COWLES FOUNDATION FOR RESEARCH IN ECONOMICS
AT YALE UNIVERSITY

Box 2125, Yale Station
New Haven, Connecticut 06520

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ECONOMICS AMONG THE SCIENCES

by

Tjalling C. Koopmans

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The title of my address implicitly assumes that economics is itself one of the sciences. I believe that to be so, and intend as I go on to indicate more fully in what sense I hold that view. However, my principal aim in choosing my topic is not that of claiming any particular status for economic analysis. Rather, I want to share with you some observations I have made over the last six years as a result of involvement in various interdisciplinary studies, through reading the reports of other such studies, or discussing them with colleagues in various fields of science.

With increasing frequency natural and social scientists are indeed finding themselves thrown together in the study of new problems that are of great practical importance for society, and essentially interdisciplinary in character. Prominent among these are problems of environmental policy, such as the protection of air and water quality. Another class of problems concerns a desirable long-range mix of technologies of energy supply, conversion and use. These

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two classes of problems overlap, for instance, with respect to the disposal of nuclear wastes, heat rejection to the environment, and—in the case of fossil fuels—the as yet poorly understood global and regional effects of sustained large releases of carbon dioxide into the atmosphere.

Assembled in pursuit of such studies, our interdisciplinary group soon finds that its diverse participants ask different questions; use different concepts; use different terms for the same concept and the same term with different meanings; explicitly or implicitly make different assumptions; and perceive different opportunities for empirical verification—which may lead them to apply different methods to that end. The result can be politely concealed bewilderment, possibly a suppressed surge of "we-and-they" feeling, in the worst case a growing mistrust that only time and sustained interaction can overcome.

I shall try to illustrate the difficulties of such interaction by a few examples from recent studies involving, besides economics, mostly the natural sciences and engineering. Limitations of experience, background and time have compelled me to omit examples involving a strong participation from the other social sciences. Had my guide, mentor and dear friend Jacob Marschak lived to give this address, and had he chosen a similar topic, the social sciences would have received an emphasis reflecting their importance to the problems of contemporary society. The writings Marschak left us, and the program and the Proceedings of last year's meeting of the American Economic Association, stand together as a monument to his awareness and vision of the actual and potential contributions of the social and behavioral sciences.

To prepare for the task I have set myself, I have requested and obtained interviews with a somewhat casually selected sample of natural scientists and engineers, and with a few colleagues in economics. Their responses have been drawn on in the preparation of this address, without attribution by name. I here express my, and indeed our, indebtedness for the help we have been given. Later on, I will cite some statements verbatim.

Table I can serve as a two-dimensional table of contents for my discussion. Three topics of study are listed on the left. On each of the three topics a recent study has been made by or for the National Research Council. I will draw mostly on the first two studies and briefly mention the third.

My principal intent is not that of criticizing these studies or of evaluating their findings. I want merely to identify some of the issues that arise in their formulation, contrast responses to these issues in different professions, and comment on the methods that have been or might have been proposed or applied by the respective collaborating professions. Those issues and methods that I shall have time to refer to are set out along the top of the Chart. Each check mark in a cell of the Chart indicates that a reference is made to that issue or method in my discussion of that topic of study.

I. The Case for Helium Conservation

This study is described in the preface to the Report[1] as "a task that had to be undertaken quickly and completed with great speed".

TABLE I

	ISSUES AND METHODS IN ESTIMATING BENEFITS AND COSTS						
	Measures of Value			Estimation From			
ILLUSTRATIVE DECISION PROBLEMS	GNP (a)	Health and Life (b)	Energy (c)	Production Process and other Technical Data (d)	Market Behavior (e)	Dis-counting (f)	Uncer-tainty (g)
(1) Helium Conservation	✓		✓	✓		✓	✓
(2) Technology Mix of Future Energy Supply and Use	✓	✓		✓	✓	✓	✓
(3) Automobile Emission Control					✓		✓

Note: Check marks designate issues or methods discussed or mentioned for each illustrative problem.

Likewise, on the concluding page (40), it is called a "preliminary analysis".

The principal current source of helium is as an optional by-product of the production of natural gas, in which it may occur in concentrations ranging (by volume) from 10% on down with increasing costs of separation. Present demand for various industrial and space uses falls below present supply, and a program of storage in the partially depleted Cliffside gas field near Amarillo, Texas, is in operation. The study is motivated by the anticipation of a substantially higher future demand.

The report of the Helium Study Committee lists, on pages 35-36, five steps that can be taken for the purpose of increasing the rate of storage.

