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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of BOSTON EDISON COMPANY, et al., Pilgrim Nuclear Generating Station, Unit 2	Docket No. 50-471

JOINT TESTIMONY OF PAUL L. CHERNICK AND SUSAN C. GELLER ON BEHALF OF THE COMMONWEALTH OF MASSACHUSETTS

COMMONWEALTH OF MASSACHUSETTS

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TABLE OF CONTENTS

INTRODUCTION AND STATEMENT OF QUALIFICATIONS

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 at 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 Peta Pi, and to associate 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 regresion and other topics in modeling. My resume is attached to the end of this testimony as Appendix A.

Q: Have you testified previously as an expert witness?

A: Yes. I have testified jointly with Susan C. Geller before the Massachusetts Energy Facilities Siting Council and the Massachusetts Department of Public Utilities in the joint proceeding concerning 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 have also testified jointly with Susan C. Geller in Phase II of D.P.U. 19494, concerning the forecasts of nine New England utilities' forecasts and NEPOOL's forecasts, and jointly with Susan Finger in Phase II of D.P.U. 19494, concerning Boston Edison's relationship to NEPOOL. 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:

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, and in Phase II of D.P.U. 19494, as described above. I have also filed expert testimony in two cases before the Massachusetts Energy Facilities Siting Council, in cases involving long-range forecsts 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: Will you please describe the subject matter of your testimony?

A: Yes. With respect to the so-called "need for power" issue, we have been asked to comment on the following specific areas of the N.R.C. Staff's case and the Applicant's case:

1. The Oak Ridge model;
2. The NEPOOL model;
3. The "displacement" of oil generation by nuclear generation argument;
4. Several miscellaneous points:
   a. discount rates;
   b. capital cost estimates;
   c. O+M expense estimates;
   d. NEPOOL need vs. BECO need.

I. COMMENTS ON THE OAK RIDGE NATIONAL LABORATORY MODEL

Q: What criticisms do you have of the ORNL electricity demand model?

A: There are problems with:

1. the method of endogenizing average price;
2. the criteria for model specification;
3. the choice of independent variables; and
4. the projections of the total average electricity cost.

Q: What problems do you see with the endogenizing of the average price variable?

A: The purpose of including a price equation in the model is to "reflect the interaction of demand and supply occurring within a declining block rate structure." (ORNL Report, p. 1-3) The price equation does not correctly model the impact of average consumption per customer on average price per KWH.

First, there are problems of aggregation. For the individual customer facing a declining block rate, average price per KWH declines with demand. However, as the authors of the ORNL report note, when aggregated over customers and rate schedules, the relationship between average consumption per customer and average price per KWH can be positive or negative. What the authors of the ORNL report neglect to point out is that at that level of aggregation there may not exist a constant relationship in the aggregate. Therefore, without further disaggregation within customer classes it may simply not be possible to model the effect of multi-step pricing.

Second, the price equation incorrectly models the relationship between average consumption and average price under multi-step pricing if the relationship exists. According to the ORNL model, changes in average consumption affect only the "profit" portion of average price. Profits are defined as the average price per KWH for a given customer class net of total cost averaged over all sectors. The total cost variable is some sum of the various cost components: costs of fuels, operation, maintenance, taxes, interest, capital, and depreciation of generation, transmission, and distribution plant.

The price equation contains the following errors:

1. it omits the effect on average cost of average consumption, total consumption, customers per square mile, and consumption mix by class;

2. the price equation contains no independent variables that would reflect changes in rate schedules, a curious omission given that the purpose of including a price equation is to adjust for the effects of multi-step pricing; and

3. the price equation omits causal variables that determine changes in profit margin over time. One would expect profits, as set by the regulatory process, to vary with the capital intensity of production, with general economic conditions, and with market interest and inflation rates.

Third, the forecasts resulting from the price-endogenized model are problematic. The model projects declining profit margins for all the states in the New England region. For most of the 6 states, the decline is so drastic as to be implausible. Perhaps the regression captured the following phenomenon. During the 1960's, when total average costs were falling, regulatory lag probably resulted in high profit margins. In the 1970's, with rising costs, regulatory lag may have worked against the utility companies; in any case, profits fell. In this case, the regression would show profits falling with rising average consumption per customer, but we would not expect the trend of declining profit margins to continue indefinitely into the future.

The ORNL report provides a mathematical derivation of the price equation. These mathematical formulations and manipulations conceal two important and weak assumptions. The peculiar formulation of the price equation follows directly from these assumptions.

Q: Would you point out these assumptions?

A: First of all, profits are assumed to be independent of costs. This assumption seems incorrect, and also seems to have no relation to the actual regulatory process. Under rate regulation, profits are constrained to a level which provies a fair rate of return on the utility companies' investment in plant and equipment. The fair rate of return is set with some consideration to the financial condition of the utility company (i.e., the capital structure is often used to calculate overall cost of capital) and more general economic factors (i.e., general economic conditions may affect the allowed return on equity).

The result that K appears additively in the relation $P_i = f_i(Q_i, k)$ is equivalent to this ORNL assumption of the independence of production costs and profits. The mathematical derivation of this additive relation is largely window dressing. It consists essentially of differentiating and then integrating an equation. Since the second operation is the exact inverse of the first, the net result should be the original equation (plus a constant). The reason for the new result is that the independence of costs and profits was implicitly assumed in the differentiation:

$$\frac{dF}{dk} = \frac{dk}{dk} + \frac{d\Pi}{dk} = 1$$

A second assumption made is that "price depends more basically on quantity and average costs" than it does on quantity and rates. As a result, the ORNL model treats declining block rate structures as though a single variable, total average cost, alone determines charges in rate structure or as though the block rate structure within a given state remains constant over time. In addition, it is assumed that the average cost variable is exogeneous.

Q: What objections do you have to the criteria for model specification?

A: The ORNL report provides inadequate documentation by which to evaluate the criteria used. The report does mention that other models were tested, but we do not know the models nor the reasons for rejecting them. According to the report, "it was soon found that forecasts were quite sensitive to equation specification" (p. 3-12). Therefore, the criteria for model specification merits particular attention.

