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**THE POVERTY OF EFFICIENCY:
SEARCHING FOR A THEORY OF POLICY ANALYSIS**

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May 1989

No. 304

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Kenneth Boulding's recent talk to us was entitled "Economics as an Institution." I suspect that some may have missed the meaning implied by his title and so I will start by clarifying that point. Institutions are the collectively acknowledged rules, conventions, and norms of behavior within a society; institutions shape beliefs and behaviors, and likewise beliefs and behaviors will ultimately shape institutions. Therefore, when Boulding addressed us on the topic of "economics as an institution" he was talking to us about the rules, and norms, and conventions that define the beliefs and behaviors of economists as members of a profession. Members of a profession hold certain beliefs about acceptable behavior, with those beliefs being reinforced by institutional arrangements--either written rules or understood norms of behavior. The very idea of a scientific discipline is a collection of individuals holding somewhat similar beliefs about the acquisition of knowledge. Boulding was therefore talking of the disciplinary beliefs of economists, and the practice of economics as informed--and indeed constrained--by those beliefs.

Lecture presented in the Kenneth Parsons' Lecture Series,
Department of Agricultural Economics, University of Wisconsin, Madison,
April 19, 1989.

Tonight I want to speak to you about a particular set of shared beliefs among economists, beliefs that infer norms of proper behavior if an economist is to be regarded as an objective scientist when he or she engages in policy analysis. Economists, in the interest of preserving disciplinary coherence, will often be heard to declare that a particular piece of work is "not economics," or it is "not objective," or it is "not science." This evening I want to address the logical foundations of the shared belief that gives rise to such statements regarding objectivity and scientific behavior within economics. I will be considering, indirectly, the doctrine of scientism which holds that the methods of the natural sciences are appropriate for all pursuits of knowledge and ultimate truth, including those in the social sciences and the humanities. Economists are particularly taken by scientism and it seems fitting to discuss--in a lecture honoring Kenneth Parsons directly, and John R. Commons indirectly--the contemporary significance of this particular doctrinal devotion. My comments will have a direct bearing on the behavior of economists as we engage in policy analysis, an activity central to this department's past, and to its future.

My comments this evening should not be taken to represent an institutionalist attack on the belief system of orthodoxy. On the contrary, many of the points I will make have been made before by mainstream theorists such as Robert Dorfman, E. J. Mishan, Paul Samuelson and Amartya Sen. My purpose is to seek a logically consistent and theoretically defensible role for economics in policy analysis. I can do no better than

to quote Commons himself. In the very early stages of his Institutional Economics, Commons notes,

The problem now is not to create a different kind of economics-- 'institutional' economics--divorced from preceding schools, but how to give to collective action, in all its varieties, its due place throughout economic theory. In my opinion this collective control of individual transactions is the contribution of institutional economics to the whole of a rounded-out theory of Political Economy. [Commons, 1934, pp. 5-6]

My remarks will address mistaken beliefs about scientism, the confused distinction between positive and normative, and most importantly the insupportable belief in economic efficiency as a norm--as a decision rule--for policy analysis.

Many economists trained since the 1950s have acquired a particular set of beliefs that suggest it is difficult to be an objective scientist and also to participate in public policy analysis beyond simple declarations regarding efficiency. I will argue that these beliefs are based on false impressions of what constitutes objective science. That is, I will challenge the belief of the policy oriented economist who imagines that he or she is immunized against charges of normative or value-laden behavior by following a strategy of restricting policy advice to questions of economic efficiency, claiming that all other implications--including distributional matters--are beyond the scientific competence of the economist. I will maintain that these beliefs arise from a particularly narrow definition of economics and appropriate economic methods, and from a disregard for known theoretical arguments exposing the inherent fallacies implied by such beliefs. I will close with a few observations on contemporary concerns for the collective control of new technology, and for policy analysis in a world in which each passing day seems to bring news of yet another threat to human health.

Our starting point must be with the concept of policy analysis and the economist's role therein. Policy analysis got its start, in a general sense, with the Flood Control Act of 1933--as amended in 1936--in which it is stated that the government would undertake public works on rivers and harbors if "the benefits to whomsoever they may accrue are in excess of the estimated costs, and if the lives and social security of people are otherwise adversely affected [Dorfman, 1976, p. 2]." In those days it was not immediately obvious what constituted a benefit, while costs were rather better understood as the necessary expenditures to bring about the planned project. Still there were other costs that needed explication and the language of the legislation left that matter open to the interpretation of various parties.

