COMMONWEALTH OF MASSACHUSETTS
ENERGY FACILITIES SITING COUNCIL

RE: NORTHEAST UTILITIES' DECEMBER 31, 1977 SUPPLEMENT TO ITS LONG-RANGE DEMAND AND ENERGY FORECAST, E.F.S.C. NO. 78-17

TESTIMONY OF PAUL CHERNICK ON BEHALF OF THE ATTORNEY GENERAL OF MASSACHUSETTS

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

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

Q: Please describe briefly your professional education and experience.

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 associate membership in the research honorary society Sigma Xi. I am the author of <u>Optimal Pricing for Peak Loads and Joint Production: Theory and Applications to Diverse Conditions</u>, Report 77-1, Technology and Policy Program, Massachusetts Institute of Technoloby. 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.

Q: Have you ever testified as an expert witness?

A: Yes I have. I testified before the Energy Facilities Siting Council and the Massachusetts Department of Public Utilities in the joint proceeding docketed by the E.F.S.C. as 78-12 and by the D.P.U. as 19494, Phase I. My testimony covered appliance penetration and saturation, elasticity, effects of price on peak loads, and a variety of modelling issues.

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

A: I read 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 (January 1, 1978); and NU's response to 57 Information requests by the Attorney General.

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 any of the electric utility forecasts I have seen, which includes all Massachusetts companies, among others. 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 models?

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

1. the specification of the migration 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 a 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.

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

A: Three aspects of equation 16 seem rather problematic: the derivation and application of the MIX variable, the presentation and interpretation of the estimated relations, and the time-trending of employment.

The MIX weights appear to be the result of NU's judgemental adjustments of Battele'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 services (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.

The regression coefficients are not presented in EED; in the future, they certainly ought to be. When the regression results were presented in response to question AG-5, NU's failure in this response to define the variables either explicitly or implicitly (by providing the input data) renders the results quite unintelligible. Furthermore, it cannot be determined whether the population and employment inputs were properly defined to be of equivalent magnitude so that the desired portion of the MIX variable are 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.

The time-trending of the employment ratios have the problems common to all such trending. 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; 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., if even nothing else changed in the economy non-manufacturing employment would increase every year.

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 $[\alpha_e + \beta_e \text{ (ln time)}]$ 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 given 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 SIC20 and SIC28, nor how the "calibration over the historic period" (question AG-7) was compared between models with and without the cost index. These decisions may have been made on a reasonable basis, but it is impossible to evaluate those judgments without either second-guessing NU or obtaining more information than they supplied in response to a straight-forward request to "explain why time and growth multipliers are used for some industries in equation 19, but not for others" (AG-7). 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 you have on the growth multiplier?

A: First, equations 20c and 21 combine to imply the relationships listed in Table I infra. For example, if national growth is negative and costs are much lower locally, then the faster national employment falls, the fåster local employment grows. 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 infra 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

