA offers in support of its assumptions indicates that the NEGEA forecast is too high.

Q: In what ways do you find the documentation of the use of data to be incomplete and contradictory?

A: In the case of the residential forecast, the discussion of the calculations done to produce the energy requirements forecast continually confuses the terminology (names of the variables). There are large steps missing from the description of the calculations. In the case of all three customer classes, worksheets provided indicate that the company has misrepresented the methodology used to derive the forecasts.

Q: Can you give us some examples of the inconsistent labeling of variables?

A: Yes. The documentation continually confuses the term "dwelling" with the term "customers", and therefore all variables that are a function of customer or dwelling number. For example, NEGEA's rebuttal testimony (p. 14-15) equates dwelling number to customer number. Also, the responses to the Attorney General's information request (see QR1, QR5, and QR6) confuse average KWH use per dwelling with average KWH per customer. The two terms are not synonymous. A comparison of (EFSC 78-4) Schedule IR-1 with Tables E-1 for New Bedford indicates that historically, the average use per non-electric customer always exceeds the general dwelling average KWH use.

Another source of confusion is the mixing-up of the terms "new dwelling units", "dwelling units on line", "dwelling starts" and "new dwelling permits".

Q: In what ways has the methodology been misrepresented?

A: The worksheet provided in response to the Attorney General's information request QC-2 (in EFSC 78-4) presents disaggregated projection figures (by the three divisions in the New Bedford service area) that appear to contradict the documentation of the methodology for the residential, commercial and industrial energy forecasts.

Because of the confusion of terminology, the source of the residential forecast is unclear.

The company's discussion of the methodology gives the impression that a customer number and an average use per customer projection were derived, and that total energy requirements were derived as the product of these numbers. But the worksheet indicates that the new customer usage and growth in existing customer usage were treated separately. The worksheet displays the following calculation:

$$\begin{array}{l}\text{(Total Energy Use}\\ \text{of Electric and}\\ \text{Non-Electric}\\ \text{customers)}\end{array} = \left[\begin{array}{l}\text{(Total Energy Use}\\ \text{in previous year)}*(1.02)\end{array}\right] + \left[\begin{array}{l}\text{(Energy Use Of}\\ \text{New Customers)}\end{array}\right]$$

In the case of the forecast for electric heating customers, it would appear, contrary to the documentation, that the forecast does not assume a 1.5% growth in average KWH use.

Only the energy use per new electric dwelling increases at 1.5% per year, and a dwelling is "new" for only the first year it is on line. The energy use per existing electric dwelling is asssumed to increase at a faster rate, 2% per year.

In the case of the commercial customer class, according to the rebuttal testimony of Mr. Byrne, the forecast was calculated for each town separately and summed to produce the divisional forecasts. According to the response to information request QC2 (EFSC 78-4):

> The information was used to develop a forecast of commercial energy requirements as follows:
>
> 1. For developments which have received all necessary agency approvals or which are under construction, specific energy requirements which were obtained during interviews were added into forecast requirements in the 1 to 3 year time frame as appropriate.
>
> 2. Estimated energy requirements of commercial developments which are at the proposal stage (in design without any or all agency approvals) were added into the forecast energy requirements in the 3 to 5 year time frame.
>
> 3. Forecast increases in energy requirements from commercial growth in the 5 to 10 year time frame have been based on the assumption that a new fixed relationship between commercial and residential energy sales will have become evident and that the percent commercial growth will match the percent residential growth.

The documentation gives the impression tht the forecast for the first 5 years was derived from customer and town-specific projections of energy requirements. If this were the case, one would expect to see uneven growth rates in the three division forecasts.

The worksheets provided in response of the Attorney General's information request QC2 (in EFSC 78-4) reveal growth rates with a surprisingly consistent pattern: "base load growth" is equal to 1% of the preceding year's total and is added to growth due to new construction. The following table exhibits the annual rates of growth due to new construction in each of New Bedford Gas and Edison Light Company's three divisions:

	C & V	Plymouth	N.B. Div.
1978	1%	1	1
1979	1	1	1
1980	2	2	1
1981	3	2.5	1.5
1982	4	3	2
1983	5	3.5	2.5
1984	5.5	4	2.5
1985	5.5	4.5	2.5
1986	5.5	4.5	2.5
1987	5.5	4.5	2.5
1988	5.5	4.5	3.5

For reasons of confidentiality, the only support offered for NEGEA's industrial forecast is an unsubstantial claim that the projection is based on the sum of the projections of individual industrial customers. Apparently Mr. Byrne first discussed with each industrial representative the important factors to consider in making a forecast, and then left each customer to his own devices to generate a ten year forecast.

According to the response to information request QC2 (in EFSC 78-4), the industrial growth rates for 1979 through 1987 for the three divisions in the New Bedford Gas and Edison Light Company service area are as follows:

	New Bedford	Plymouth	Cape and Vineyard
1979	2.0	0	6.9
1980	2.5	0	3.0
1981	3.0	0	3.0
1982	3.8	0	3.0
1983	4.0	0	3.0
1984	4.0	0	3.0
1985	4.0	0	3.0
1986	4.0	0	3.0
1987	4.0	0	3.0

It is another unlikely coincidence that independent forecasts of industries purchasing different amounts of electricity and growing at different rates should sum to forecasts with constant annual growth rates over a period of 5 or more consecutive years.

Q: Where do you see evidence that NEGEA has overestimated the future energy requirements?

A: The direct evidence is limited to the residential portion of the forecast. The review of the commercial and industrial forecast cannot be evaluated because the underlying assumptions are not quantified. In the residential forecast, the company presents evidence that indicates, if anything, that the projections of customer numbers, electric heating penetration rates, and average KWH usage per electric and non-electric customer are all too high.

Q: What are your criticisms of the projections of customer number?

A: NEGEA does not provide the sources of its new dwelling permit projections nor its dwelling start projections derived therefrom. Instead NEGEA presents (as an after-the-fact verification), a comparison between the new dwelling start projection and independent state and regional ageny population projections. Indeed the analysis that NEGEA presents as verification of its figures, actually indicates that NEGEA's new dwellings forecast is an overestimate. Table NE-I presents a comparison of projections of population growth derived from the two sources. The numbers suggest that the use of NEGEA's new dwelling forecast would overestimate growth in population by more than 20%, implying an overestimate of the increase in New Bedford residential consumption to a comparable degree.

NEGEA's claim, that a "correlation" between population projections verifies its housing forecast, is unfounded. A comparison of population <u>totals</u> in this case is misleading. The population totals are bound to be similar. Population growth is only a small percentage of the existing population base, and differences in estimates of growth will appear as an even smaller percentage of

TABLE NE-1:

COMPARISON OF NEGEA HOUSING FORECAST AND THE STATE AGENCY POPULATION PORJECTIONS

Service Division	Population increase 1976-1985: NEGEA FORECAST(a)	Population increase 1976-1985: CCPEDC Forecast (or most probable est.)	% difference (b)
Cape & Vineyard	42,770	37,792	13.2
New Bedford Div.	19,576	13,927	40.6
Plymouth	38,511	30,333	27.0
TOTAL	100,857	82,052	22.9

Notes:

(a) Source: Response to the Attorney General's First Information Request, Schedules 1R, 2, 3, 4.

(b) %difference = (NEGEA forecast - CCPEDC forecast)/~~NEGEA~~ CCPEDC forecast.

population total. But it is the estimates of changes in population that we are interested in here.

Q: What are your criticisms of the projections of electric heat penetration rates?

A: The Second Supplement to NEGEA's forecast assumes a rapid five-percentage point growth in penetration rates starting in 1981. These high penetration rates are based on the assumption of a move to an all electric economy. According to the response to QR-10 (in EFSC 78-4), NEGEA projected that:

> increases in electric heat penetration rates would start to occur in 3 to 4 years, which indicates the time period the public feels is still necessary before a firm national all-electric energy policy can be implemented.

The company claims to have two lines of support for these penetrations rate projections, the interviews and the historical data. Neither provides adequate support. The interview data, if it has any validity, actually contradicts the all-electric economy assumption.