One example of specification criteria that the ORNL report provides gives cause for concern. The report discusses the treatment of alternative fuel price variables. According to this discussion, the ORNL modellers excluded the cross-price variables when the coefficient had the incorrect sign. They did so on the grounds that "when inclusion of a variable leads to an implausible sign. . . this may be indicative of a misspecification" (p. 5-2). The possibility of misspecification is not limited to the single variable with the wrong sign. According to Rao and Miller (1971):

> Often when regression coefficients are estimated the sign is opposite to that which the researcher believes to be true. When this happens, many researchers unfortunately drop the guilty variable from the regression equation with no further mention. In many cases, however, this is not an acceptable procedure, for a wrong sign may be a warning, inter alia, of incorrect definitions, specifications, or interpretations.1/

The mere exclusion of the variable is particularly inappropriate when there are a priori reasons for thinking the variable does belong in the equation. Unfortunately, the ORNL modellers make additional argument based on factually incorrect assumptions. They argue incorrectly that the natural gas price should be insignificant in the New England residential demand equation because

> . . .natural gas has never been an important fuel used by residential customers in this region even though data on the price of residential gas are available. (p. 5-3)

Actually a high proportion of total natural gas sales in New England are to residential customers. The ORNL authors also argue that

> . . . natural gas and fuel oil were used primarily for space heating, and electricity was used predominantly for other end uses.

1/Potluri Rao and Roger LeRoy Miller, Applied Econometrics (Belmont, California: Wadsworth Publishing Co., Inc., 1971) p. 44.

This statement overlooks the fact that natural gas and fuel oil competes with electricity in end uses other than space heating. In New England a high proportion of water heating uses gas or oil, and a significant proportion of ranges and dryers are gas-fueled.

Q: What problems do you see with the choice of independent variables?

A: Four of the variables present problems of estimation and interpretation:

1. The natural gas customer variable is an inappropriate measure of gas availability. Since the number of natural gas customers is a function of the relative prices of gas and electricity, this variable may capture some of the electricity price effects.

2. The dummy variable for "investigating a structural shift between periods of falling real electricity prices and rising real electricity prices" also captures some of the price effects, and may result in an underestimate of price elasticity.

3. The customer classification dummy variable is an inadequate solution to a data problem: the reclassification of customer classes that places industrial and large commercial customers in one customer class. This dummy variable reflects only sudden reclassification events, when existing customers are shifted from one customer class to another. There is no variable to correct for the effects of new non-industrial customers as they come on line on the industrial rates. The "value added in manufacturing variable" understates the amount of activity in

the industrial class. The bias may be picked up by other variables in the regression equation. As a result, the value-added coefficient could be overestimated, the price coefficient underestimated, or a combination of the two.

4. The use of state dummy variables is a crude substitute for identifying the underlying causal variables that explain differences in consumption patterns among states. The use of a dummy variable restricts the coefficients of the independent variables to being constant across states within a region. Considering that the regression estimates reveal substantial variation among regions, it is unreasonable not to expect consumer behavior to vary among states within a given region.

Q: What problems do you see with the projections of the total average cost of electricity?

A: In the projection of total average cost, the increase in nuclear construction and uranium fuel costs is omitted. The modellers ignore the possibility that as demand rises, the utility companies will draw power from plants higher in the loading order, thus raising the average fuel cost per KWH.

Q: Finally, do you have any comment on the price elasticities used in the industrial model by ORNL?

A: Yes. The industrial model uses price elasticities (see p. 5-11of the Report) that are so low, and that are so variable between regions, as to call into question the validity of the entire industrial forecast.

Q: Beyond the econometric problems mentioned above, does the Staff analysis contain other serious errors?

A: Yes; the Staff analysis ignores mandated conservation and improperly converts the ORNL energy sales forecast to a demand forecast.

Q: How substantial an error is the Staff's omission of mandated conservation measures?

A: Very substantial. Even conservative estimation of the impact of DOE residential appliance efficiency standards and of ASHRAE 90-75 building code standards in commercial cases would result in an 8% decrease in the Staff's 1990 base sales forecast. Utilizing their naive methodology, this converts to a decrease of around 2000 MW in 1991-92 peak, leaving NEPOOL with a 19.4% reserve in 1991-92 (23.5% in 1990-91) without Pilgrim II or NEPCO 1 or 2, even accepting the Staff's basic sales forecast methodology for determining peak, and capability forecast (no MASCO, no other cogeneration, no new hydro past 1983,no wood plant, no fuel cell, no refuse).

Q: How does the Staff analysis improperly convert the ORNL energy forecast to a demand forecast?

A: The ORNL forecast predicts rapid increases in the industrial share of sales and corresponding decreases in the residential share.

Since the class load factor for industrial use is greater than that for residential sales, the projected shifts in consumption should increase the system load factor. For example, applying the class load factors implicit in NEPOOL's forecast for the 1989/90 peak to the ORNL base sales forecast by class yields a 1990/91 winter peak of 20828 MW, and a 25.4% reserve margin without Pilgrim II, the NEPCO units, or any other capacity beyond the staff forecast.

II. THE NEPOOL MODEL

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

A: Until recently, we had available only the Report on a Model for Long-Range Forecasting of Electric Energy and Demand to the New England Power Pool by NEPOOL Load Forecasting Task Force and Battelle-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 an ongoing investigation into Boston Edison's construction program (DPU 19494/Phase II) had been unsuccessful.

In the latter case, we recently received, through cross-examination of Mr. Bourcier, copies of partial output from the runs of the model which produced the NEPOOL forecast, forty five "Model Documentations" which revise and supplement the Report, and other information which Mr. Bourcier supplied orally. As of the time this testimony was written, no response to our discovery on BECO in this case had been received.

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

A: Yes. Both the Report and the Documentation raise almost as many questions as they answer, due to the nature and style of the documents:

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 discussion of the criteria for choice, or goodness-of-fit measures.

3. Some important inputs are user specified, and are therefore not presented in the Report.

4. At this writing, only partial results of the Model are available. Such important intermediate results as sales by end use, appliance penetrations, appliance saturations, labor force participation rates, and value added have not been reported.

5. 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 between local and national unemployment. Rather than estimating these relationships over time for each state, NEPOOL estimates <u>across</u> the New England states for the period 1960 to 1970. What is really being measured, then, is the attractiveness of Massachusetts, or Vermont, relative to the rest of the country in the 1960's, rather than the effects of changing unemployment rates. This "cross-sectional fallacy" can be quite dangerous; Figure I illustrates how even the sign of the cross-sectional relationship can be different from that of the relationship which holds for each state. Furthermore, due to the nature of the estimation procedure, neither national unemployment nor time-dependent changes can directly effect the migration rate.