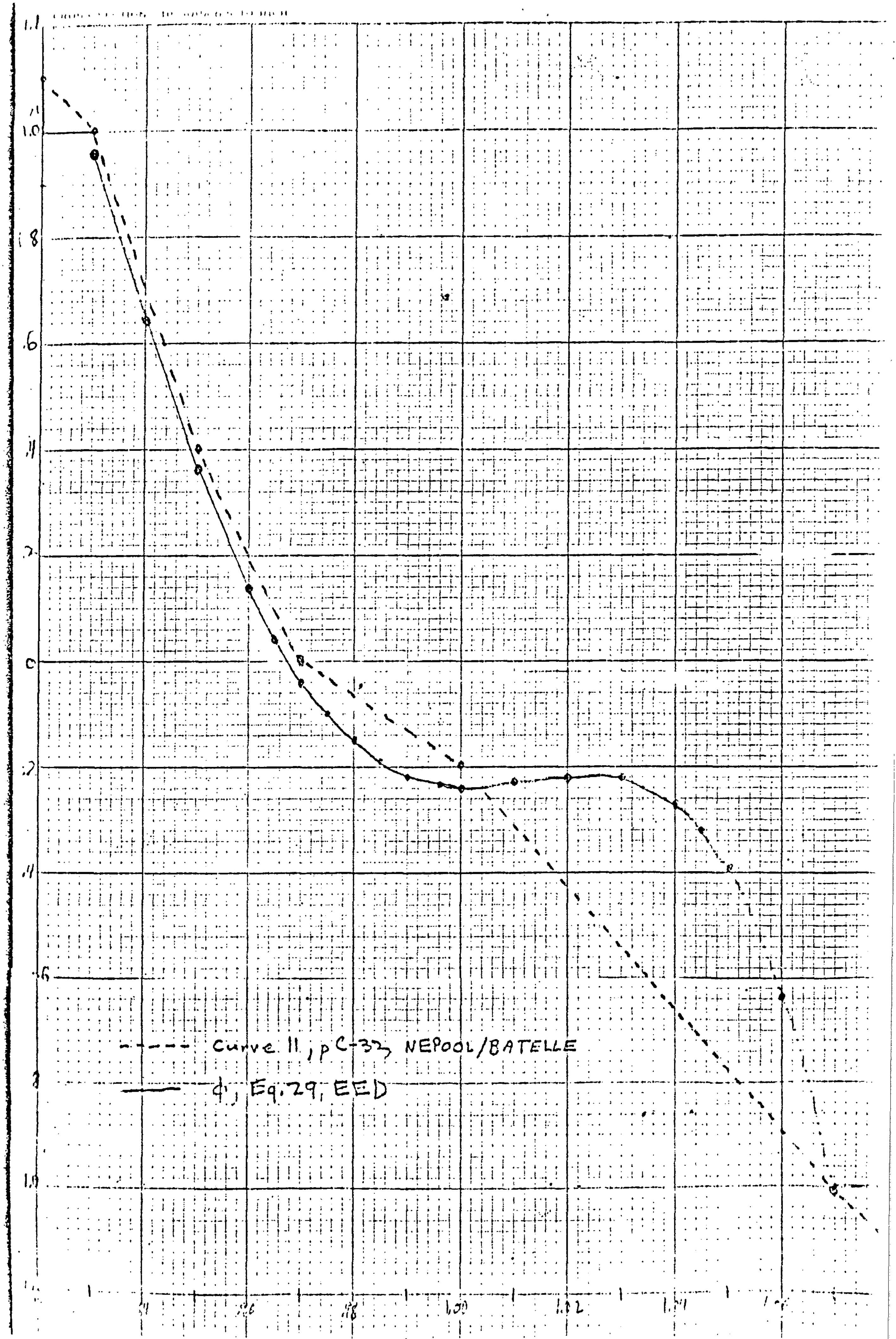

1.1
1.0
.8
.6
.4
.2
0
.2
.4
.6
.8
1.0
.86
.88
1.00
1.12
Curve 11, p C-32, NEPOOL/BATELLE
φ, Eq. 29, EED

Table I

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 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.

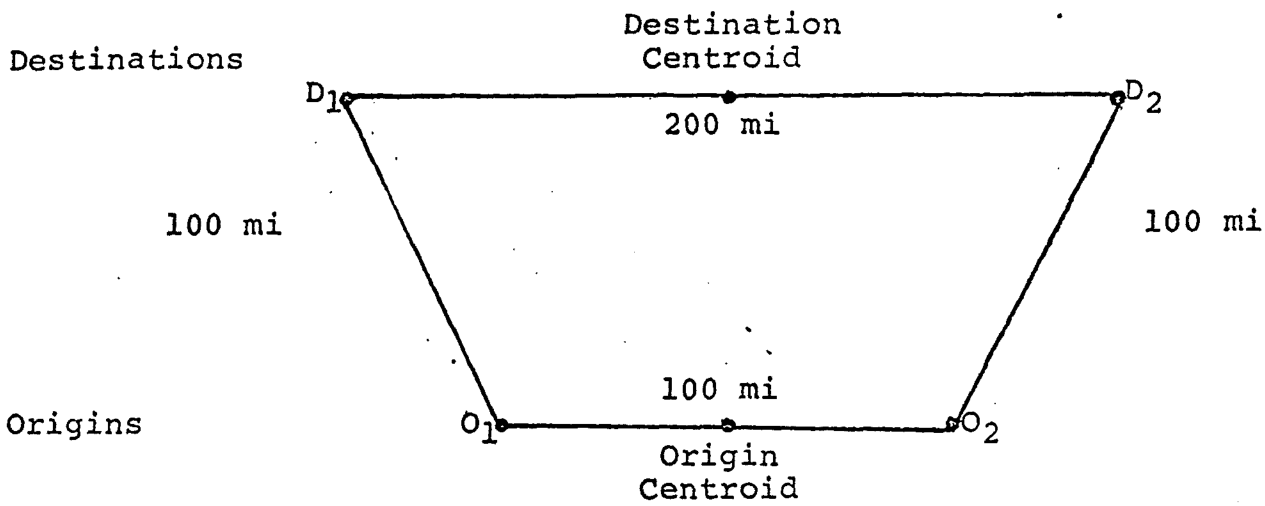
With respect to transportation costs, the major problems concern measurement of distances. While the measurements of distance from New England to other regions are somewhat crude, the real problem arises within New England. NU assumes that all shipments from any part of their service territory originate 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 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.

Q: What about energy costs?