Q: What are your objections to the interview data?

A: The interview data completely contradicts the assumptions made in the NEGEA forecast.

In the response to the Attorney General's information requests QR-8 (EFSC 78-4), NEGEA reported the following distribution of response:

30 percent - Strong supporters of Electric Heat
10 percent - Mild supporters of Electric Heat
10 percent - No pro or con attitude toward Electric Heat
10 percent - Mildly against Electric Heat
40 percent - Adamantly against Electric Heat

It appears that among the interviewees there were substantial differences of opinion, and that NEGEA took its position at one extreme. This case illustrates the need for careful documentation of interview responses and of the interpretation of these responses by company forecasters.

Q: What is your evaluation of the company claim that the historical data supports the heating penetration rate projections?

A: According to Mr. Byrne, the historical evidence is as presented in Schedules 2, 3, and 4 of his direct testimony in EFSC 78-4. Schedules 2, 3 and 4 present penetration rates as the rates of two incommensurate variables, the number of electrically heated <u>new dwellings on line</u> divided by the total number of <u>new dwelling permits issued</u>. As Table NE-2 shows, the historical relationship between the number of electrically heated new dwellings on line and the number of new dwelling permits issued, is volatile. From the data provided in Schedules 2, 3 and 4, it appears that for some reason high penetration rates tend to occur in years when there were relatively small numbers of new

TABLE NE-2:

HISTORICAL ELECTRICAL HEAT PENETRATION RATES IN THE THREE NEW BEDFORD DIVISIONS FOR THE YEARS 1970-1976

DIVISION	Electric Heat Penetration rates %(a) RANGE	AVERAGE(b)
Cape & Vineyard	6.9 to 68.5	26.9
Plymouth	9.4 to 86.6	31.8
New Bedford Div.	1.7 to 27.9	16.6

Notes:

(a) E.F.S.C. 78-4 Schedules 2, 3, and 4 (Revision 1)

(b) "Average" equals the total number of new electric homes on line, summed over the years 1970 to 1976, divided by the total number of new dwelling permits, summed over the same time period.

dwelling permits issued. Given the sizeable impact of a projected rise in penetration rates on the residential forecast, this component of the forecast merits careful analysis. Such an analysis should consider, in addition to various scenarios of future national energy policy: (1) past and future variations in the mix of dwelling size and type (e.g. single family houses, duplexes, apartments, mobile homes and summer homes); (2) the impact of energy policies on penetration rates in the past (in particular, the impact of the promotion of electric heating by utility companies through rates, advertising, etc.); (3) changes in the lag between the time that a permit is issued and the time that the dwelling is occupied; and (4) changes in the rates of demolitions or vacancies of the existing housing stock.

Q: How does the data presented by NEGEA indicate that their projection of average use per heating customer is too high?

A: The data for all three divisions of the New Bedford service area that NEGEA presents to support the projected 1.5% growth in average use per new electric dwelling show a steady decline in the average consumption of the heating customer for the past 4 years. According to the Company "[t]he results indicate that further decreases are very unlikely". By itself, a steadily declining consumption does not imply increases in consumption in the future. It is true that there must be some limit to the reductions electric heating customers can accomplish, but NEGEA offers no evidence that customers have in fact reached that lower limit. Neither is there anything in the data to support the projection of an <u>increase</u> in consumption.

NEGEA based this 1.5% growth figure on a rather pessimistic view of the potential for conservation, on an assumption that conservation is a transitory phenomenon. In the response to the Attorney General's Information Request QR6 in EFSC 78-4, NEGEA gives the following implausible argument:

> It is our judgment based on opinions and attitudes gathered during the interview process that the average use will soon begin to increase. We believe that the consumer has responded to the extent reasonably possible to the rising cost of electricity as exemplified by the leveling off of the average KWH usage. It is our judgment that decreases in KWH use resulting from such factors as increases in appliance efficiency and improved home insulation will be offset, particularly in the short range, by additions dependence on wood stoves, additions to existing homes, and the addition of larger new homes with electric heat.

The idea that people will make additions to their homes, stop using wood-burning stoves, and in general consume as they did before the electric price increases, contradicts assumptions made in other parts of the forecast. In particular, it contradicts the assumption of an all-electric economy, the assumption on which the rapid growth in penetration rates is based.

The expectation of a transition to an all-electric economy must be predicated on a pessimistic view of the national energy situation and on a scenario of high prices and low availability of alternative fuels. In such a high-cost world, we would expect that the new electric home owners would be less willing electricity consumers; and more generally that people would be less and less able to afford an energy intensive life-style. We would not expect to see people building bigger homes and buying more electric appliances. It would be at least as likely that more people would invest in wood-burning stoves, solar home heating, and other conservation measures.

Certainly none of the evidence offered by NEGEA would justify inflating average use per <u>existing</u> customer by 2% per year.

Q: What in NEGEA's documentation indicates that the projections of average energy consumption by non-heating customers are too high?

A: In responses to the Attorney General's information requests, the company gave two contradictory descriptions of the derivation of these projections. According to the response to QR-1 (in EFSC 78-4), average use of non-heating customers of both New Bedford and Cambridge service areas were:

"based on straight line projections of historical trends, judgmentally adjusted to consider information gathered during interviews."

A "straight line projection" of historical trends commonly refers to a linear regression with respect to time. As Table NE-3 shows, the NEGEA projection of the change in average use by 1987 exceeds, by a substantial amount, the projection derived from the linear regression of 1971 to 1976 data.

According to the response to QR-6 (in EFSC 78-4), the average use projection is based not on a straight line projection, but on a subjective adjustment to the latest year's (1976) growth figures. It is unreasonable to base a projection on a one year data base. By the same argument the third supplement to the forecast should be based on the 1977 growth figures of 0.1, 0.8, and 2.0 for the Cape and Vineyard, Plymouth, and New Bedford divisions respectively.

Q: What do you conclude from the foregoing discussion?

A: I conclude that NEGEA's forecast is unreliable and should be given little credence by anyone.

Q: Does this conclude your testimony on the NEGEA forecast?

A: Yes, it does.

TABLE NE-3:

AVERAGE USE OF NON-HEATING CUSTOMERS IN NEW BEDFORD AND CAMBRIDGE: A COMPARISON OF THE NEGEA FORECAST AND A LINEAR REGRESSION OF 1971 TO 1976 DATA

	NEGEA FORECAST		LINEAR REGRESSION	
Service Area	1987 avg. Consumption (kwh)	%growth 1976-1987	1987 avg. Consumption (kwh)	%growth 1976-1987
New Bedford	6686	23%	6278	15.7%
Cambridge	3454	11%	3195.5	2.7%

IX. THE MASSACHUSETTS MUNICIPAL WHOLESALE ELECTRIC COMPANY

Q: Please define the scope of your testimony.

A: I have made an evaluation of the MMWEC forecast methodology. This evaluation is based on a review of the initial 1975 forecast, the first two forecast supplements, the information filed in response to requests made by the Attorney General and by the Energy Facilities Siting Council, and the tapes of the pre-hearing conference in EFSC 78-1. In this review, I focused largely on the residential forecasts, but the criticisms I make in my testimony apply more generally to all of the customer classes.

MMWEC's incomplete, inaccurate, and at times inconsistent description of their methodology makes a fair evaluation of their methodology difficult and time-consuming. If MMWEC were to state clearly for each of the 30 municipal departments the input data used, the numbers and the time periods, the assumptions made, and the calculations done, then a reviewer could make conclusions concerning the reasonableness of the various components of the methodology, and in the areas of disagreement, could test the sensitivity of the forecast results to differing assumptions. I found that before I could evaluate the methodology, I had to make many trial calculations just to attempt to determine the methodology actually used.