Other problems appear in the migration section. NEPOOL admits that wages influence migration, but wages do not appear as a variable in forecasting migration. Similarly, NEPOOL recognizes that schooling influences migration, yet no attempt was made to identify the impact of expansion of higher education in Massachusetts in the 1960's, which certainly attracted more out of state students in 1970 than a decade earlier. No significance tests are offered for the equations; it is not clear that the relations are not simply artifacts of chance. The statistical tests which are provided by NEPOOL indicate that much of the variation in the data is not explained by the equations. Finally, NEPOOL corrects the equation for young males to take out the effects of the military draft in 1970; it does not appear that the countervailing effect of either the Cold War military activity of 1960 or the function of colleges for draft avoidance in 1970 was similarly factored out.

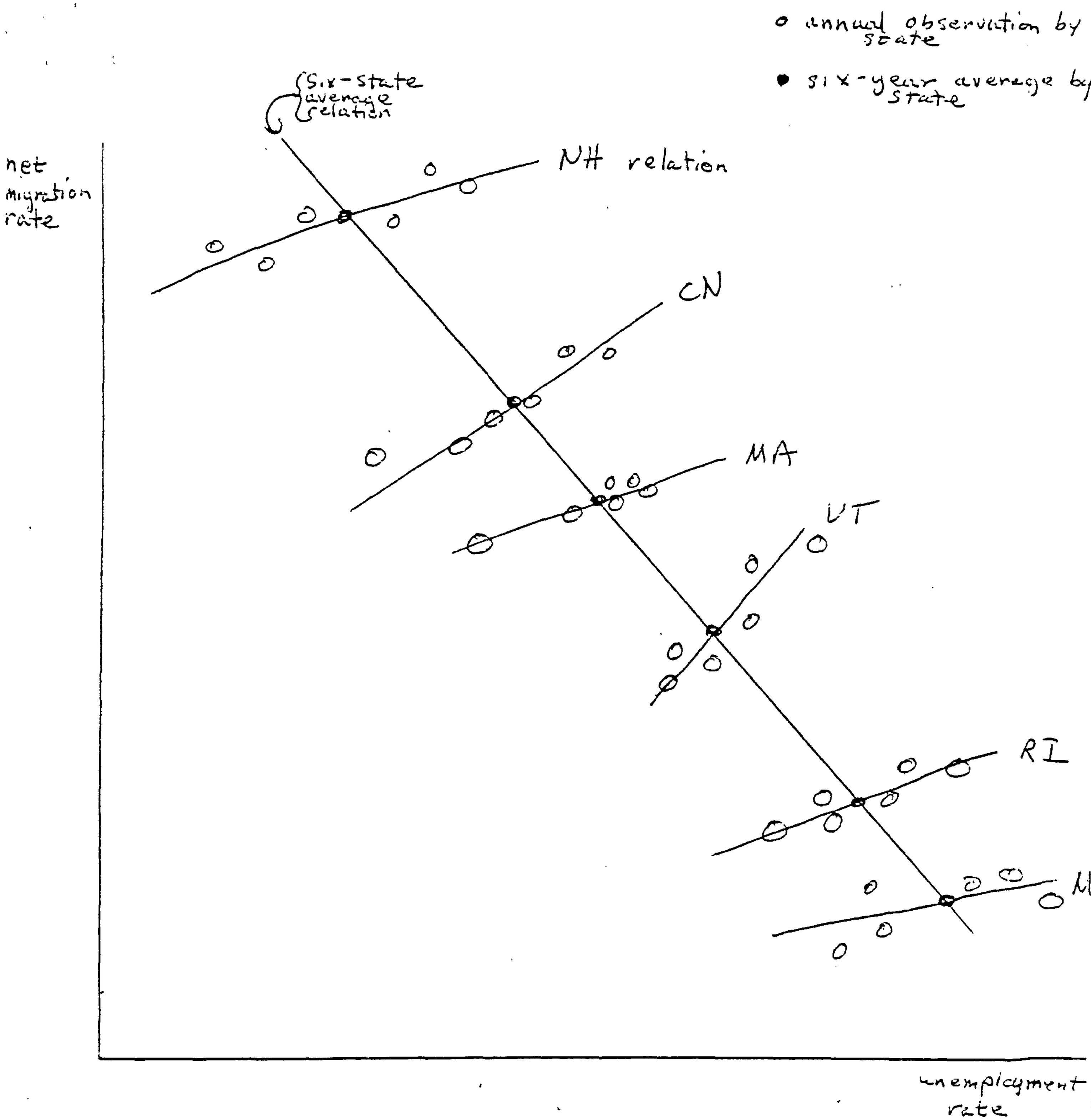


Figure I: Example of the Cross-sectional Fallacy

The sensitivity analyses performed on the migration equations are ambiguously explained in the Report. It is unclear whether the slope coefficients were changed in absolute value or actual level; whether the intercepts, the means, or some other point was held constant when the slopes were increased; and what NEPOOL actually did when it "dropped the error term". In any case, the equations have been revised but no new sensitivity tests were reported.

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

A: Yes. This rate (LFPR) is estimated for each age/sex group as a linear function of jobs per capita and/or of time. Even though data from the years 1960 and 1970 are used, the presence of the time variable probably results in the jobs per capita variable capturing primarily differences between states, just as the migration equations do. For various cohorts, one or both variables are omitted; no reasons are offered for these differences. Finally, having gone to the trouble of estimating some approximation of New England labor forces participation functions, NEPOOL tacks on two time trends based on national projections. It seems that the application of these trends either double counts the effects NEPOOL has attempted to measure directly or eliminates the need for the direct estimation process. In short, it is impossible to determine from the documentation how NEPOOL's LFPR equations were really derived and whether that derivation is reasonable.

Q: How is employment forecasted by NEPOOL?

A: Non-manufacturing employment is forecast as a ratio to state population. Manufacturing employment is forecasted by multiplying exogenous forecasts of national employment growth rates (by SIC) by a "cost index multiplier" to account for differences in local and national costs.

Q: Is the non-manufacturing employment growth forecast reasonable?

A: No. It has two serious problems. First, NEPOOL assumes that all non-manufacturing employment serves local population; in fact, much non-manufacturing employment may be serving businesses and/or serving customers outside the state (e.g., Massachusetts' hospitals and universities, Connecticut's insurance firms, and considerable portions of various states' agriculture and tourism). Second, NEPOOL is apparently projecting non-manufacturing employment per capita in each sector in each state to grow at national rates, despite historic tendencies, in several cases, to grow more slowly and fall more rapidly than the national average. Unfortunately, NEPOOL's documentation on this point is so vague that it is not possible to determine exactly how this projection is performed.

Q: What comments do you have on the cost index multiplier for manufacturing employment?

A: First, NEPOOL's equations 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 faster local employment grows. This relationship is definitely counter-intuitive.