In addition to creating a new branch of economic analysis--benefit-cost analysis--the legislation also compelled all government agencies to make "explicit estimates of the gains and losses to be expected from their proposals, and to defend the proposals in the light of these estimates [Dorfman, 1976, p. 3]." Thus was policy analysis born. About this same time the development of the field of policy analysis was influenced by three British economists working in London, Cambridge, and Oxford. Lionel Robbins published, in 1932, his famous work An Essay on the Nature and Significance of Economic Science. Seven years later Nicholas Kaldor and John Hicks offered their independent contributions regarding welfare propositions and the prospects for a value-free science of collective choice. These three events, U.S. legislation regarding public works projects, a polemic by a London economist in the thrall of logical positivism, and the Kaldor-Hicks compensation test continue to influence welfare economics--and thus policy analysis.

Lionel Robbins introduced logical positivism into economics, and he then defined economics in terms of that doctrine. Logical positivists believe that there are only two kinds of propositions in a science--those that are true by definition (tautologies), called analytical statements, and those that are empirical propositions (called synthetic statements). Propositions that do not fit these two classes are said to be lacking truth content and hence are value-laden propositions. Since Robbins, scientific economics has restricted itself to deduction based on the two kinds of acceptable propositions--tautologies and synthetic (or empirical) statements having truth content--they can be related to phenomena in the real world. But of course policy analysis cannot function with only tautologies and empirical propositions and so it became useful to emphasize the distinction between ends (or objectives) and means (or instruments). With this, economists could supposedly remain scientific by restricting their analysis to means only. This practice was congenial to the earlier notion--popularized by the elder Keynes--that there is a clear demarcation between statements that are positive, and those that are normative [1917].

To Robbins, the definition of economics was the study of the allocation of scarce means among competing ends, such ends being beyond question to the economist. To remain objective, economists should not choose between different ends (be normative), but must restrict themselves to recommending "means" so as to accomplish given "ends" (be positive). A close reading of Robbins reveals that he used the word "means" to refer to factors of production or financial resources that could be allocated across alternative employments. That is, Robbins envisioned an economics that was very much like the theory of the firm as we now understand that

notion. In the world of policy analysis however, there are few--perhaps no--policies (institutional arrangements) that can be assumed to be purely neutral means without intrinsic value of their own [Hutchison, 1964].

The means-ends distinction is further tied up with the idea of interpersonal utility comparisons; to ponder and to discuss ends the economist must make comparisons across individuals, while to discuss means is thought to be ethically neutral. Prior to Robbins and the ordinalists, the old welfare economics made use of the idea of social utility as a summation of individual utilities, in a sense intended as a way to discuss the general well being of the community via something called "material welfare." On this view, utility was an individual concept, while welfare was an aggregate concept. Utility, to these economists meant usefulness--rather like the current dictionary definition [Cooter and Rappoport, 1984]. It was Jevons who transformed the term utility to become synonymous with "desires" or "preferences," an idea that Pareto had referred to as "ophelimity." Utility was not subjective, but ophelimity was. Once the term "utility" took over both meanings--usefulness and desires or preferences--its practical content diminished. When the old welfare economists--Pigou and Marshall--thought of interpersonal comparisons of utility they thought in terms of the general well being of people, and the usefulness of policies to address their problems. Public programs for the homeless certainly had utility in that they were useful to the needs of the homeless or the ill-housed. But to ponder the desires of people for housing programs introduced a complication. Hence, in comparing the utility of alternative social states, the old welfare economists could be concerned with the general usefulness of alternative social states for accomplishing certain social objectives.

But Robbins had crafted a new concept of economics that continues to dominate. We do not--we cannot as scientists--discuss ends. If we are to remain objective and positive, we can only advise policy makers how to achieve the efficient level of hopelessness, of unemployment, of nuclear accidents, of chemical-tainted food, and of pollution. And of course there is the further belief that efficiency is synonymous with social optimality. To those so confused, an efficient policy is also socially optimal implying yet a further leap of faith--and yet another value judgment under the guise of objectivity. Today, efficiency will usually mean the passage of the potential Pareto improvement criterion--or the Pareto test as I will refer to it. The Pareto test is said to be met if the gainers from a change are potentially able to compensate the losers out of their realized gains and still have some retained net surplus. Notice that the compensation need not actually occur; it is only necessary that it be capable of occurring.