The initial 1975 filing somewhat defines a methodology: it describes data, collected from responses to the Questionnaire, the use of the algorithm (as described in Appendix B10 of MMWEC's Response to EFSC Information Request dated July 12, 1978) for the projected years 1976 to 1980 (or 1981), and the use of some annual compound average growth rate to project consumption past the year 1980 (or 1981). Even for the 1975 forecast, however, I was not able to reproduce results, perhaps because the output of this algorithm was subject to undocumented "subjective" adjustments. The 1977 and 1978 forecasts are entirely undocumented subjective adjustments of the initial forecast; as a result, the methodology is unreviewable. I must, therefore, base my testimony on the methodology and data relating to the initial 1975 forecast. But to the extent that the 1977 and 1978 forecasts are based on the 1975 methodology (as is MMWEC's claim), my criticisms apply to the supplements as well.

Q: Do you have any criticisms of the statistical methodology used in the forecast algorithm?

A: Yes. A major weakness in the algorithm is its dependence on the trending of historical data. Simple time trending is a generally discredited projection technique. A recent EFSC Decision states:

> Trend line analysis, even coupled with judgmental modifications to the line, can no longer be said to be a valid forecast methodology.[1]

For purposes of demand forecasting, time trend analysis requires too simplified a view of the world. Trend line analysis uses time as a surrogate for the various economic, weather and demographic factors that actually affect energy consumption. The simple time trend model must assume that throughout the historical and projected time period chosen either (1) that these factors will always happen to change in such a way as to have no net effect on the growth rate of annual energy consumption or (2) that energy consumption is unaffected by such factors.

Q: Doesn't the MMWEC methodology take into account some of these true causal factors by incorporating into the forecast subjective estimates of historical and projected conservation?

A: Not in a reasonable manner. The MMWEC methodology does not systematically identify and project trends that may significantly affect growth in energy consumption. Nor does it attempt to quantify the impacts of these significant factors on the rate of growth of consumption,

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[1] Boston Edison Company: Initial Demand and Energy Forecast and its First Supplement thereto. E.F.S.C. No. 76-12 and 77-12.

even on a crude level. Instead, MMWEC defers to their department managers' "intimate knowledge" (or more correctly, to a line drawn on a graph accompanied by a non-rigorous discussion of the reasoning behind it). The end result is that MMWEC incorporates into the forecast a pessimism about the potential for conservation. This pessimism is unsubstantiated by MMWEC and is contrary to experience.

Q: You have criticized the conservation input because of the unreviewability of the subjective estimates. Do you have any reason to doubt the ability of managers to estimate the extent of conservation in their service area?

A: Yes, I do. The shortcomings of the conservation input are inherent in MMWEC's very definition of the term. There is an ambiguity in the formulation of the managers' task that makes their responses meaningless. MMWEC defines conservation as:

> That amount of energy which was not used by an existing customer because of a conscious act or willingness to forego or modify a previously <u>normal use</u> of an existing electric appliance or device or the product of such an appliance or device. Such as lowering of heating thermostats, reduction of lighting levels and periods of illumination, the use of cold water for clothes washing, the delay or reduction of use of existing air conditioners and installation of insulation materials. (MMWEC Response to AG Information Request 7/27/78; Question 6. Emphasis added.)

Clearly, this definition implies that conservation cannot be estimated unless the "normal use" is well-defined and known. Usage is affected by a multitude of variables, such as the cost of electricity, the cost of other fuels, the cost of conservation, population size and structure, family size, housing mix, household income, economic activity, and appliance stocks. "Normal use" presupposes "normal" levels of at least some of these variables; it is not clear how each manager interpreted the concept of normality. Even if the concept were rigorously defined (e.g., "all variables except population remain at 1970 levels"), it is hard to see how a manger could isolate the effects of the variables which were to be normalized, unless a complex model (econometric and/or engineering) were formulated for each town. Without such a model, it is only possible to speculate about the extent to which managers' definition of reality reflect conditions of the early 1970's, which no longer exist, an extrapolation of the trends of the late sixties to values which have never existed, or something else.

Q: Even assuming the subjective estimates of the managers are accurate, will the algorithm correctly incorporate that information into the forecast?

A: No, it will not. For any projected conservation (CONS) greater than zero the algorithm (as described in

Appendix B-10 of the response to EFSC Information Request of July 12, 1978) will produce an inflated forecast. This error occurs because the way that the algorithm factors in the CONS imput[2] is inconsistent with the definition of the conservation ratio in the 1975 Questionnaire.

Q: Please describe this inconsistency.

A: As the question is posed in the 1975 questionnaire, the CONS variable is defined as a percentage of total electricity consumption (TEC). For historical years, the algorithm does use the CONS variable as defined. It derives BASE from TEC by subtracting out seasonal load (TSU) and adding CONS x TEC:

$$\text{BASE} = \text{TEC} - \text{TSU} + (\text{CONS} \times \text{TEC})$$

However, for projected years the algorithm uses CONS as if it were a percentage of BASE, not of TEC:

$$\text{TEC} = \text{BASE} + \text{TSU} - (\text{CONS} \times \text{BASE}).$$

The forecast of consumption k years into the future is thus incorrectly inflated by the amount CONS x (TEC - BASE). To make the algorithm consistent with the questionnaire, equation 1-A-4-k of Appendix B-10, page xi should be replaced by:

$$\text{TEC}_k^{\text{PER}} = \frac{\text{BASE}_k^{\text{PER}} + \text{TSU}_k^{\text{PER}}}{1 + \text{CONS}_k}$$

[2] See Appendix B-10, page B-10-xii.

Q: Suppose the algorithm were corrected as you suggested. Can you say anthing about the effect of the CONS input on the energy forecast?

A: As I stated before, a pessimistic viewpoint about the potential for conservation is inherent in the data, or rather in the algorithm's interpretation of the data. Incorporating conservation into the forecast algorithm had the effect of adjusting upward the consumption data so as to erase the dampening effect of the oil embargo from the data set. Ironically, the net effect of the conservation input tends to inflate the projected growth rate of energy consumption above that actually experienced during the historical 1970 to 1975 time period.

Q: How did you come to this conclusion?

A: First of all, I examined the energy reductions per customer (listed in Table AG-7A of the MMWEC response to AG request of July 12, 1978 in EFSC 78-1) in conjunction with the managers' responses to question 6 of the 1975 Questionnaire. I could distinguish four types of responses regarding residential conservation:

> <u>TYPE A</u>: The manager estimates zero conservation throughtout the historical and projected years. Nine departments[3] fall in this category.

> <u>TYPE B</u>: The manager assumes that although some conservation did occur in at least one of the

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[3] They are Ashburnham, Boylston, Georgetown, Paxton, Sterling, Templeton, Wakefield, West Boylston, and Mansfield.

historical years 1973-1975, no conservation will occur in the future. Ten MMWEC members[4/] fall into this category.

TYPE C: The manager assumes positive conservation in the historical years 1973-1975; and conservation during projected years, but dwindling to zero by 1980. Five MMWEC[5/] municipals fall in this category.

TYPE D: The manager assumes increasing CONS during the historical period and a constant CONS continuing throughout the forecast period 1976-1980. Only five[6/] out of the 29 departments showed this moderate optimism.

Q: How can an assumption that consumers are willing to conserve result in a higher forecast than would result from an assumption of zero conservation?

A: The effect of the CONS input on the forecast depends not only on the magnitude of the CONS variable, but also on the pattern of changes in CONS over time. For example, it is a simple exercise to show that when CONS is a constant proportion of TEC throughout the historical and projected years, then the TEC projection will be invariant with respect to the magnitude of CONS.

4/They are Chicopee, Danvers, Westfield, Groton, Hingham, Holden, Ipswich, Littleton, Middleboro, and North Attleboro.

5/They are Belmont, Reading, South Hadley, Hudson and Marblehead.

6/They are Shrewsbury, Peabody, Holyoke, Hull and Middleton.

Intuitively, it can be seen that the 15 projections with CONS inputs of types B and C will give inflated forecasts. Adding conservation into the historical data will raise the slope of the trend line that will be used to project BASE. With a lower and declining, or zero, CONS in the projected years, less energy will be subtracted out of the BASE forecast than was added into the historical trend. In addition, projecting a <u>decline</u> in the conservation ratio inflates the annual growth rate of projected consumption. For example, suppose it is assumed that, from some year to the next, the conservation ratio declines from 5% to 4%. The algorithm adds this 1% difference to the already projected growth rate.