Step i: Stop the current venting of helium which has been separated from natural gas allocated as a feedstock to petrochemical industries. Store the helium instead.

Step ii: Designate helium currently stored in Cliffside a "national strategic reserve" for possible major technical changes that greatly expand future demand.

Step iii: Reactivate presently idle separation plants to reduce the release of helium resulting from productive combustion of the host gas, and store the helium instead.

Step iv: Build new helium separation plants on helium-rich gas streams. Store the helium.

Step v: Delay the use of undeveloped helium-rich gas fields.

Ultimate Step: Extract helium from the atmosphere.

The ultimate step is not included in the report of the Committee, but is mentioned in the transcript, page 135, of the Public Forum held as part of the study. It involves a process that by present technology costs a large multiple of the cost of extraction from natural gas containing .3 to .5% helium.

Steps i, iii, iv and the "ultimate step" consist of successive technical process choices. Taken in that order, they correspond to the economist's notion of a long-run supply curve, indicating how the cost of each additional unit of supply is a rising step function of the cumulative supply up to that point—assuming a constant state of the technology of extraction. These steps need to be carried out only according as the expectation of demand growth becomes larger and firmer. Steps ii and v, shown indented, are steps whose timing should depend on additional factors besides the separation cost sequence already mentioned.

The expectation of a much larger demand well into the 21st century is documented, in the Report and in the Forum, by a fascinating enumeration of anticipated future technologies. Many of these are based on or may utilize superconductivity, so far attained best by cryogenic techniques for which helium is the working fluid. The superconductivity is in turn expected to be applicable to a number of uses, such as power transmission with low energy loss, energy storage, and a number of applications of strong magnetic fields. Among the latter are several "technologies that either do not now exist or are in early stages of development (pp. 13, 18)," such as magnetic containment for nuclear fusion reactors. Another possible application is magnetohydrodynamic (MHD) power generation that converts some of the energy contained in a high temperature gas stream from either a coal-fired burner or a nuclear fission reactor directly into electircity, instead of routing all the energy through a conventional steam cycle. The MHD development is further along in the Soviet Union. There also is a development — furthest along in West Germany and Japan — of magnetically levitated low-noise high-speed trains.

It should be added that research is in progress on the use of aluminum and possibly other materials reaching low resistivity at temperatures of 20 - 30°K, a range reachable using liquid hydrogen as a collant.[2,3]

From the economic point of view, the case for the helium storage program is not convincingly made either in the Report or in the Forum. I have not found either cost or benefit estimates for the program. Actually, because of the importance of energy supply processes among the increased uses of helium listed above, the benefits cannot be estimated without comparable cost and fuel availability estimates for alternative energy supply and use technologies which have low or zero helium requirements. In other words, to assess the helium storage program, one also needs a long-run model of the energy sector of the economy that addresses our Problem (2). One will, of course, also need to consider other important helium uses that are not directly energy-related.

Before turning to Problem (2), I draw attention to a few passages in the Helium Report that will provide background for Sections III and IV below, where I shall discuss the choice of measures of value in which to express benefits and costs. The Report (p. 23) contains an important piece of information bearing on cost comparisons, to the effect that the energy requirements for extracting helium from the atmosphere are about 1000 times those for extracting it from natural gas containing .3 to .5% of helium. Large as that figure is, I shall describe later the economist's case (Section III) for including in the calculation inputs other than energy, such as that for plant and equipment, and (Section IV) for taking into account that the costs of all kinds in any required future extraction from the atmosphere will not be incurred until much later.

In fact, one statement in the Report reads as a rejection of the idea that the time at which capital cost is incurred is at all pertinent. In a description of the possible role of the Government in implementing the five steps, the Report says (on page 38):

> "The burden of the discount rate as a criterion of performance could be eliminated and the present debt to the U. S. Treasury written off."

I shall explain in Section IV why I think not many economists will support the proposal to eliminate the criterion of the discount rate. Meanwhile, the statement leads one to infer that the capital cost component is not neglible as a factor in the decision.

II. Technology Mixes of Future Energy Supply and Use

My second illustration is a study[4] that was carried out as an input to the deliberations of the Committee on Nuclear and Alternative Energy Systems (CONAES, in short), and of its Synthesis Panel. As explained in the preface, it is a "supporting paper" published without having gone through the customary report review procedure of the Academy. While for the other illustrations I have not named authors or committee chairpersons, I should not conceal that I was the chairman of the group, called the Modeling Resource Group (MRG), which collectively did the work described in its Report. The group consisted mostly of economists and operations researchers, two somewhat like-minded professions. My comments on interdisciplinary interactions about the ideas and findings of the group will therefore draw on discussions with members of other professions within and outside CONAES.