The Pareto test of Kaldor and Hicks revived welfare economics in its "new" version via the expedient of a consumer theory based on preferences and the concept of indifference rather than on utility of the old kind. Kaldor's method was to separate production from distribution, a task that Pigou could not accomplish because of his utilitarianism. To Kaldor this seemed to avoid the problem of interpersonal comparisons of utility since production dealt only with outputs per unit of input, and every economist knows that people prefer more to less; it therefore seemed safe to suggest that no value judgments were implied.

This transition to total output, being consistent with Robbins' increasingly accepted concept of the boundaries of economics, tended to reinforce the idea that economics was not about increasing satisfaction of

the citizenry directly, but rather economics was about increasing the production of goods and services which--when consumed--gave satisfaction. Economics ceased to be about people and their relationships to one another as it had been before--and as it always was to Commons--and began to be about commodities. Economics came to be about the production of commodities (or about efficiency), and the welfare that commodities could impart in consumption. The distribution of income which determined one's ability to acquire commodities, and so the relative welfare of members of society from those commodities--or from other sources--may be of concern to the political scientist and the sociologist, but the serious objective economist had nothing to contribute here.

Kaldor was thus able to argue that it was "quite impossible to decide on economic grounds what particular pattern of income distribution maximizes social welfare" [Backhouse, 1985, p. 302]. Only later would it be realized that one did not know--indeed one could not know--the value of production without knowing the distribution of income and the associated price vector that provided the weights to the physical quantities being produced. That is, the new welfare economics showed the value of an unambiguous Pareto optimum, but in the absence of old-fashioned utilitarianism, economists were unable to say exactly what it was that had been optimized at the Pareto optimum point [Backhouse, 1985]. Samuelson soon showed that we cannot even be certain that group A is better off than group B even if A has collectively more of every-thing [1950]. It was beginning to seem that the very essence of economics--that more is preferred to less--was being challenged.

Stripped of the ability to comment--as scientists--on the usefulness of different policies for different segments of society, economists retreated further by claiming that we could not say anything about a policy that would reduce the income of the rich by, say, two percent while using those funds to provide housing for the homeless. Since we could not make interpersonal comparisons of utility, the inability to be certain whether the rich lost more than was gained by the poor removed us from the policy dialogue altogether. Notice that the inability to be absolutely certain about the balance of individual preferences in this policy matter justified a retreat from the debate. The desire for perfection in predicting utility gains and losses had become an impediment to meaningful participation in the policy arena. One could always ask about willingness to pay and to accept compensation, but these measures are dominated by income distributional considerations about which economists must remain silent.

This transition from "positivism" as a behavioral norm for scientific activity--to efficiency as an objective truth rule--was aided by those searching for a "value-free" way to participate in just this type of policy debate. Friedman's confused writings on "positivism" were instrumental in furthering this transition [1953].¹ For awhile it seemed as if the potential compensation test of Kaldor and Hicks, would offer an escape from the pessimism about policy advanced by Robbins. But first Scitovsky, and later Samuelson would show that it was not to be. Finally Kenneth Arrow offered his own unique contribution to the evolution of economic thought and policy analysis by proving that there was no possible mechanism that would allow economists to aggregate over individual choices to arrive at consistent and coherent collective choices. In the very first sentence of his famous

book, Arrow posed the false dichotomy that continues to influence economists. He said, "In a capitalist democracy there are essentially two methods by which social choices can be made: voting, typically used to make "political" decisions, and the market mechanism, typically used to make "economic" decisions [Arrow, 1951, p. 1]." In the remainder of his book he proceeded to prove that to rely on voting would lead to inconsistent choices.

To say that social choices can be made in the political arena by voting, or in the economic arena by markets, gives a rather new and curious meaning to the term "social choices." It would seem that when we talk of social choices we have in mind rather conscious and explicit collective acts of choosing particular courses of action. For instance, a social choice is whether to provide for the homeless or to let them fend for themselves, or it is to decide if certain toxic compounds shall be permitted in our groundwater, or whether to build a nuclear submarine fleet, or whether to subsidize agricultural producers. But to say that the market will make decisions about whether to provide for the homeless, or to subsidize agricultural producers is nonsense. The market does not make social choices--rather it reflects the outcomes of millions of individual choices. Arrow cast the argument in a specious manner and then concluded that voting would lead to inconsistent results. The message was clear--markets are the only way that consistent choices can be made. The essence of competitive markets is atomistic behavior leading to economic efficiency, and therefore analysis that focuses on changes in economic efficiency is objective science.