Essentially, the methodology assumes that conservation is a trasitory phenomenon, that consumers will turn more lights back on, turn in their new appliances for less efficient ones, and remove their insulation.

Q: Using actual data from one of the department forecasts, can you illustrate the implications of a Type C response?

A: Yes, I will use Belmont as an illustration. I derived two forecasts, assuming two different conservation scenarios: (1) conservation does not occur throughout the historical and projected years (the zero conservation scenario) and (2) energy reductions occur as estimated by the Belmont Manager and reported in Appendix AG-7A (the positive conservation scenario). In making these calculations, I followed the MMWEC algorithm, with two exceptions: for lack of data, I assumed that Belmont's residential class has no segregated members and no seasonal load. These assumptions will not affect the qualitative results. I derived the time trends from the historical period 1970 to 1974. The data and calculations are shown in Table 1 and 2.

Table 1

Data: Total Energy Conserved Per Customer and Number of Customers

Year	Energy Reduction[1] (KWH/Customer)	Number of[2] Customers
1970	0	9177
1971	0	9185
1972	0	9193
1973	114.9	9234
1974	219.9	9250
1975	173.6	9243
1976	148.0	9299
1977	121.1	9322
1978	92.1	9346
1979	63.2	9370
1980	0	9394

1. Source: Appendix AG-7A

2. Source: For the period 1970-1975, Appendix B4 and for the period 1976-1980, Appendix B2

We can see from Table 2 that the positive conservation scenario gives a 1980 projected energy consumption of 59,267 MWH, while the no-conservatiuon scenario gives a 1980 projection of 55,162 MWH. Thus, we see that assuming positive conservation can indeed result in a higher forecast, 7.4% higher in this case.

Table 2: A Comparison of Two Forecasts Varying the Conservation Input

	Zero-Conservation Scenario		Positive Conservation Scenario			
	Class Total (mwh)	kwh per customer	Total mwh consumption + conservation (mwh)	kwh per customer	subtracting out projected energy reductions	Projected Total Energy Consumption
1970	46,908	5111	46,908	5111		
1971	48,254	5254	48,254	5254		
1972	50,250	5466	50,250	5466		
1973	52,817	5720	51,646	5835		
1974	49,612	5363	51,908	5583		
1975	50,303	5442		5616		
1976	52,223	5616		5866	5718	53,172
1977		5680		5976	5828	54,329
1978		5744		6087	5994	56,020
1979		5808		6198	6135	57,632
1980	$\boxed{55,162}$	5872		6309	6309	$\boxed{59,267}$

MMWEC's use of the average compound growth rate to project consumption past 1980 will magnify the difference between these two forecasts. This effect occurs because a CONS pattern that produces a steeper trend line for projecting 1976 to 1980 consumption also increases the compound average growth rate for those years. The assumption of declining conservation also adds to the 1976 to 1980 average growth rate. For example, suppose MMWEC were to use the 1976 to 1980 average compound growth rate from this residential forecast to project energy consumption to 1985. Then the no-conservation scenario, with its 1976 to 1980 average annual growth rate of 1.38%, would produce a 1985 projection of 59,074 MWH, whereas the positive conservation scenario, with its 1976 to 1980 average annual growth rate of 2.75%, would produce a 1985 projection of 67,876 MWH. The positive conservation projection of 1985 total residential consumption would be 15% higher than the no-conservation forecast.

Q: Would you comment on the reasonableness of this methodology and the reasonableness of the results?

A: The forecast is highly sensitive to the CONS input. Basically, the CONS input can be used to fill in any discontinuities in the historical trend, and, in the case of 15 out of 29 projections, the CONS input <u>has</u> served the purpose of erasing much of the experience of the oil embargo years from the data. The result, intended or not, is a smoother historical growth path and a higher trend line. At the very least, the use of subjective estimates of a variable as important as conservation should be (1) accompanied by a clear statement and justification of the assumptions and reasoning underlying each estimate and (2) based on a clear definition of the term "conservation" linked to a clear understanding of how the algorithm factors the variable into this forecast.

I attempted to piece together from the initial forecast, supplements, and information responses, some idea of whether the CONS input and its effect on the forecast are indeed consistent with MMWEC's conceptions about the potential for conservation, and its impact on growth in consumption and its sensitivity to increasing electricity prices. I turned up a collection of vague, inaccurate and contradictory statements that casts further doubt on the validity of the methodology. I found, for example, the following sentences on Section II, page 6 of the initial forecast:

> The magnitude of energy conservation as experienced during the 1973-1974 period was reduced during the latter part of the forecast. It was assumed, however, that a general conservation awareness would prevail throughout this period resulting in some reduced consumption.

This statement is inaccurate; nineteen out of 29 departments assumed zero conservation by the residential class throughout the entire forecast period, and 17 out of 29 departments assumed zero conservation by all other classes for the same time period. As another example, section II, page 3 of the MMWEC initial forecast contains the following statement:

> The price of electricity is projected to continue its current price climb for the period of this forecast. This price increase, however, is not expected to dampen completely the demand for, and use of, electricity during the forecast period.

Here MMWEC appears to be saying that price, although it will not "completely dampen" demand, will exert <u>some</u> downward pressure on growth in consumption. Perhaps MMWEC was unaware of the inflationary effect of the conservation input on their forecast. That appears to be the case from the following inaccurate description by MMWEC of the conservation input, taken from Section 7 of the response to the EFSC information request (in EFSC 78-1) dated August 7, 1978:

> The conservation effect is trended forward based on the manager's response to question 6 and is subtracted from the projected base use per customer in the class being considered. This results in a load forecast that is adjusted downward for the estimated impact of energy conservation.

The methodology's use of the conservation input appears to conflict with MMWEC's own conceptions of the potential for conservation. MMWEC's implicit assumption that conservation is short-term and transitory also conflicts with the results of econometric studies and with common sense. The conservation input appears to ignore increasing appliance and machinery efficiencies, the likelihood of federally imposed building codes and appliance efficiency standards, increasing consumer awareness and access to funds for insulation and weather stripping, and the ability of utility companies themselves to encourage or require conservation through load management techniques and revisions of rate structure (e.g., flattened rates and time of day rates).

Furthermore, customers' adjustments in their electric consumption take time. Econometric studies of electric demand consistently find that long-run price elasticity[7/] exceeds short-run price elasticity. Their results would imply that the effects of the 1973 and 1974 price increases will be felt <u>increasingly</u> throughout the time period of MMWEC's forecast, in addition to the effects of

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7/Elasticity is defined as the percentage change in consumption divided by the percentage change in price.

further price changes. In contrast, MMWEC assumes that these effects decrease over time.

Table 3 of Chernick & Celler in Phase I of this Case summarizes a set of 12 studies that estimate long-run and short-run price elasticities. Included on this table are all recent studies that were considered important enough to be included in three surveys of the literature, Taylor (1975), Levy (1973), and Sharefkin (1974). This matter was discussed in detail at great length in Phase I of this case.

Q: Do you have any comments on any other components of the methodology?

A: Yes. With regard to the seasonal load component, the separate trending of the two components of energy consumption, seasonal load and base load, is largely window-dressing. The forecast of total energy demand will be totally independent of projected growth in seasonal load (1)where the manager assumed that no conservation will occur in any of the projected years (response types A and B, discussed on page ~~6~~ 133 above) or (2) if the algorithm is revised (as I suggested previously) to correct for the inconsistent treatment of the CONS variable.

Q: Do you have any concluding remarks on the MMWEC forecast?

A: Yes, I do. EFSC regulations and M.G.L. c. 164, §69J require the forecast to be based on substantially accurate historical information and reasonable statistical projection methods. My review of the MMWEC forecast, forecast supplements, and responses to the information requests lead me to believe that the MMWEC methodology fails on both counts.

Q: Does this conclude your testimony on the MMWEC forecast?

A: Yes, it does.

X. CENTRAL VERMONT PUBLIC SERVICE CORPORATION

Q: What materials were available to you in preparing this portion of your testimony?