In addition, NEPOOL provides no documentation for the three complex cost index multiplier curves which it uses for various states. The multipliers often produce worse backcasts than the national growth rates alone.

Q: Are the cost comparisons on which the cost index multipliers operate performed in a reasonable manner?

A: Each SIC's costs are divided into fractions for labor, transportation, taxes, energy and others. For each fraction, a local-to-national cost ratio is derived. Problems arise in all five areas.

With respect to labor costs (RLC), the major problems arise with respect to an equation which adjusts RLC as a function of local

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

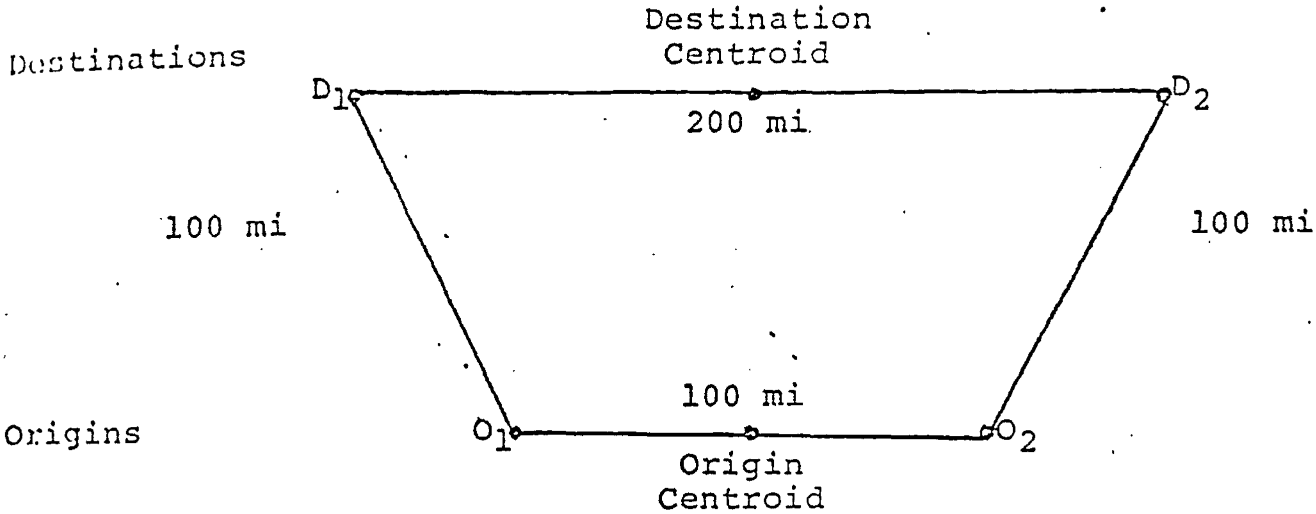
and national unemployment rates. There is no documentation of this equation, and NEPOOL has apparently never tested it. Yet this equation will adjust labor costs downward in the forecast period. Furthermore, NEPOOL adjusts RLC more rapidly when RLC < 1 (local costs are cheaper than national costs) than when RLC > 1. NEPOOL's reasoning on this matter is opaque.

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. NEPOOL assumes that all shipments from any part of a state originate at the state 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, <u>infra</u>.

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 II



SUPPOSE:

Shipments originate equally from O_1 and O_2
Shipments from each origin are equally divided between D_1 and D_2

THEN:

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

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.

Q: What about energy costs?

A: NEPOOL uses the 1971 ratio of local electric prices to national electric prices. This was an unusually good year for New England electric prices. It would appear to be more appropriate to use at least the weighted average of 1970 to 1975, which will be somewhat higher, or to use more recent data and trends. In addition, both electric and other energy costs may rise faster in New England, due to oil prices. No change in the ratio is forecast.

Q: If NEPOOL 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 forecast and that a reasonable growth modifier function could be formulated, the exercise is pretty pointless if most costs evade both measurement and projection. Furthermore, NEPOOL'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: Are there any further problems in the economic submodule?

A: There is one potentially quite serious generic problem. NEPOOL does not seem to have maintained consistency of the internal forecast with the exogenous forecast which drives it. It is not clear that projections of LFPR, or man-hours per employee, or productivity, or wage rates, or energy costs in the NEPOOL model are compatible with the values Wharton Economic Forecasting Associates uses. For example, suppose that WEFA is projecting that low rates of labor productivity growth, shorter weeks, low wages, and high energy costs will generate large employment. If NEPOOL then takes that large employment growth and assumes higher wages, cheaper energy, longer weeks, and higher productivity, the demand forecast will be directly inflated by the lack of consistency.

In fact, in some cases NEPOOL's forecasting may be internally inconsistent, as well. For the manufacturing employment forecast, wage rates are projected to fall compared to national levels, while for determining personal income (and residential electric use) they are projected to rise at historic national rates.

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, the saturation functions suffer from several errors:

1. 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.

2. An income relation is improperly used as though it were an appliance price relation.

3. The effects of electric price and service costs on effective appliance price are neglected.

4. NEPOOL assumes that real appliance prices will fall rapidly although the most recent data available indicates that real prices are rising.

5. Prices of electricity and alternative fuels are not incorporated in any way; increasing electric costs may counteract the effects of the falling real price of appliances which NEPOOL incorporates.

6. The saturation functions are applied to appliances for which the measured price and/or income are not particularly relevant to purchase decisions.

For example, 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. Income should not affect fuel choice, and 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. Furthermore, NEPOOL assumes, without any supporting data or analysis, and often in contradiction to available evidence, very high penetrations of dishwashers and room air conditioners in new construction; increases in total refrigerators saturation; accelerated increases in the ratio of frost-free to standard refrigerators; and constant shares of controlled waterheating.

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. Unfortunately, NEPOOL's model incorrectly measures fuel costs (both in the estimation of the model and in forecasting) and some capital costs, inadequately models the advantage of gas heat over oil heat, explains very little of the observed variation in data, ignores demolitions (which inflate penetration rates) and is improperly adjusted by state. For example, the 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 even 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 price) are forecast; in the case of electric price, the effect of rate reform and elimination of promotional rates should also be considered.

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

A: Curiously, the Report and Documentations do 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, nor does family size affect the distribution of housing types, which is held constant. This error inflates space conditioning use.

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. Apparently, NEPOOL does not understand the sources of anticipated efficiency improvement.

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 may imply that the targets will not be met and that efficiency may actually decline.

NEPOOL does not apply the DOE efficiency standards 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.