For that purpose it may suffice to give only the briefest description of the questions addressed, the assumptions made, and the methods used. One important question arises from the fact that several competing objectives enter into the choice of a long-run energy technology mix. The net economic effect (economic benefit minus cost) of the development of a given technology mix can be estimated in a crude way, as suggested in Table I, column (a), by its effect on the Gross National Product (GNP). In addition, one will also want to register risks of adverse effects such as mining accidents, air pollution, acid rain, oil spills, possible leakage from nuclear waste disposal, or diversion or proliferation of weapons-grade nuclear materials. For brevity, all such effects will be called "environmental" effects. The place in Chart 1 for these impacts is Column (b), tersely dubbed "health and life" .

The Risk/Impact Panel of CONAES decided not to try to estimate money equivalents for such adverse impacts of various magnitudes. Were such estimates possible and available, then one could also define and find a balance between desired benefits and adverse "environmental" impacts that remain after scrubbers, inspectors, Civex and the like have done their jobs. Not having such estimates, the MRG turned the question around: Assume that tentatively chosen upper bounds are imposed on the use of technologies that have such impacts. Estimate the loss in GNP associated with these bounds. Then that number also places a price tag on the reduction in "environmental" impacts achieved by those bounds. Thus, even if an a priori valuation of the reduction in impacts is not available, then such a valuation is still implicit in any decision actually taken. It may help the decision makers to know these implicit price tags.

I will list only the principal assumptions made for this purpose. Numerical values were assigned to three sets of variables. As principal exogenous, also called "realization", variables were chosen

R1. The growth rate of real GNP, out to 2010, in the absence of new environmental bounds on energy technologies,

R2. Capital cost levels of present and potential future energy technologies,

R3. Availabilities of oil, gas, uranium, at various costs of extraction,

R4. Long-run price and income elasticities of demand for end-use energy forms.

The "policy" variables represent the hypothetical bounds already described,

P1. Moratoria on new nuclear construction,

P2. Limits placed on output of coal and shale oil.

Forming a third category, the "blend" variables have traits of both realization and policy variables,

B1. Discount rates applied to future benefits and costs,

B.2 Oil import Price or Quantity Ceilings.

The method applied was to compare the already specified projection of a rising future GNP in which no bounds have been imposed on the use of energy technologies (the *base case*), with other projections in which such a bound or bounds were imposed. This procedure was carried out for each of three long-run models of the U. S. energy sector. The numerical inputs into the three models were the same for almost all realization and blend variables, except for the price and income elasticities of demand, which were specific to each model. Two ideas central to current economic analysis entered into this procedure. One is the use of an optimization algorithm to simulate the behavior of a competitive market economy, in any one year, and through time. The other is the use of long-run elasticities of demand. For demand by end use consumers, these are to be based on econometric analysis of time series and/or cross sections of income, prices, and quantities consumed. For industrial demand, a process analysis of alternative industrial energy-using processes may add valuable information.

The principal finding was the proportionally small effect on GNP of sizable cuts in energy use below its base case growth path. In interpreting this finding, note that the optimization procedure implies an assumption whereby the economy responds to anticipated changes by minimizing the cost of adaptation. The principal means of adaptation are changes in the type and composition of the capital equipment for the extraction, conversion, transport and use of energy — at the regular time for replacement or earlier.

Table 2 shows the numerical results. For two models, with

TABLE 2

ESTIMATED FEEDBACK FROM CURTAILED GROWTH OF ENERGY USE TO GNP, 1975-2010, U. S.

Policies	Reduction out of the Base Case in		Price Elasticity of Demand for Energy
	Energy Use in 2010	Discounted* Sum of GNP 1975-2010	
(1) Bounds on Specific Technologies	Up to 20%	1 to 2%	-.25 or -.4
(2) Zero Energy Growth thru Conservation Tax	60%	1 to 2%	-.5
		up to 30%	-.25

Source: MRG Report, [4], Tables III.22 and III.23.

*Discount Rate: 6% p.a.

price elasticities of -.25 and -.4 , respectively, policies entailing percentage cuts in energy use out of the base case that gradually increase to between 10 and 20% in the year 2010, were found to cut not more than 2% out of the discounted sum of annual real GNP, 1975-2010, and a comparably small percentage out of GNP for the single year 2010. The instruments of the curtailment of growth in energy use were, in row (1) of the Table, the placement of bounds on specific energy supply technologies described above. In row (2), a zero-energy-growth path is simulated by the imposition of a hypothetical "conservation tax" on primary energy flows. The rate of this fictitious tax must increase as the GNP continues to grow in spite of the downward pull from the zero-energy-growth path.