A: Since neither BECO nor NEPOOL had a copy of Central Vermont Public Service Corporation's (CVPS) forecast methodology, we had to turn to other sources. We were able to obtain copies of certain CVPS responses to data requests of the Vermont Low Income Advocacy Council (VLIAC) in a rate case (PSB Docket No. 4230), late in 1977. Most of the relevant material consists of a document entitled "Residential Sales 1977-78 Forecasting Methodology" although there is also a "Methodology for Forecasting Load Growth by Customer Classification", which covers other classes. Judging from the dates involved, we assumed that this document represented CVPS' input to the 1/1/78 Load and Capacity Report; since Vermont is only reported as a group in the L&C Report, we could not confirm this. As Table CV-I shows, the L&C Report shows a faster growth rate for Vermont than is shown in this CVPS forecast.

Q: Please describe the structure of the CVPS forecast.

A: The Residential Forecast is by far the most complex portion of the methodology, involving Vermont population, Vermont households, CVPS customer number, an adjustment for vacation homes, appliance saturation and average usage per appliance. Industrial, commercial, government, and lighting sales are projected by much simpler methods.

TABLE CV-I FORECAST PEAK
AS A PERCENTAGE OF VERMONT GROUP PEAK

Year (January)	Forecast Vermont Peak (1)	CVPS Peak (2)	CVPS as % of Vermont
1978	794 NW	381 NW	48%
1979	831	395	48
1980	864	408	47
1981	895	418	47
1982	932	428	46
1983	966	436	45
1984	997	445	45
1985	1041	454	44
1986	1076	467	43
1987	1112	481	43
1978-87 growth rate	3.81%	2.62%	

Notes:

(1) From 1978 NEPOOL Load and Capcity Report
(2) From Response to ULIAC Question 12, PSB 4230

Q: Please describe the industrial methodology.

A: Apparently, CVPS estimated the short-term (2 to 3 year) planned load additions by their "industrial" customers (including at least large stores) and then extrapolated this estimated annual growth linearly through the forecast period. In addition to being extremely simplistic, this approach also counts only additions, without any recognition of deletions. For example, both new construction and occupation of currently vacant space are counted, but no allowance appears to be made for new vacancies in existing or new space. Installation of additional equipment is considered, but not replacement with more efficient equipment. And no conservation measures, in construction or in operation, are even considered.

Q: Is the commercial forecast methodology any better?

A: Apparently not. It seems that CVPS based the commercial forecast on a comparison with national utility forecasts, on the belief that rising oil prices will increase electric heating of commercial space, and on the observation that "(w)ith the continuing improvement of the economy,. . . reconstruction, modernization and new buildings should really catch hold during 1977-1986."

Even if CVPS had been able to relate these factors directly to their forecast, their relevance is at best marginal. The national utility forecasts have tended to be grossly overstated since the early 1970's; even if they were accurate, the growth in sales for air conditioning in retirement areas such as Florida and Arizona is not directly related to the causes of electric consumption in Vermont. Rising oil prices should be accompanied by rising electric prices; conservation and cogeneration make far more sense than electrification under these circumstances. And CVPS seems to be assuming that commercial growth will be at least as vigorous over the entire next decade as it was during the recovery from the recession of the middle 1970's. In fact, while the forecast text claims that the commercial growth rate is 4% in 1977 and 5% beyond, the attached tables indicate that the forecast growth rate increases to 7% in 1986.

Overall, then, the commercial forecast is arbitrary and unfounded, as is the industrial forecast.

Q: Please describe the residential customer number methodology.

A: CVPS starts with a Vermont Department of Health forecast of 1% population growth for the state and applies a slightly rising overall headship rate to determine Vermont households. A fraction of these households are then allocated to CVPS. This fraction fell steadily from

55.1% in 1973 to 54% in 1976, and CVPS predicts "slowing of economic growth in the CVPS service area compared to the Burlington metropolitan area", yet the CVPS fraction of Vermont households is projected to <u>rise</u> to 54.5% over the forecast period. Once again, CVPS contradicts itself.

Q: Please describe CVPS' computation of average residential use.

A: CVPS forecasts saturations of several appliances, based in part on trending the results of 1971 and 1976 saturation surveys, in part on other data (for example, dryer saturations are based on unspecified clothes washer saturations), and in part on subjective adjustments. These projections are multipled by a constant KWH/unit to forecast annual sales. No adjustments are made for appliance efficiency, for any other conservation measure, or for family size.

CVPS has found that it seriously overestimated consumption in the past, due to sampling errors (the 12% of their residential customers who are seasonal are apparently under-represented in the saturation surveys and obviously have lower annual use per appliance) and errors in average usage estimation. These errors increased after rate increases in the middle 1970's, and CVPS attributes the difference to conservation. However, CVPS assumes that the errors will decrease in the future to the lowest historical values, obliterating all such conservation effects. Therefore, CVPS is implicitly assuming that the long-run price elasticity for electricity is zero and that the conservation measures of the last few years will be reversed (turning up thermostats, ripping out insulation, etc.).

Q: Does CVPS incorporate any improvements in appliance efficiency?

A: No. In fact, CVPS explicitly states that they have not recognized energy conservation efforts in their forecast.

Q: What other factors influence the residential forecast?

A: All residential use is projected to increase at .5% annually due to unforeseen appliances. In addition, large increases in space heating penetration are forecast, for baseboard heat (for which average consumption remains constant, despite smaller family size, rising electric costs, better home design, etc.), storage heat (which is amazingly projected to consume twice the energy of baseboard heat) and an undefined heating category (which consumes 30% more than baseboard). Non-heating use by heating customers is also assumed to be much higher than average. On the whole, electric heating projections appear to be seriously inflated and substantially undocumented.

XI. THE NEPOOL MODEL

Q: What materials have you reviewed in preparing this portion of your testimony?

A: We had available only the Report on Model for Long-Range Forecasting of Electric Energy and Demand to the New England Power Pool by NEPOOL Load Forecasting Task force and Batelle-Columbus (6/30/77), hereinafter referred to as "the Report". Our requests for further information, both through the EUA forecast case (EFSC 78-33) and through the current case, have been rebuffed.

Q: Do you have any special reservations about reviewing the NEPOOL model based on the Report?

A: Yes. The Report raises almost as many questions as it answers, due to the nature and style of the document:

1. Many relationships are estimated from data which are not provided. In many cases, the exclusion of the data is understandable, considering its bulk, but makes discovery even more important than in relatively self-contained forecasts.

2. Selected functional forms are presented, without the rejected alternatives, a discussions of the criteria for choice, or goodness-of-fit measures.

3. In some cases, it is unclear whether the data behind the relationship is cross-sectional or time series; national, regional, statewide, or by service area; aggregate or disaggregated.

Q: Does CVPS incorporate any effects of electric price or equipment efficiency in any class?

A: Except for some recognition of increasing lamp efficiency in the street lighting sector, CVPS completely ignores all conservation, whether induced by price or technology.

Q: Does this conclude your testimony on CVPS' forecast?

A: Yes.

4. Many important inputs are user-specified, and are therefore not presented in the Report.

5. We know that the model has undergone testing and revision since publication of the Report, but we do not know what was revised, or how. Nor do we know whether the model ever existed in exactly the form presented in the Report.

6. At this writing, the only results available are for winter peak demand. Such important intermediate results as sales by class (or end use or state), appliance saturations, employment, family size, income, and value added have not been reported. part

7. Several important sources on which the model is based are unpublished NEPOOL/Battelle products, testimony in other cases, comments made in panel discussions at industry conferences, and the like. Considering the sophistication of the NEPOOL model, these omissions prevent any thorough review of the model.

Q: Please describe the structure of the model.

A: Conceptually, the NEPOOL model is divided into seven major sections:

1. The demographic submodule, in which population, migration, and labor force participation are determined;

2. The employment submodule, in which employment by industry type is determined;

3. An interface between the economic/demographic module and the power module, which sets household number, housing type mix, and income distribution;

4. The residential power submodule, which determines appliance saturations and average use patterns;

5. The industrial power submodule which determines value added and KWH/$ value added for each SIC;

6. The commercial power submodule, which determines base load consumption per employee, saturation of electric space heating and cooling, and weather sensitive load for each commercial category; and

7. The miscellaneous power submodule, which forecasts such uses as street lighting, agriculture, mining, railroads, utility use, and losses.