In addition, NEPOOL simply ignores the probable enactment of residential appliance efficiency standards beyond the current DOE targets and the inevitable effects of building code changes on electric use by space conditioning and water heating.

Based on "remarks" and "testimony" by NERA personnel, NEPOOL makes a number of peculiar assumptions. They assumed unrealistically high (up to -1.2) short-run price elasticities for several appliances, and rather low (as low ass -0.5) long-run elasticities for other appliances.

Use by refrigerators, freezers, and televisions is amazingly assumed to exhibit no price elasticity at all. The elasticities were arbitrarily manipulated to yield aggregate residential sales in the calibration period.

Use in the miscellaneous category is predicted with the formula:

$$M = (.067 * t + 1.836) * Y * (.996 +.032\ t) * M70 + C$$

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

The first factor is NEPOOL's perceived time trend for appliance expenditures as a fraction of income in the period 1960-1973, which is extrapolated out indefinitely.

The third factor reflects NEPOOL's projection of falling real appliance prices.

One 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). NEPOOL's assumption is incorrect for another reason. NEPOOL is assuming that a doubling of personnel income will result in an immediate doubling in the <u>stock</u>, not just the purchase rate, of appliances. This is equivalent to assuming that the lifetime of appliances is only one year.

In any case, NEPOOL does not offer any demonstration that the hypothesized relationships exist between appliance expenditures, appliance stock, and appliance consumption.

The next problem arises in NEPOOL's assumption that miscellaneous 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. NEPOOL has also 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 an assumed falling appliance price) which was already captured in the trend. Again, NEPOOL's failure to document the model precludes adequate review. In any case, NEPOOL's projections of falling appliance prices are improper.

As a result of its triple trending (time, income, and appliance price) miscellaneous appliance use is expected by NEPOOL to increase over three times as fast as overall residential use from 1976 to 1990, at least for some states (not all the data has been made available).

Q: Are there errors in NEPOOL's handling of the interaction of appliances?

A: Yes, in at least two cases. Mr. Bourcier acknowledged one serious error which understates the reduction in range use due to increasing saturation of efficient microwave ovens. In addition, it does not appear that the model projects the net energy savings due to microwave ovens that the Report indicated were appropriate.

The effects of wood stoves on electric space heating use are incorporated for only two states; even in these states, the effects of wood stoves are held constant after 1979.

Q: How does NEPOOL initialize its 1970 appliance consumption figures?

A: NEPOOL found that 1970 residential consumption was overforecast by the model. NEPOOL therefore adjusted downward the average connected loads for most appliances, by a state-specific factor of 3.4% to 22.1%. Miscellaneous use, air conditioning and heating are excluded from the adjustment on the basis that "they were originally N.E. values." In fact, miscellaneous use is based solely on data from Connecticut, the state for which the adjustment is smallest. Large portions of the errors in other states' backcasts may result from differences in miscellaneous consumption from the 200 Connecticut customers from whom the miscellaneous data was extrapolated.

Window air conditioning usage appears to be based on Ohio and Baltimore data and on 1977 estimates by BECO and Northeast Utilities (Documentation 15). None of these sources used any New England consumption data, although New England cooling degree days are considered. Electric heating consumption is based on 169 all-electric homes (perhaps of identical size and vintage) in Amherst, Massachusetts (Report, p. G-17). Perhaps the 22.1% error for Maine results from an overestimate of average heating consumption in that state based solely on the Amherst sample and weather.

Since it is the unadjusted uses, miscellaneous and space conditioning, which grow fastest in the forecast, NEPOOL's improper exclusion of these uses from the 1970 adjustment increase the overall forecast growth rate.

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 (an 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 roundabout 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 th 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: This factor has been falling since 1970, yet NEPOOL arbitrarily assumes that it started increasing in 1977. In addition, it is not clear whether the national employment forecasts utilized by NEPOOL use the same man-hour assumptions, and whether the data was appropriately selected. On the latter point, NEPOOL indicates that only "selected observations" were used in establishing the hours per employee ratio; it is not clear whether this selection affected other portions of the calibration process. In any case, the sudden increase in man-hours inflates the industrial forecast.

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

A: NEPOOL uses two models for VAMH. Model 1 is a constant and Model 2 is an exponential growth rate. NEPOOL provides no documentation for their choice of model for each SIC for each state (plus New England and totals). In fact, the New England relationships, to which the states are assumed to converge, are not even provided in the documentation.

Q: How does NEPOOL forecast the ratio of KWH sales per dollar of value added?

A: NEPOOL derived their electric intensity trends for some sort of backcast and calibration procedure, involving the estimation of two trend factors. NEPOOL does not provide:

any rationale for the double trending,

any description of the estimation methodology,

any explanation of the level of aggregation (SIC, state, etc.),

any description of the data, such as its source or comprehensiveness,

any data,

any of the estimated trends, or

any indication of goodness-of-fit or of statistical significance of the equations utilized.

Therefore, only NEPOOL knows what was done and whether the method and results make any sense.

The documentation issue is complicated by NEPOOL's claim that special industry studies for seven SIC's, including self-generation, were performed and "the results of all the studies are reported in self-contained studies available at NEPLAN," (p. H-15 of the Report). It would now appear that these reports are not available, if they exist at all, and that NEPOOL's projections for these SIC's, to the extent they rely on the studies, are also undocumented. Despite the reference to self-generation, it appears that potential industrial cogeneration is generally ignored in the NEPOOL forecast.

Q: Does NEPOOL adjust the industrial sales forecast to reflect electric price?

A: Yes. NERA's undocumented elasticities are applied: most of the SIC's long-run elasticities are assumed to be -0.3, which is very small. Other SIC's are assumed to have short-run elasticities as high as -0.45, which seems excessive.

Therefore, long-run price effects will be very small for all industrial use, and may not even compensate for the price effects in the energy intensity trends, let alone capture the effects of recent and future price increases.

Q: What price effects are captured in the energy intensity trends?

A: Two types of price effects are incorporated in these trends, which should not be included. First, some of the long-term adjustments in equipment and processes to the period of falling energy prices in the 1960's must have continued into the 1970's; thus, some of the effects of falling prices are incorporated in those trends. Second, the short-run price elasticities used in the Model (and the calibration) are certainly too high compared to the long-run elasticity used and probably too high in absolute terms as well. As a result, the short-run impacts of the price increases of the 1970's are exaggerated; to yield accurate backcasts, NEPOOL must have exaggerated the energy intensity growth rates as well. For both these reasons, NEPOOL"s energy intensity forecasts are apt to increase far too rapidly.

Q: Do similar problems arise in the commercial submodule as in the residential and industrial submodules?