Another finding, reported in row (2), was that for the effects of the larger cuts in energy use, the value of the long-run price elasticity of demand for energy becomes crucial. In a sensitivity analysis made with one model, a zero-energy-growth policy from 1975 on, leading to a 60% cut in energy use out of the base case in 2010, was found to induce only a 2% cut in cumulative discounted GNP if that price elasticity is -.5, but a 30% cut if it is -.25. I shall come back later to the estimation of the elasticity parameters found to have been very important.

III. Interdisciplinary Differences in Outlook

We have now assembled enough reference material for us to make a start with our main topic—the way in which differences in outlook between the disciplines affect the conduct and evaluation of joint studies.

The most significant difference between economics and the natural sciences lies in the opportunities for testing and verification of hypotheses. Jacob Marschak used to say that economists carry the combined burdens of meteorologists and engineers. Like the meteorologists, they are expected to predict the future course of important variables in their field of study. Just as engineers design more and more efficient machines, economists are also expected to improve the design of society where it affects good use of resources. But, like the meteorologist, the economist has traditionally been confined to drawing inferences from passive observations, records of data generated by the turbulence of the atmosphere or the fluctuations and trends of economic life. Finally—a very important difference—meteorologists and engineers have all the laws and measurements established by physics and chemistry available to them, fully documented by experimental tests and results.

Traditionally, economists have not searched for similar inputs from experimental or observational research of a psychological or sociological nature. In the fifties and early sixties they have engaged in some experimentation of their own on behavior under uncertainty and in bargaining and gaming situations. However, the findings of this work have not been put to use as premises for modeling an entire economy. For that purpose, over a few articulate protests, many economists have been satisfied to postulate simple rules of behavior by consumers and business firms. The terms "introspection" and "casual empiricism" have been used to describe the cognitive sources of these premises. In the version of the currently dominant "neoclassical" school of thought, these premises express optimizing responses of demand and supply to a uniform price system: satisfaction-maximizing by consumers, profit-maximizing by firms.

These premises have a certain intuitive plausibility about them. Undoubtedly, their widespread adoption has also been aided by the richness of the body of inferences one can draw from them. In fact, the premises form the logical foundation for the paradigm of neoclassical economics: the concept of an equilibrium of prices and quantities that in some way ties together the economic decisions taken by all seemingly independent agents. Conceptually the prices and quantity responses may describe a stationary state over an extended period of time. More realistically, they will be dated variables and thus also link decisions that vary over successive periods, to sustain a moving intertemporal equilibrium.

Parenthetically, use of the term equilibrium does not imply an assumption that the real economy actually is at any time "in equilibrium". Rather, the notion of equilibrium is a first approximation, a reference point or path, like the cycles of Ptolemy without the epicycles and the eccentricity.

If the market were to extend to all pertinent economic decisions over the entire period considered, the result of an intertemporal equilibrium would be an "efficient" path of the economy in the limited sense that no one can be made better off at any time without someone being made worse off at some time. Where market power interferes with competition, or where important economic decisions are made at government levels, the instinct of the neoclassical economist is to recommend that legislation, regulation, the use of suitable incentives or direct government decision either restore or mimick the operation of the competitive market.

In the present context, an important trait of the neoclassical model is that it does not postulate one sole primary resource, be it labor, energy or any other, whose scarcity controls that of all other goods, and which thereby becomes a natural unit of value for all other goods. The model of production is such that — not by logical necessity, but as an empirical fact — any primary input to production can be substituted to some extent for any other. If such substitution does not take place within one-and-the-same production process, then it can still come about through suitable changes in the levels of several processes and in the inputs to these. In this view "the energy problem" is not one of just "saving energy", regardless of the cost in other resources. It is rather one of seeing to it that the increasing real cost of domestic energy extraction and supply, and the increased market power of OPEC, are — over time — reflected in the real prices of primary energy forms relative to other primary inputs, and thereby in different degrees in the prices of all other goods and services. In the projections described above, the energy prices are calculated so as to be in balance with an efficient path of the technology mix into the future, and thereby to induce the right amount of energy saving. In particular if, as projected by MRG, real prices of primary energy rise in this path, then energy use is projected to grow less than proportionally to GNP.