We will attempt to review briefly a sampling of the deficiencies in each section.

Q: Please discuss the deficiencies in the demographic submodule.

A: The migration equations have some serious flaws. Migration rates are postulated as a linear function of the differential betwen local and national unemployment. Rather than estimating these relationships over time for each state, NEPOOL estimates across the New England states for the period 1965 to 1970. (Actually, unemployment rates are averaged for 1966 to 1969, which seems strange, considering the inevitable delay in migration decisions.) What is really being measured, then, is the attractiveness of Massachusetts, or Vermont, relative to the rest of the country in the late 1960's, rather than the effects of changing unemployment rates. This "cross-sectional fallacy" can be quite dangerous; Figure NP-1 illustrates how even the sign of the cross-sectional relationship can be different from that of the relationship which holds for each state. As a result of the estimation procedure, neither national unemployment nor time-dependent changes can directly effect the migration rate.

Other problems appear in the migration section. The equations for one quarter of the age-sex groups are not statistically significant. The model estimates are only for civilian migration; military migration, which has a sizable impact on a few age groups, is apparently ignored. It does not appear that the ratio of migrating children to migrating adults has been revised to account for smaller future families. It is not clear how the model assigns children of various age groups to migrating adults nor whether 20-year-old immigrants can bring 14-year-old children with them. (Note that age matters only for those children old enough to form their own households in the forecast periods.) Migration of people over 64 remains at average 1960-70 levels, without any allowance for changes related to income, time, living costs, or demographics.

The reported sensitivity analyses are ambiguous. It is unclear whether the slope coefficients were changed in absolute value or actual level; whether the intercepts, the

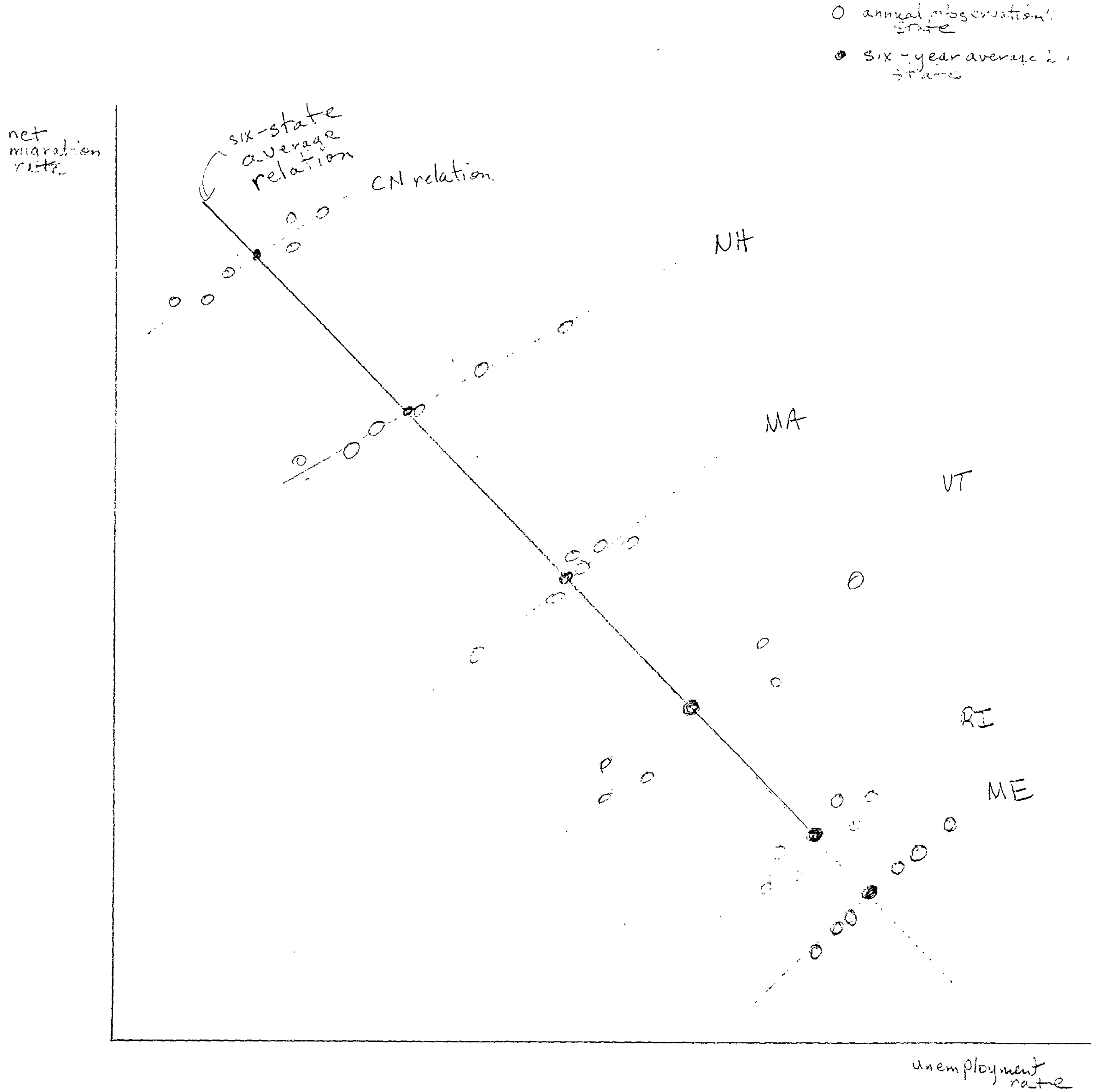

Figure NP-1: Example of the Cross-sectional Fallacy means, or some other point was held constant when the slopes were increased; and what NEPOOL actually did when it "dropped the error term".

Q: Do similar errors occur in the estimation of labor force participation rates?

A: Yes. This rate (LFPR) is estimated as a linear function of jobs per capita and of age group as a fraction of population. Again, cross-sectional data across states is used as if it were time-series data. NEPOOL even explains the disappointing results they achieve by invoking "good economic times" and "hard times" when the data <u>really</u> represents various states in 1970. It is likely, for example, that the negative correlation for young people between LFPR and the age group fraction of total population is not a measure of job competition at all, but an artifact of the large student population of Massachusetts. NEPOOL's interpretation is contradicted by the positive relationship between population fraction and LFPR for men over 24 and women over 21, and by the obvious fact that most job competition is determined by skill type, education, and sex, rather than age. Additionally, the reported signs on the jobs-per-capita variable are wrong in five cases, and the significance levels of many of the coefficients are quite low. (NEPOOL supplies some goodness-of-fit numbers but does not say what statistic they represent.) most

Q: How is total employment determined?

A: The employment submodule separately estimates household-serving, business-serving, and export employment. All industrial employment is assumed to be for export. Commercial employment is allocated to export where the state employment fraction in a category is greater than the national fraction. Export employment forecasts are based on exogeneous national growth rates, while business-serving employment is a fraction of total statewide employment in the previous year, and household-serving employment is a trended fraction of state population.

Q: Are the export portions of commercial activity determined in a reasonable manner?

A: No. NEPOOL's assumption that any activity with a share of state employment above the national average must export the product of the surplus employment is simply not valid. For example, more than the average amount of employment in Massachusetts is in Services, for example; in part, this is probably due to the greater local consumption of health and social services. On the other hand, Massachusetts has an agricultural export industry (such as cranberries), despite the fact that total agricultural employment is low. These examples illustrate the need for more detailed study of the export components of employment.

Q: How is household-serving employment distinguished from business-serving employment?

A: NEPOOL presents percentage breakdown for 20 commercial categories, but does not explain how these figures are used. Therefore, it is not clear whether NEPOOL repeated NU's error with the MIX variables (see Section II, <u>supra</u>). It is also not clear how employment in the eight commercial categories that NEPOOL generally uses are broken down to these 20 detailed categories, nor why six of the 20 categories happen to show 57%/43% splits between households and businesses.