A: Yes. The same deficiencies in documentation recur. For example, NEPOOL mentions that commercial sales could have been used to drive the submodule, but does not explain why employment was used instead. Other more specific problems arise as well.

NEPOOL estimates 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 prices. Furthermore, the time trend may be inflated by the effects of the gas shortage which occurred during the data gathering period. NEPOOL apparently has not attempted to follow up on this study, to determine whether the trends inferred from 1975 data have persisted. In any case, no significance tests are reported for these crucial equations; there is no indication that the observed time trend is significantly different from no trend or from a negative trend. In fact, the time trend was added late in the estimation process; this is probably because the time trend was not very helpful in explaining energy intensity.

In any case, this poorly documented relation for one sector in two states is extrapolated to all commercial categores in all states. All factors, including the time trend, seasonal usage, air conditioning use, and space heating use, and simply scaled to total sales, with the implicit assumption that construction sites, warehouses, schools and offices all use electricity in the same pattern. This is not plausible.

Q: Is price elasticity handled properly in the commercial sector?

A: No. While the short-run elasticity is reasonable (-.2), 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 of 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). Therefore, the saturation trends should be discarded and the long-run elasticity increased to reflect reality.

Another problem occurs in the commercial air conditioning saturation forecasts. Saturations in 1970 are estimated on the basis of numbers of customers with air conditioning, rather than the number of employees in air conditioned commercial space. Since large commercial customers - large office towers, large stores, shopping malls - are already air conditioned, the fraction of air conditioned space (or employees) probably far exceeds the fraction of air conditioned customers. Therefore, NEPOOL is overestimating the potential for expansion.

Q: Does NEPOOL properly incorporate commercial conservation?

A: No. NEPOOL completely omits any form of mandated conservation, such as revisions in building codes, habitation codes, and lighting levels, temperature limits in space conditioning, and appliance efficiency standards. Some of these measures may impact consumption soon (lighting and temperature levels), while others will gradually improve the efficiency of the building stock. NEPOOL also ignores the potential for commercial cogeneration, which is beginning to be realized by such projects as MASCO.

Q: Are there also problems in the miscellaneous power 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.

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 commercial forecast; there is no indication of how this double counting would be prevented.

Q: Are there any other problems with the NEPOOL demand forecast which transcend individual submodules?

A: At least two such problems are evident in the forecast. First, NEPOOL uses a rather low electric price forecast which is completely undocumented. Second, NEPOOL completely neglects the possibility of reforms in utility rates and operation, such as the establishment of time-of use rates, marginal cost pricing, fair backup and purchased power rates (for cogenerators and other power producers), load management, and utility conservation programs (e.g., voltage regulation, energy efficiency audits and consulting, changes in conditions of service).

Q: Do the results generated by the NEPOOL model confirm the existence of the problems you have discussed?

A: Yes. The model was calibrated on the 1970-1976 period and therefore generally fits well in that period. However, NEPOOL's backcasts for sales growth in 1976 and 1977 (where available) exceed actual growth for each of the major customer classes. Similarly, the model overforecast growth in total output by 1.4 percentage points in 1976, by 4.1 points in 1977 and 3.3 points in 1978. If the average post-calibration error continues in the NEPOOL forecast, output will rise at 0.4% in the 1978-89 period, to a total of only 86520 GWH in 1989, which is 36% less than the NEPOOL forecast for that year and only about 4.5% larger than 1978 output.

Peak growth rates were also overstated in both 1977 an 1978 by 3.5 percentage points. If this error continues in the rest of the forecast period, peak demand will grow at 0.3%, to a peak of 16019 MW in 1989. With existing capacity (minus scheduled retirements and retirements of all capacity now in deactivated reserve), currently planned purchases, and the capacity now under construction, New England would have a reserve margin of 54% in 1989.

Q: Please summarize the NEPOOL forecast.

A: NEPOOL appears to have created a model with numerous unjustified growth-producing assumptions including most of the factors mentioned above. NEPOOL then utilized high short-run elasticities and large commercial conservation corrections to neutralize this excessive growth in the calibration period. Once the calibration period ends, the model grows much too rapidly. Continuation of the infated trends, coupled with new growth-producing assumptions and errors, will produce inflated forecasts.

Q: Does this conclude your testimony on the NEPOOL demand forecast?

A: Yes.

III. COMMENTS ON THE DISPLACEMENT OF OIL GENERATION BY NUCLEAR GENERATION ARGUMENT

Q: Do you have any comments on the testimony proffered by BECO on the general subject of the desirability of building Pilgrim II, independent of future electric demand, on the grounds that Pilgrim II would displace oil-fired generation?

A: Yes. Although the amount of oil Pilgrim II would displace varies directly with future NEPOOL demand for electricity, Pilgrim II surely would displace some oil. However, BECO makes no claim that Pilgrim II is the most cost effective way to reduce New England oil use, as compared to a vast array of alternative uses for two to four billion dollars of capital. Like all "economic" goods, capital is a scarce resource. Some alternatives could displace oil by supplying considerable quantities of electrical capacity and energy; examples would include pond hydro, cogeneration, more extensive use of excess Canadian hydro capacity, and plant biomass or waste materials. Other electrical supply options, such as intermittent hydro and wind generators may provide much more energy than capacity, but that energy will still displace oil. Oil can also be saved by reducing electricity use through rate reforms (elimination of declining blocks, customer and demand charges, and discounts for larger users), conditions of service (strict requirements for energy-efficient appliances and building shell in new construction including passive use of solar energy and natural cooling); elimination of master-metered service; and technical and financial assistansce to customers in insulation of buildings and hot water systems, weatherproofing, selection of efficient appliances, and the like. The return in terms of oil savings may also be very high for investments which neither create additional electric energy nor reduce its use. Examples of such non-electric investments include insulation and other conservation measures in oil-and gas-heated buildings and appliances; conversion of oil-fired boilers (including utility boilers) to partial coal, waste or biomass firing; improvement of existing boiler efficiencies; and utilization of waste heat from industry and utility sources for space and water heating.

Q: Has BECO compared the cost-effectiveness of such a range of oil-saving investments?

A: Not that we are aware of; certainly no such analysis is contained in the May, 1979 "need for power" testimony filed by BECO.

Q: Could BECO implement all of the investments you have listed?