The contrary doctrine—that regardless of prices there is a persistent relationship between energy use and GNP—has frequently been expressed in the engineering literature. In line with this observation, the MRG finding of a possible small impact on GNP of incisive bounds on specific energy technologies led to lively correspondence and discussions with members of CONAES, and of its Supply/Delivery Panel, an engineering-oriented group. I should add that the MRG study was not the first modeling study to cast doubt on the doctrine referred to. By my knowledge the first was a study by Ed Hudson and Dale Jorgenson summarized in [5].

IV. Discounting Future Benefits and Costs

We are now ready for a closer look at the discounting of future benefits and costs. This practice reflects a simple technological fact combined with the paradigm of equilibrium over time. The simple fact is that — short of capital saturation — society can temporarily curtail the production of current consumption goods by transferring some factors of production to the formation of additional suitable capital goods, in such a way as to return a multiple (> 1) of the same unit bundle of consumption goods in the future. Efficient intertemporal equilibrium then demands that the present value of the goods returned to consumption be equal to that of the goods not now consumed. The quantity of the future bundle being larger, its *per-unit* present value must be correspondingly lower. In a projection that gives to one unit of the future bundle a future real market price numerically equal to its current price, a discount factor $d < 1$ must be applied to the future market price to obtain the present value, per unit of the future bundle. Given competitive markets for capital, present goods and future goods, and ignoring differences in risk, different investments bearing fruit in the same future year t will tend to give rise to the same discount factor

$$d_t = \left(\frac{1}{1+r_t}\right)^t \quad ,$$

where r_t is the annual discount rate applicable to the period from year zero to year t. The usual practice in cost-benefit analyses is to assume also that r_t is independent of t, $r_t = r$, say.

This reasoning simply registers the economic accounting implications of assumed intertemporal efficiency with capital nonsaturation. To many highly educated people, there is something ethically offensive about it.

A difficult practical problem on which economists still differ among themselves is how to read a good estimate for r from capital market and other data. Different tax rates on corporate and individual incomes complicate the problem. Considering this and various market imperfections, the precluded alternative use of funds drawn upon for a public project also enters into the choice of r. I will not venture into these questions here.

Coming back to helium storage, the discounting criterion would lead most economists to recommend that those steps of the storage program be implemented for which the rate of return on the total investment (not that of energy alone) exceeds or equals the discount rate appropriate to the problem. Step I, storing helium currently vented, is likely to meet the test. The problem is to estimate which of the four or six steps would.

Two final remarks, the first added as an afterthought since August 30.

The two issues we have discussed—whether to count only energy costs or all costs, and whether or not to discount future benefits and costs—are logically distinct implications of the notion of intertemporal equilibrium. However, psychologically they are related. If one counts only energy costs, everything is expressible in equivalent Btu's, and to the physicist, steeped in the law of conservation of energy, Btu's are the same everywhere and at all times. To discount future Btu's therefore seems not just strange but outright wrong. So it is. But the economist does not discount quantities of any kind. He discounts only real values, that is, quantities (including energy) multiplied by real prices that reflect the expected balance of cost and preference as of a specified future time and beyond. It is to these prices that the discount factor is applied. I am hoping that this simple distinction may help to reduce misunderstanding between the professions.

Secondly, our reasoning has proceeded blandly as if there were no uncertainty about the outcome of the development of processes expected to be substantial users of helium. If there is considerable uncertainty, economists may want to add an allowance for risk to the discount rate. They may also wish to experiment with models in which judgmental probabilities are attached to these uncertain outcomes. This device may produce insights even if the conclusions depend on admittedly uncertain premises. A study of this kind is included as Chapter IV in the MRG Report.

V. Attaching Values to Health and Life

The question of estimating the value of health and the value of life arises mostly in contexts where either public decisions, or public monitoring of private decisions, can be shaped so as to improve health and prolong life. One example is the investment of public funds to diminish physical risks to traffic by the design of roads, bridges, turnouts and crossings. Another is regular expenditures for traffic police, building inspectors and other law enforcers who restrain some people from killing or hurting themselves or others by recklessness or neglect.

A common trait of these decisions is that from good experience records one may be able to estimate the years of lives saved, perhaps also of health and limbs preserved, per dollar spent on efficiently run projects or activities of this kind. Such calculations make it possible to spot discrepancies between different projects in regard to "health and life benefits" bought per dollar spent. The ideal of equilibrium then suggests redistributing expenditures, if needed, in order to maximize total benefits from the given expenditure for protection. Valuations of health or life that have a modicum of public approval could result from such redistribution. Note that these valuations, also called *shadow prices* are in effect set by the budgetary decision makers, whether they are aware of it or not.