Like NU, NEPOOL sets business-serving employment as a fraction of total employment in the previous year, with the implausible result that employment can increase in each year even if nothing else changes. Also, export recreational activity is forecast at the 1970-73 growth rates, without correction for economic growth rates or gasoline prices.

Q: How good is the export employment forecast?

A: This section of the NEPOOL model is essentially identical to the corresponding portion of the NU model. In Section II, we discuss the problem with cost measurement and the cost index multiplier (corresponding to NU's growth multiplier). In addition, NEPOOL's results indicate mediocre performance on statistical tests and backcasting.

Q: Are appliance saturations projected in a reasonable manner in the residential power submodule?

A: Most appliance saturations are forecast as functions of household income; this is generally a good approach, although family size probably should be included for several appliances. However, no distinction is drawn between new market penetration and old market conversions or acquisitions; this may be a serious deficiency for central air conditioning and electric ranges.

Prices of electricity and alternative fuels are not incorporated in any way; increasing electric costs may counteract the effects of falling real price of appliances which NEPOOL incorporates. In any case, electric penetration of the range and dryer markets will primarily respond to relative fuel prices and efficiencies, to space heating fuel, and, for ranges, to performance; if falling appliance price has any effect, it would be to reduce the slight capital cost advantage some electric versions enjoy over their gas counterparts.

Electric space heating penetrations are forecast by use of an equation that incorporates electric and oil heating capital and operating costs, promotion by the utility, fraction of housing that is single family, and degree of urbanization . This equation was estimated on the basis of data from thirty-two utilities around the country; since heat pumps are very popular in some warm areas, NEPOOL's cost comparisons may be seriously tainted. Problems are also evident in the estimate of alternative fuel cost: gas is not considered as an alternative for New England, and new furnace efficiency is assumed to be constant from 1966 on. NEPOOL also gives no hint of how the variables (most importantly, electric and oil prices) are forecast; in the case of electric price, the effect of rate reform or promotional rates should also be considered.

NEPOOL indicates in the Report that water heater penetrations or saturations could not be adequately modelled, and therefore must be supplied by the user. Again, we have no idea what values NEPOOL used in its forecast.

Q: Are NEPOOL's projections of average annual use per appliance reasonable?

A: Curiously, the Report does not provide this information. NEPOOL provides only "connected load" for each appliance, which is multiplied by a fraction, F (which varies over the days of the week, the seasons, the time of day, between appliances, and in some cases with temperature) to determine hourly demand. The annual sum of these F's then determines use per appliance. Even in the absence of this information, however, several shortcomings are evident.

NEPOOL has determined a relationship between family size and the annual use by ranges, refrigerators, dryers and water heaters. But this relationship is only applied to determine 1970 consumption, despite the fact that household size is projected to fall over time. No family-size adjustment is calculated for other appliances.

Electric water heater consumption increases with dishwasher saturation, but does not respond to dishwasher or clothes washer efficiency improvements, which should have a substantial effect on average consumption.

Average use by refrigerators, freezers, dishwashers, and dryers are projected to increase by as much as 2% annually. These figures are based on trends in the 1960's in California; in a time of falling electric prices. They are simply irrelevant to NEPOOL's forecast for the 1980's. In addition, since dishwasher and dryer efficiency targets are formulated on a per-load basis, these trends would imply either that the targets will not be met (that efficiency may actually decline) or that the load profiles (the F fractions) will change, which NEPOOL apparently is not forecasting. Either DOE or NEPOOL itself seems to disagree with each alternative.

Does not

NEPOOL does not make clear whether the DOE efficiency standards are applied so that refrigerators and freezers each comply as a class. NEPOOL recognizes separate frost-free and standard versions of both appliances, and projects a greater saturation of frost-free refrigerators (the forecast split for freezers is not specified). If the efficiency improvements are applied to the two versions separately, NEPOOL would again be predicting that the entire appliance class will not achieve the DOE standards.

NEPOOL's explanation of their price elasticity adjustments is horribly garbled. The Report indicates that base consumption for each appliance is multipled by

$$\left[\frac{Pn}{Po}\right]^{E}$$

where Pn = Price of electricity in period n
Po = Base price of electricity
E = Price elasticity estimate

It then goes on to discuss long and short-run elasticities, which do not appear separately in the equation. Nor does the Report explain how the effects of prices in various preceding years on current consumption are actually evaluated.

Based on "remarks" and "testimony" by NERA personnel (who prepared the NEES forecast), NEPOOL makes a number of peculiar assumptions. They assumed unrealistically high (-.5) short-run price elasticities for several appliances, and very low (again, -0.5) long-run elasticities for other appliances. The latter group includes ranges and water heaters, which have gas alternatives and should therefore

numbers different

have a -1.75 long-run elasticity, according to NERA. Use by refrigerators, freezers, and televisions is amazingly assumed to exhibit no price elasticity at all. Again, no price forecast is given.

Use in the miscellaneous category is predicted with the formula:

formula slightly different

$$M = (.0183 + .0064t) * Y * (1.043)^t * M_{70}$$

where M = miscellaneous appliance use per household
Y = personal income per household
M70 = miscellaneous use in 1970
t = year-1970

The first factor is NEPOOL's perceived time trend for appliance expenditures as a fraction of income in the period 1960-73, which they extrapolate out indefinitely. The third factor reflects NEPOOL's projection of falling real appliance prices.

The basic problem with this formulation lies in the assumption that electricity consumption is proportional to appliance expenditures. This is a suspect position; many new appliances will replace older, less efficient versions of the same appliance (as in home sound equipment) or will substitute for other appliances (as in many cooking devices) or will be used only quite infrequently (as many shop and kitchen tools). In any case, NEPOOL does not offer any demonstration that the hypothesized relationship exists.

The next problem arises in NEPOOL's assumption that appliance purchases increase as a function of time, rather than as a function of income. Both models may fit well in the historic period (in fact, it is unclear how well NEPOOL's time trend fits the data), and the income explanation has more causal appeal.

Finally, it appears that NEPOOL may have established the time trend using dollars deflated in a normal manner (e.g., by the CPI) and then added a 4.3% growth in appliance sales (due to falling appliance price) which was already captured in the time trend. Again, NEPOOL's failure to document the model precludes adequate review.

As a result of its triple trending (time, income, and appliance price) miscellaneous appliance use is shown by NEPOOL to increase over twice as fast as overall residential use from 1970 to 1975 in Massachusetts. It would be interesting to see what fraction of NEPOOL's 1989 residential forecast is generated by this mechanism.

Q: Is the NEPOOL industrial submodule any better than the residential submodule?

A: No. The same problems in documentation exist, compounded by peculiar formulations, internal contradictions, and outright inaccuracies. There does not appear to be a single measure of goodness-of-fit or significance reported in the entire industrial submodule, for example.

Q: Please describe the industrial submodule.

A: NEPOOL first divides the industrial employment (as output of the economic model) into production and non-production employees. To derive KWH sales, the production employment in each SIC in each state is then multiplied by annual man hours per employee, value added per man hour, and KWH per dollar of value added.

Q: Please describe NEPOOL's forecast of production employment?

A: It seems that rather than model the ratio of production to non-production employees directly, NEPOOL chose to forecast the growth rate in value added per employee for each class and then back out the ratio. This is a round-about approach, and NEPOOL really does not explain why it is used. Even NEPOOL became confused by this section of the module: on p. H-2 the Report says that the ratio increases if the production productivity growth rate is less than the non-production productivity growth rate (which is true), while on p. H-4 the Report claims the exact opposite. Furthermore, since the non-production employee productivity projections are based on New England data (from unspecified source and years) and the production employee productivity projections are from state data, the data seems to be incommensurate. Finally, NEPOOL's manipulation of the value-added-per-production-employee trending also affects the validity of the ratio.

Q: Please describe NEPOOL's projection of annual man-hours per employee.