A: Some of the options would require D.P.U. or other regulatory approval, and new legislation might be helpful in other cases, but not should be as difficult as getting approvals for a nuclear power plant and for CWIP charges to finance it. In any case, if Pilgrim II is not built, as much as $4 billion in investment funds may be freed up for homeowners and business to implement conservation measures, which will stand to save oil almost immediately. It is possible that New England will actually use less oil in the next forty years if BECO simply cancels Pilgrim II and makes no other direct investment. Of course, BECO's rate structure and those of other NEPOOL members, including purchased power rates and backup rates, can help to encourage cogeneration and conservation of electricity, at minimal cost to the utilities.

Q: Do you know of any evidence that suggests that investments other than Pilgrim II will be more cost-effective in saving oil?

A: Yes, many analyses have been conducted which indicate that conservation measures, in particular, are very cost effective. For example, the New England Energy Policy Alternatives Study, conducted for D.O.E. and the New England states by the Massachusetts Energy Office (now called the Mass. Office of Energy Resources, an "interested state agency" in these proceedings) concluded that "conservation, at least for the next decade, is the region's best strategy for reducing oil imports, reducing overall energy costs, creating new jobs, and increasing gross regional production." The Study further concluded that "the effect of not building or delaying an additional nuclear plant in the region by 1985, together with economically efficient and attainable conservation in all sectors of the economy, can lead to lower electricity prices and even greater economic benefits than conservation alone."

IV. MISCELLANEOUS COMMENTS ON THE APPLICANT'S CASE

A. DISCOUNT RATES AND COST-BENEFIT ANALYSIS

Q: Does BECO properly discount expenses over time to allow a proper comparison of expenses in different years?

A: No. BECO's discounting procedure contains serious errors which result in an inconsistent and incorrect analysis.

Q: Please explain how a consistent cost-benefit analysis would be conducted.

A: The purpose of performing a cost-benefit analysis is to determine which of several actions (such as building Pilgrim II) has the greatest net benefits to society or to a particular subset of society. Obviously, the first step in such an analysis is the selection of the group whose welfare is to be maximized. Then all the costs and benefits to that group must be estimated for each alternative for each year in which such benefits differ between alternatives. Finally, the costs and benefits occurring in various years must be converted into comparable units, by discounting the several annual figures back (or forward) to a common year, utilizing an appropriate estimate of the time value of money to the target group.

In performing its evaluation of a three-year delay of Pilgrim II, BECO appears to select New England ratepayers as the target group. This choice is apparent in BECO's calculation of the annual capital and operating expenses which a hypothetical ratemaking body might pass through to the consumers in the service territories of the owners of Pilgrim II, and of the annual fuel costs to New England. Alternative target groups would include society (all of New England, the U.S. or the world), the customers of a particular subset of New Enland electric companies, or the stockholders of those companies. The types of costs included in the analysis, as well as their timing, varies from one target group to the next. For example, if New England society is the target, local and state taxes are not a cost at all; if the world is a target, oil should be priced (for the evaluation) at its real replacement cost, not at the higher cartel price. If the evaluation is being performed from the point of view of the companies, construction expenses should be assessed as they are incurred by the utilities, not when they are later passed on to the customers; from the perspective of society, the expenses are probably incurred even earlier, when manpower, machinery and materials are first committed to the production of components.

Q: What inconsistencies arise in BECO's cost-benefit analysis?

A: First, BECO acts as if the ratepayers of some forty electric companies were all customers of one company. The owners of Pilgrim II include both private and public utilities, who are variously regulated by about five state commissions, by FERC, and some others of whom are unregulated. Some owners utilize CWIP charges, others do not (and cannot); some charge low costs of capital, other charge high costs. Therefore, any resemblence between BECO's analysis and the actual costs incurred by New England electric customers in various years is largely coincidental, even if BECO's estimate of capital, operating, and fuel costs are accurate.

While BECO's aggregation problem is serious, it is overshadowed by an even more dramatic error. After calculating expenses in each year as if they were incurred by hypothetical ratepayers, BECO discounts using BECO's own cost of capital, instead of using a composite cost of capital to the ratepayers. This is completely incorrect and inconsistent. BECO can consistently use (earlier) company expenses and (lower) company cost of capital (actually, the calculation should be performed for each owner, or an appropriate composite of owners), or consistently use (later) ratepayer expenses and (higher) ratepayer cost of capital, but not a mixture of the two. BECO finally estimates the net present value to hypothetical ratepayers who happen to have BECO's estimated cost of capital.

Q: Does this error represent a clear bias in one direction?

A: Yes. It is extremely likely that the discount rates of New England ratepayers exceed 10.83%. The composite cost of capital may be as much as twice the value BECO uses. BECO's error will tend to exaggerate the benefits of capital expenditures (e.g., Pilgrim II) which are projected to reduce future operating costs.

Q: Does BECO properly estimate its own cost of capital?

A: No. The relevant cost of capital is the total increase in annual payments (interest, dividends, taxes) necessary to finance Pilgrim II. Old bonds issued at 3% interest rates are quite irrelevant to the cost of Pilgrim II which will be financed with bonds paying closer to 10%. Therefore, it is future, not historical, cost of capital which must be estimated. Additionally, equity costs require additional tax revenues from the ratepayers (if we assume the ratepayers are the subset of society whose welfare is being maximized), and thus cost of capital estimates should include this tax effect.

Q: Does this error extend beyond the area of discount rates?

A: Yes. BECO also appears to use an unrealistically low cost of capital in its AFUDC rates, which will tend to understate the true cost of Pilgrim II to the company's ratepayers (and New England ratepayers as well, since BECO rates are apparently assumed for all owners).

Q: You suggested several alternative consistent target groups for the purpose of evaluating Pilgrim II. Is each of those groups equally relevant for policy purposes?

A: Presumably, the NRC is not interested in whether Pilgrim II would be good for BECO's stockholders or managers, so analyses based on BECO's perspectives are meaningless for this proceeding. Beyond that, the NRC may determine whether Pilgrim II can be justified simply for its effects on ratepayers qua ratepayers, or whether the impact on New Englanders (or Americans) as taxpayers, wage earners, or consumers of other goods, is also relevant. For example, building Pilgrim II might conceivably decrease electric rates, but increase taxes, interest rates, and unemployment (compared to alternative uses of the capital), so that ratepayers were actually worse off with it than without it. Only a regional cost-benefit analysis can evaluate the total impact of Pilgrim II on New England.

Q: Has such a study been conducted?

A: Yes. The NEEPA study measured at least some of the major impacts of nuclear construction and concluded that New England would be better off with fewer nuclear plants and more conservation.

B. CAPITAL COST ESTIMATES

Q: Does BECO estimate Pilgrim II's capital cost in a reasonable manner?