A: NU uses the 1971 ratio of Connecticut electric prices to national electric prices. This is the only year in which the



SUPPOSE:

Shipments originate equally from 0, and 0_2
Shipments from each origin are equally divided between D, 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 = $\sqrt{\frac{3}{2}} \times 100\text{mi} = 86.6\text{mi}$.

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

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. Three aspects concern me: the documentation of current appliance consumption figures, the assumed effective date of DOE appliance efficiency standards, and the calculation of refrigerator efficiency standards. These concerns can be briefly stated.

NU says (question AG-26) that consumption data is derived from the FEA Conservation Program for Appliances in the Federal Register for July 15, 1977. For most appliances, I cannot find annual Kwh figures in that source, and I think it appropriate for NU to explain the origin of the numbers they used.

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 repsponsible 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 would seem to require either revision or more specific and substantial support.

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 refrigerators, 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 1971-76,

3. vacancy rates for commercial property are generally negligible,

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

5. 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 disaggregate analysis of the commercial sector, NU's approach may be reasonable, but it should be compared to other simple techniques. For example, simple econometric models which relate commercial electric use to population, personal income, electricity price, and lag effects, may be both as accurate as the present model and easier to explain, review and update.

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 p. 148 of EED, NU states that demolitions are 1% of the remaining stock of buildings constructed prior to 1960. 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. (Incidently, 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 volitile 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. 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", the concept of negative new floor space is clearly problematic.

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. 15, supra) then the basic calculation laid out in the respons to question AG-37 (with some clarification from p. 148 of EED) is appropriat

$$\begin{pmatrix}\text{incremental}\\ \text{use in existing}\\ \text{buildings}\end{pmatrix} = \left[\begin{pmatrix}\text{previous}\\ \text{years}\\ \text{use}\end{pmatrix} - \begin{pmatrix}\text{use in}\\ \text{demolished}\\ \text{buildings}\end{pmatrix}\right] \cdot \left[\frac{\text{current PEU}}{\text{previous year PEU}} - 1\right]$$

Note that whenever PEU falls, incremental use by existing buildings must be negative. Furthermore, this formula eliminates any growth in the use in demolished buildings but does not net out the previous year's use by those buildings.

I have attempted to reproduce the calculation of column 6 of Table 63. My results are given in Table 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 state service area has a negative value for column 6 for 1975, when PEU was declining. Also, subtracting out the MWH's attributed to the demolitions produces incremental values close to NU's for 1972, 1974 and 1976, but not for 1971 (my result is negative), 1973, or 1975.

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

year	Sq. Ft. (a)	Kwh/Sq. ft. (b)	Mwh (c)	Surviving Kwh (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:

(2) from column (3), Table 63
(3) (14.04 ÷ 53.8) x (PEU/sq. ft., t-1) from column 10, Table 63
(4) (column A) x (column b)
(5) (column 5, t-1, Table 63) - (column c)
(6) $PEU_t \div PEU_{t-1}$, column 10, Table 63
(7) (column d) x (column e)

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

Table III

ear	growth in total sales(1)	increment in existing buildings(2)	decrement due to demolitions(3)	increment in new buildings	new building electric use(5) Kwh/sq. ft.	Electric penetration(6) percentage
	(a)	(b)	(c)	(d)	(e)	(f)
'71	71830	8563	11330	74597	59.92	110
'72	103998	33448	11136	81686	50.96	89
'73	53383	5899	3763	51247	45.39	79
'74	-53133	-93442	3760	44069	22.07	43
'75	54177	-29459	3327	86963	-44.92	-91
'76	37519	10833	3184	29870	33.67	67

otes:

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

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 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 prices.

Q: What problems arise in NU's estimation 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, 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), 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:

> The standard $\bar{R}^2$, 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 (and similarly, price) from the specification should be especially well justified.

A: 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. While this formulation omits the cost of capital from decisions regarding production methods, this may not be a major problem.

A difficulty does arise in that only the current 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 charge. 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 (1976?) levels. Since NU is forecasting dramatic decreases in wage rates (at least relative to national levels), it would be appropriate to at least investigate the model's sensitivity to alternative price forecasts.

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

A: I do not understand it. As presented on p. 174 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. The 1987 "other" sales of 1531.9 (Table 70, EED) would have to be the result of a 51% increase in projected Connecticut industrial output from 1986 to 1987. Some error must have occurred in the production of EED, but the answer to AG-45 does not refer to any such error.

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. 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: Do you have any other comments on NU's forecast methodology?

A: Yes. Given the important effects of electricity prices (including past price increases) on demand, it would seem appropriate for NU to include elasticity explicitly in the forecast. If they feel that various adaptations and modifications to their methodology (for example, retrofitting of energy conservation equipment) have substantially captured price response, they should still compare the magnitude of those impacts to the effects predicted by conventional elasticity estimates. Understanding the impact of prices is vital to responsible forecasting, planning, and policy-making.