A: Since this factor is held constant in the projections, the questions which arise are whether the national employment forecasts utilized by NEPOOL make the same assumption, and whether the data was appropriately selected. On the latter point, NEPOOL indicates that only "selected observation" were used in establishing the hours per employee ratio; it is not clear whether this selection affected other portions of the calibration process.

Q: Please describe NEPOOL's forecast of value added per man-hour.

A: NEPOOL uses ~~three~~ two models for VAMH. Model 1 is a constant, Model 2 is an exponential growth rate, and Model 3 assumes exponential growth times the growth in value added. NEPOOL provides no documentation for their choice of models for each SIC for each state (plus New England and totals). NEPOOL claims that "the model that fit the historical data best was initially selected". If NEPOOL means that the model with highest R^2 was selected, the statement is incorrect, since this criterion would always select Model 3, yet NEPOOL used Models 1 and 2 as well. If NEPOOL is not referring to an R^2 test, then this is one more undocumented specification process.

why employment was used instead. Other more specific problems arise as well.

NEPOOL estimates a retail trade electric consumption per employee on a data-set of 196 customers in Connecticut and Maine. Only a short-run price elasticity is used; the lagged effects of falling electric price are probably captured in the time trend, which is then extrapolated into the forecast. Therefore, the retail trade sales forecast contains an implicit forecast of falling electric price. Finally, no significance measures are reported.

The other categories of commercial sales are based in some way on the retail study. The documentation of this extrapolation is very vague.

Commercial price elasticity is modelled in the same manner as other classes; again, documentation is very incomplete and vague. NEPOOL uses a rather high short-run
-.2 elasticity of -0.3, which is based on 1974 and 1975 data, contaminated by the effects of the recession, and is also based on trending of 1970-73 use with an unspecified model which omits price effects. The long-run elasticity of -1.0 is somewhat low, as NEPOOL admits. NEPOOL claims that this is appropriate, "since the selection of electricity for heating and cooling is treated separately through the saturation functions." But the heating saturation functions are based on upward time trends from the period

1966-1975, which captures the effects falling prices, and the air conditioning "trends" are not documented at all. (Furthermore, the saturation rates are not corrected for commercial construction rates, which are probably important determinants.

The fact that the commercial model tracks historical growth from 1970 to 1973 without price adjustment indicates that there is an increasing upward bias over time.

Q: Are there also problems in the miscellaneous use over submodule?

A: Yes. For example, in the street lighting sector, KWH per unit of population is trended at the 1960-1974 growth rate for most states, despite recent declines in usage growth and in some cases, total usage. No goodness-of-fit measure is reported for the Massachusetts function.

In the agriculture sector, KWH per farm employee is trended on 1966 to 1974 data, which captures a falling trend in electric price; a very low long-run elasticity of -.3 and a relatively high short-run elasticity of -.2 are applied in the forecast. The same elasticities are used in the mining and quarrying forecast.

Railroad sales, utility company use, and sales for resale are user-specified and therefore not explained in the Report. NEPOOL warns that company use and some railroad use is already included in the commerical forecast; there is no indication of how this double counting would be presented.

Q: Does this conclude your testimony on the NEPOOL model?

A: Yes.

XII SUMMARY

Q: What percentage of projected NEPOOL demand growth is attributable to the forecasts you have reviewed?

A: The ten utility forecasts we have reviewed (including BECO) constitute about 92% of the demand growth in the 1978 Load and Capacity Report. Table S-I breaks this down by company.

Q: Of the ten forecasts, are any reasonable enough to be used in planning capacity additions?

A: No.

Q: Are there any consistent problems across the various forecasts?

A: None of the forecasts properly handle price elasticity, either short-run or long-run for any class. Except for NU, every forecast has several serious errors in the residential forecast, most commonly involving consumption per appliance and often saturations and customer number as well. (NU's problems in this class, while not numerous, are not minor either.) Most of the forecasts either ignore even the modest DOE efficiency standards or completely misapply them.

Commercial and industrial forecasts generally suffer from some combination of oversimplification and subjectivity. Of course, there are many special cases, such as NU's fantastic negative electric use per employee and PSNH's selective projection of KW demand and load factor, in which other sorts of errors are committed.

The derivation of peak load is generally quite crude, neglecting all forms of load management and time-of-use pricing, as well as appliance mix and shifts in sales between classes. Again, there are some exceptions, such as NU.

Q: Are the forecasts mutually consistent?

A: In several cases they are not. For example, there are some conflicts between NU and UI over the allocation of Connecticut growth. Similarly, EUA projects continued migration out of BECO's service territory into Brockton Edison's territory, while BECO assumes that migration will end. EUA similarly expects BECO appliances and commercial enterprises to move south, while BECO does not. Our inability to secure information on the forecasts of smaller companies in northern New England has precluded any real consistency testing for Vermont, New Hampshire and Maine. And the shoddiness of NEES' modelling and documentation renders similar analysis in Massachusetts, New Hampshire, and Rhode Island quite difficult.

Q: Is there any overall bias in the errors you have identified?

A: Yes. The errors appear to predominantly exaggerate future sales and demands.

why employment was used instead. Other more specific problems arise as well.

NEPOOL estimates a retail trade electric consumption per employee on a data-set of 196 customers in Connecticut and Maine. Only a short-run price elasticity is used; the lagged effects of falling electric price are probably captured in the time trend, which is then extrapolated into the forecast. Therefore, the retail trade sales forecast contains an implicit forecast of falling electric price. Finally, no significance measures are reported.

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Commercial and industrial forecasts generally suffer from some combination of oversimplification and subjectivity. Of course, there are many special cases,

TABLE S-1: COMPARISON OF COMPANY PROJECTED GROWTHS

Company	Winter Peaks From 1978 L+C Report 1977/78	1988/89	Growth Rate %	Fraction of NEPOOL Growth (non-coincident) 1978-1989
Northeast Utilities	3703	5209	3.15%	16.1%
NEES	3018	5444	5.51%	25.9%
Boston Edison	1786	2700	3.83%	9.8%
Public Service of New Hampshire	1156	2503	7.28%	14.4%
Central Maine Power	1147	1969	5.04%	8.8%
United Illuminating	850	1121	2.55%	2.9%
Eastern Utilities Associates	658	1029	4.15%	4.0%
NEGEA	613	990	4.45%	4.0%
MMWEC	672	1171	5.18%	5.3%
Vermont(1)	794	1187	3.72%	4.2%
CVPS	381	507(3)	2.62%	1.3%
Fitchburg	73	114	4.14%	0.4%
Taunton	68	116	5.26%	0.5%
Miscellaneous(2)	500	858	5.03%	3.8%
NEPOOL coincident	15005	24177	4.43%	--
NEPOOL noncoincident	15039	24409	4.50%	100.00%

(1) only CVPS portion reviewed

(2) Newport, Maine Public Service, Bangor Hydro, Connecticut Municipals, not reviewed

(3) Extrapolated from 481 in 1986/87

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Research Experience

12/77-present:	Utility Rate Analyst, Massachusetts Attorney General. Responsibilities include reviewing and commenting on demand forecasts and proposed peak-load rate structures, as well as other electric utility issues involving planning, modelling, and evaluation.
9/76-9/77:	Thesis research on pricing policy as an optimization technique. Considered peak-load and joint-production problems, effects of technological factors on optimal pricing structure, and value judgements underlying the analysis. Published as Technology and Policy Report 77-1.
2/76-1/77:	Liquefied Natural Gas as a public safety problem. Compared analytical techniques for public evaluation and decision-making. Developed paper into proseminar project assignment, guided student groups, critiqued final reports.
9/73-5/74:	Transportation and Community Values Project, MIT. Worked in Cambridge and San Francisco preparing examples of regional transportation plans for California Department of Transportation. Developed data display and comparison techniques.

Education

1968-1970;1973-1974	Massachusetts Institute of Technology. S.B. in Civil Engineering, June 1974.
1972-1973	University of Maryland. Mathematics Education.
1974-1977	Massachusetts Institute of Technology. Civil Engineering and Technology and Policy. S.M. in Technology and Policy, February 1978.

APPENDIX A