A: No. The BECO cost estimate is based on Bechtel estimates for contractor scope and on BECO estimates for AFUDC, potential contingency, and other owner's costs. As discussed elsewhere in this testimony, BECO's calculation of the time value of money used during construction appears to be improper and understated. At this point, we will just discuss the direct capital costs. Bechtel has estimated costs for many facilities, including many nuclear power plants, prior to constructing them. This set of facilities provides a potentially useful data base for verifying the accuracy of Bechtel's cost projections and for correcting estimation procedures to yield better estimates. However, Bechtel has apparently never attempted to utilize this historical data to compare estimated and actual costs. While our requests in other regulatory proceedings for the data necessary to comprehensively assess Bechtel's past performance have to date been unsuccessful, we have been able to obtain one relevant comparison. According to Russell J. Maroni, BECO's Nuclear Planning and Cost Control Group Leader, Pilgrim I (another nuclear plant for which Bechtel and BECO prepared cost estimates) was originally estimated to cost $110 million at the time of the granting of its construction permit, and actually cost $230 million when it went commercial five years later. This is a cost overrun of 109%; extrapolating to Pilgrim II's construction schedule of 6.5 years yields a 142% overrun for exponential extrapolation.

This analysis suggests that the Bechtel/BECO cost estimate of Pilgrim II would be more reasonable if it were multiplied by a factor on the order of 2.0 to 2.6 to correct for their tendency to underestimate. It is therefore more reasonable to expect Pilgrim II to cost in the neighborhood of $3.79 billion to $4.93 billion, rather than $1.895 billion. This does not include any correction for BECO's errors in calculating AFUDC rates.

The reasonableness of these corrected figures is confirmed by their agreement with the results of W.E. Mooz's statistical study for D.O.E., Cost Analysis of Light Water Reactor Power Plants (June, 1978). Mooz's final regression equations indicate that a continuation of past trends would result in a total cost for Pilgrim II in the $3.40 to $3.54 billion range when it went commercial.

Q: Does the capital cost of a generating plant remain constant after it goes on line?

A: Not necessarily. Equipment, and therefore additional capital costs, may be added to the plant. BECO has not captured these costs in its past analyses, even though

Pilgrim I's <u>capital</u> cost has increased by an average of 1% to 2% per year since in went on line in 1972. Some comparable cost factor should be added to BECO's cost estimate for Pilgrim II, unless appropriate analyses indicate that some other rate of capital cost addition is more likely.

C. <u>OPERATION AND MAINTENANCE EXPENSES</u>

Q: How does BECO estimate nuclear operation and maintenance costs?

A: From the testimony filed by the Applicant in May, 1979, it is not possible to determine what O+M values were used in BECO's analysis nor how they were derived. However, the "Capital" column of Ex. NP 37, NP 39, and NP 41 appears to be similar to, through smaller than comparable figures in Mr. Legrow's calculations for D.P.U. 19494, which are disaggregated. In that case (D.P.U. 19494), BECO simply escalated Pilgrim I 1978 O+M at 6% to estimate Pilgrim II's O+M. This implies that BECO believes that O+M costs rise roughly at the same rate as the consumer price index.

Q: Is this assumption consistent with historical experience?

A: It certainly has not been true for Pilgrim I, for which O+M expense (excluding refueling) rose at an annual rate of 15.3% above the CPI from 1973 to 1978.

The least-square exponential trend line through the Pilgrim I data shows an annual real growth rate of 14.8% (r^2 = .82). If that trend continues, Pilgrim I annual O+M (including a constant real cost for refueling) will be $83 million in 1986, $185 million in 1990, $508 million in 1995, and $1.4 billion in the year 2000. A linear trend also fits the data well (r^2 = .819) and projects Pilgrim I O+M of $49 million 1986, $71 million in 1990, $116 million in 1995, and $183 million in 2000. By way of contrast, BECO estimates Pilgrim II O+M of 22 million in 1986, $28 million in 1990, $37 million in 1995, and 450 million in 2000. (These values projected by BECO are actually slightly less than 1978 Pilgrim I O+M escalated at 6%.)

Q: Do you believe tha Pilgrim II O+M will actually be as expensive as indicated by the historic trends?

A: It is rather hard to believe that the exponential trends can continue indefinitely at historic rates. Nevertheless, the trends are quite real, not just for Pilgrim I, but for virtually all nuclear plants. O+M at Millstone I, for example, rose at 29% annually above inflation from 1971 to 1975, and a recent survey (B. Feldman, Testimony before the Massachusetts Department of Public Utilities, Docket No. 19494) of 19 plants indicated that O+M had been growing at about 22% annually in nominal terms. When, and indeed whether, this trend will moderate in the future is not clear. A more detailed analysis of cost trends, as well as their technical and institutional causation, would be required to confidently predict future O+M expense.

Q: Is the extrapolation of Pilgrim I O+M to Pilgrim II a reasonable procedure?

A: Considering that Pilgrim II will be designed for 70% more output than Pilgrim I, the assumption that the two plants will have equal O+M expenses implies a belief in perfect economies of scale in nuclear plant operation. We are not aware of any evidence presented by BECO to support this extreme position. If strong, but not total, economies of scale prevail, such as those described by a scaling factor of 0.5, Pilgrim II would still cost 30% more in O+M than Pilgrim I.

In addition, BECO does not appear to have determined whether Pilgrim I is apt to be the best predictor of Pilgrim II O+M cost. While ownership and location may be important factors in determining O+M, so may plant size, design, engineer, vintage, time, and other factors. It should be possible for BECO to statistically test their implicit hypothesis that only the factors common to the Pilgrim units affect O+M expense, and that none of these other factors does have any effect.

D. NEPOOL NEED VS. BOSTON EDISON NEED

Q: Does BECO's testimony on need for power indicate why BECO believes that it needs to own more capacity than it now owns?

A: No. The Applicant's evidence relates entirely to NEPOOL's alleged need for power. It is not at all clear why BECO wishes to own any additional capacity, let alone be the lead participant in an 1150 MW plant.

Q: Does BECO's testimony on need for power indicate where in New England they believe additional capacity will be needed?

A: No. Since New England has an interconnected electric system, demand anywhere in the region can be, and is, satisfied by generating plants throughout the region and even beyond. Due to NEPOOL's failure to forecast demand on a level of detail below that of the states, and the complex interaction of demand, generation, and transmission, NEPOOL's forecast does not and cannot directly establish whether there is any advantage in locating additional generation in Southeastern Massachusetts or whether BECO (as opposed to other utilities) should build any additional capacity which may be needed by other participants of NEPOOL.