After such redistribution if called for, the calculation of money values of health and life registers what in good practice we consistently spend to save a life. The process recognizes that, disturbing as it is to our sensibilities, society is being compelled by the facts of technology and behavior to set up equivalences between lives of unidentified people and bundles of goods and services implicitly of the same market or shadow value—thus bracketing contemporary lives together with current goods and services in the same category of exchangeables.

The examples given so far concern small to moderate risks affecting small to moderate numbers of people, less than one hundred at a time, say. Moreover, the time intervals between the decision to commit funds for the reduction of risks, the actual expenditure of these funds, and the reaping of benefits therefrom are moderate, less than twenty years, say. Finally, the problems are mostly local or national, not international in scope.

The long-run choices between energy technology mixes are different in these respects. By a gradual shift from oil and gas to coal, fossil fuels can remain a principal source of energy for countries with abundant resources of coal, especially the U.S., the U.S.S.R., and China, for a long time to come. Intensive current discussion with regard to this option concerns the possible climatic effects of the increase in the atmospheric concentration of carbon dioxide caused by continued large-scale combustion of fossil fuels or their derivatives, alongside with world-wide deforestation. Among the large-scale effects held possible are an increase in average global temperature, entailing dislocation of agriculture depending on how each region is affected, and an increase in the level of the oceans due to the melting of polar ice not previously floating. The present state of knowledge is not such as to be ready for an assessment of these risks. New hypotheses and observations appear regularly in the pages of *Science* and other journals. So I would describe this problem as involving an unknown risk to a large number of people.

If current estimates of the capital cost of central station solar power are realistic, the principal alternative to fossil fuels for bulk power generation is nuclear power. I am not qualified to even comment on the reactor safety and waste disposal problems. I assume, however, that the developers of these technologies would classify these problems in terms of very small risks to substantial numbers of people. Perhaps this leaves as the principal concern the difficulty of keeping industrial and weapons use of nuclear materials apart. Since on this one we are all groping in the dark, I feel I should describe this aspect of nuclear technology as an unknown risk to a very large number of people.

I cannot see my way through to a calculus of the value of human lives in large numbers, that would help clarify issues of the scope of those just discussed—although estimates of numbers of lives at risk are and will remain important. These are basically problems for judgment, even though the need for making these judgments will weigh hard on the people called upon to make them. But supposing I should be wrong, let me point to one apparent paradox to be faced in any attempt to bring a calculus of the shadow price of human life to bear on problems with a long time span.

Suppose one accepts as an ethical principle that, in balancing risks to human life in the present and in the future, equal numbers of lives should receive equal weight. This would make the present value of the future human life independent of the time at which it is lived. However, we have seen that as long as capital saturation is not attained, the present value of a standard bundle of goods in the future decreases as that future time recedes. Hence the present value of future life relative to that of future goods will be much higher than the value of present life in relation to present goods. It should not be inferred from this that future decision makers are assumed or advised to devote greater resources to safety and health than the present decision makers, although the future ones may well want to do this for reasons of their own. The inference is rather, I submit, that the present values I have described reflect a curious mixture of three ingredients: one intertemporal ethical rule, present preferences between consumption and protection, and an assumption about savings behavior of all generations within the next fifty years, say. Under these assumptions, sets of "present values" formed at successive points in time need not, and generally will not, be consistent with each other.

VI. The Empirical Basis of Quantitative Economics

I now go on to a discussion of the empirical basis for some of the quantitative statements that economics contributes to interdisciplinary studies. I will again illustrate this question with reference to the few studies I have chosen as examples. At the same time I will emphasize the role that the premises underlying the concept of equilibrium play in this process.

The premise of profit maximization implies a sub-premise of cost minimization. I regard that sub-premise as fitting reality more closely than the entire premise. This assumption underlies the supply side of the MRG study of future energy technology mixes I have described.

The premise of maximization of satisfaction by the consumer can be made more plausible and more applicable by a further specification. Applied to energy, it says that successive equal additions to a consumer's annual energy end use budget are worth less and less to him. Operationally, how much each successive addition is worth to him can be measured, for instance, by that increase in the expenditure for the rest of his consumption that he would have regarded as equivalent to each next addition to his energy consumption.

This specification implies the existence of a household demand function for energy, in which per capita demand for energy decreases as its price increases, and increases as per capita real income increases. The MRG extended this concept to the sum of direct and indirect demand for energy, the latter being the energy used as input to the production of all non-energy goods, including capital goods as well. Another extension distinguishes demand for individual fuels, where the demand for one fuel increases if the price of another competing fuel goes up.

These functions are then estimated from empirical data. In the procedure followed in the model with the estimated long-run price elasticity of -.4 mentioned above, a parametric form of these functions was fitted to cross section and time series data for seven OECD countries, including the U.S., for the period 1955-72. In the model with price elasticity -.25 the estimation procedure was not stated with comparable explicitness. In both models the estimated long-run demand functions, written with price as a function of quantity, were then integrated to estimate the benefit from the consumption of energy in all forms.

By comparison, the empirical basis for the production side is more direct. Each of the various competing energy producing, converting and using processes is represented by constant ratios of inputs to outputs, reflecting operating experience where available, or based on estimates of such ratios and of future availability dates for processes not yet developed. For instance, process estimates for the years 1985 and 2000 were drawn upon in estimating the elasticity of substitution between electric and nonelectric energy in the second of the two models just discussed. This did constrain but not by itself imply numerical estimates of the elasticities of demand for energy, whether in toto (given as -.25), or for the two components.

This completes my description of the empirical basis for the MRG procedures and the premises on which they rest. I want, in passing, to draw attention at this point to the econometric aspects of another study, designed to estimate perceived benefits of air quality improvements from residential property values in areas with different air quality[6]. The study draws on a body of econometric work in which property values are related to various characteristics of the site and the neighborhood, including air quality and other environmental amenities, and the income of the household.

The foregoing examples lead me to some broader remarks on the empirical basis of quantitative economic knowledge in general, not limited to the type of studies we are here mostly concerned with.

In all formal procedures involving statistical testing or estimation, there are explicitly stated but untested hypotheses, often called "maintained" hypotheses by statisticians. In the econometric studies we have here considered, the "premises" already discussed play that role. More in general, any statement resulting from such studies retains the form of an "if ... then ..." statement. The set of "ifs", sometimes called "the model", is crucial to the meaning of the "thens", usually but somewhat inaccurately called the "findings". For instance, in fitting demand relations, the principal maintained hypotheses specify the variables entering into these relations, and possibly other variables with which these variables are in turn linked in other pertinent relations.

The "if ... then ..." statements are similar to those in the formal sciences. They read like logical or mathematical reasoning in the case of economic theory, and like applications of statistical methods in the case of econometric estimation or testing. The heart of substantive economics is what can be learned about the validity of the "ifs" themselves, including the "premises" discussed above. "Thens" contradicted by observation call, as time goes on, for modification of the list of "ifs" used. Absence of such contradiction gradually conveys survivor status to the "ifs" in question. So I do think a certain record of non-contradiction gradually becomes one of tentative confirmation. But the process of confirmation is slow and diffuse.

For some purposes, and at considerable expense, short cuts can be made to diminish the dependence on untested "ifs". I am speaking of systematic experiments such as the so-called negative income tax experiment conducted in New Jersey over the period 1968-1972, and followed by similar income maintenance experiments in other areas of the U.S. If one wants to know whether income maintenance payments to families near the poverty line have a disincentive effect, or no effect, or even a positive incentive effect on labor supply, one does not need to have a pretested theory as to what, if anything, the family is maximizing. Alternatively, one can make such payments to a sample of families and compare its behavior with that of an unaided control group. This is what the New Jersey experiment did. In one category of families where the numbers spoke rather clearly — white husband-and-wife whole families — the effect on labor supply was found to be negative, moderate but statistically significant, and with the effect on the husband's labor supply smaller than that on the wife's. In addition, much was learned about the design, conduct and evaluation of such experiments for use in later studies.

There have not been many such experiments on a scale needed to obtain statistically significant outcomes. Moreover, they have been limited to questions of great and urgent policy importance. Meanwhile, we do need to find ways in which verification of the premises of economics, through cumulative econometric analyses and through experiments that find a sponsor, can be pursued.

I have not found in the literature a persuasive account of how such confirmation of premises can be perceived and documented. How do we keep track of the contradictions and confirmations? How do we keep the score of surviving hypotheses? And what are we doing in those directions? The same questions have been raised before, among others by my predecessors, Wassily Leontief and Aaron Gordon, and good and bad examples of concern and unconcern were referred to by both of them.

Meanwhile, unresolved issues, sometimes important ones from the policy point of view, and mostly quantitative ones, drag on and remain unresolved. Do they have to?

With one exception I am aware of, even our best college-level introductory texts of economics do not press these questions. They teach good reasoning, and describe the views of leading minds and schools of thought, present and past, in the field. Texts in econometrics teach with great care how to test assumptions and to estimate parameters, duly emphasizing the crucial role of the models. What is also needed is to teach the tested and confirmed statements.

VIII. Aphorisms on Interactions

After all I have said about the need for empirical validation, I owe you a brief report on my own casual-empirical sample study of the difficulties of interaction between scientists, engineers and economists, as seen by participants in joint studies. Rather than classifying and tabulating the views expressed, I shall let the respondents speak for themselves. The following is a selection (by me) of statements, drawn from my notes, that carried the most punch.

A physical scientist:

> "Economists are technological radicals. They assume everything can be done."

A geologist:

> "Economists have been too enthusiastic about deep sea mining. They think there is more than there is, that it is easier to get up than it is, and easier to process than it is."

A development economist:

> "Scientists think big. Economists are marginalists. Scientists don't think in terms of opportunity cost."

An engineer:

> "Economics is not dismal but incomplete. The things missed are very important."
>
> [*The reference is to the need to fit environmental protection into economic analysis, T.C.K.*]

A life scientist:

> "Market imperfection is more widespread than economists care to admit."

An economist:

> "Where economists see the invisible hand guiding the market place to produce pretty good outcomes, scientists see only chaos."

An engineer:

> "The economic motive is overrated."

A psychologist:

> "All the conclusions that are drawn from the assumption of rationality can also be drawn from assumptions of adaptive behavior."

A life scientist:

> "Economists have great skill in handling data. However, they tend to ask only for data, not for concepts and ideas. Drawing up a model is an interdisciplinary task."

An engineer:

> "Economists often use smooth production functions even when engineers might be reluctant to do so."

A life scientist:

> "Many scientists do not understand discounting."

An engineer:

> "Economics is the Thermodynamics of the Social Sciences. Everything is deduced from a few simple postulates without the necessity for knowing detailed mechanisms."

IX. Final Remarks

After this instructive intermezzo, allow me a few final words. I will not be able to match the brevity and incisiveness we just savored. However, I do look on the collaboration of the diverse professions involved in the newly discovered joint problems as an important development. To economists it is a new challenge and a new frontier. Among the problems themselves are some of great importance, nationally and internationally. They deserve the best effort and talent that can be brought to bear, within and across the disciplines.

An important talent requiring cultivation is skill in communication between disciplines. We should begin with the defusing of jargon.

Perhaps some terms should be explained at first use. To the physicist who has used calculus on problems going back to Isaac Newton, it is unexpected to learn that everything called "marginal" is a first derivative of something. It appears natural to him, however, to learn that an "elasticity" is the dimensionless slope of a curve plotted on double-log paper. There is more trouble lying in wait with "externalities", an institutional concept presupposing private property, or at least an accountability for private or public production or household decisions that is dispersed over individuals and organizations. If we will be more forthcoming with explanations of our cherished terms, our science colleagues may be more inclined to help us out with "entropy", which to me is a more difficult concept than anything economics has to offer.

A more serious problem is that, while our universities are the principal training ground for future scientists of all kinds, they do not seem to be the best place for gaining experience in interdisciplinary interaction. I believe that the root of the difficulty lies in the procedures for academic appointment and promotion. The initiative, the decisive first step, is usually taken in the department of one's own discipline. Young faculty members must prove their worth first to their senior colleagues in the field they are identified with. A joint appointment holds somewhat less promise as a stepping stone to tenure. Even our graduate students are already aware of these factors.

The increasing demand for the contributions of interdisciplinarians may gradually break the barriers down. Progress will be slow unless university faculties and administrations perceive the problem. Once they do, the irrepressible curiosity and venturesomeness of our undergraduates will provide a point at which to start and from which to build up.

REFERENCES

[1] *Helium: A Public Policy Problem,* National Research Council, 1978.

[2] L. A. Hall, *Survey of Electrical Resistivity Measurements on 16 Pure Metals in the Temperature Range 0 to 273°K,* National Bureau of Standards, Technical Note 365, February 1968, esp. p. 20.

[3] E. B. Forsyth, Ed., *Underground Power Transmission by Superconducting Cable,* Brookhaven National Laboratory 50325, March 1972, esp. Ch. IV.

[4] *Energy Modeling for an Uncertain Future,* National Academy of Sciences, 1978

[5] *A Time to Choose,* Report by the Energy Policy Project of the Ford Foundation, Ballinger, Cambridge, Mass., 1974, App. F.

[6] *The Costs and Benefits of Automobile Emission Controls,* September, 1974, U. S. Government Printing Office, Chapter IV.