Policy analysis had come to a troublesome pass. The new welfare economics--after seeming to promise so much--had reached the conclusion that it was not possible to say unambiguously that a new policy was better or worse than the status quo. And Arrow's nihilistic conclusion was yet another blow to the idea that economics could be a policy science. Since it was impossible on utility grounds to know what should be done, and since simple voting would produce inconsistent results, there was only the market to rely upon. Just short of two centuries after Adam Smith's intuitive celebration of the invisible hand, his ideas were confirmed by the best minds in the profession. While no one could say that the market was the best of all possible worlds, future Nobel Prize winners were proving that it was at least better than meddling bureaucrats. Markets at least produced efficient results, even if the proof was a tautology. And, because efficiency was related to production, and because production could be weighted by market prices, efficiency became synonymous with objective analysis, while equity became synonymous with distributional considerations.

This then brought us the metaphor of market failure; in order to justify any collective action at all it was first necessary to prove the existence of market failure. If things were going badly in the eyes of the citizenry--crime, the homeless, teenage pregnancy, wage and job discrimination against women, drugs, pollution--the economist would first search for evidence of market failures. If none were found then what existed must be efficient and--by false extension--socially optimal. The government had no business interfering with the magic of the market. Dr. Pangloss was alive and well not only in Chicago, but in lots of economics departments--and in departments of agricultural economics, for that matter.

But of course there were still the inconvenient problems mentioned above, and the persistent tendency of politicians to want to impose inefficient programs and regulations in the belief that they might actually improve the situation. In the absence of an objective truth rule about such interventions, it was thought that economists could do worse than to counsel efficiency. By the 1960s benefit-cost analysis--as applied welfare economics--was in full flower, yet there was, apparently, some concern that the message required reemphasis. Arnold Harberger, in a self-admitted "tract," felt compelled to embolden the timid, and to reassure the true believers. Fearing that policy-oriented economists were being seriously tentative and were wavering in their commitment to efficiency, Harberger offered the "Three Basic Postulates for Applied Welfare Economics" [1971]. There he noted, with some concern:

In an era when literally thousands of studies involving cost-benefit analysis or other types of applied welfare economics are underway at any given moment, the need for an accepted set of professional standards for this type of study should be obvious . . . while the highway engineer can apply professional standards to such characteristics as thickness of base, load-carrying capacity, drainage characteristics, and the like, characteristics such as scenic beauty are beyond their competence as professional engineers. In the same way, any program or project that is subjected to applied-welfare-economic analysis is likely to have characteristics upon which the economist as such is not professionally qualified to check the opinion of another. These elements--which surely include the income distributional . . . aspects of any project or program, and probably its natural-beauty aspects as well--may be exceedingly important . . . but they are not a part of that package of expertise that distinguishes the professional economist from the rest of humanity. And that is why we cannot expect to reach a professional consensus concerning them . . . economists should probably participate more rather than less in the public discussion of such matters, but hopefully in a context that recognizes the extra-professional nature of their intervention. [Harberger, 1972, pp. 3-4]

Had Harberger thought a little more about this logic he would have seen the obvious fallacy. It is clear that landscape design has little to do with the proper engineering of a road. But Harberger knows enough welfare economics to understand that the economist cannot separate the way in which income is distributed from the efficiency implications via the potential Pareto improvement test. He introduced a red herring when he equated the distribution of income with the particular plants along a new highway. To argue that income distribution to the economist is like shrubbery to the highway engineer is--to be rather blunt--silly. Notice that Harberger likens the economist to the engineer--a technician checking the drainage, the quality of the base, and so on. Just as the engineer has no professional skills in landscape design, the economist is said to have no professional skills in income distributional matters. The proper domain for both engineer and economist is where precise performance standards exist, and where consensus might be forthcoming. Building a proper road is good science about which all engineers can agree; landscaping is for others to worry about. According to Harberger, counselling efficiency is good science about which all economists can agree; income distribution--for which a clear consensus is lacking--is for others to worry about. Several years later Harberger attempted to apply the idea of different distributional weights to analyze investment projects and to determine an optimal tax structure [1978]. This was done, we are told, out of its appeal "to those nurtured in the grand tradition of economics." Upon concluding this effort with less than satisfactory results, Harberger argues that:

In the end, then, we cannot condemn as crass or unfeeling the idea [of] our profession's possibly moving toward a "consensus" based on the traditional criterion of efficiency. On the contrary, such a result might well reflect a greater and more

sensitive understanding of the value systems of our citizens and our societies, as well as a more modest and realistic appreciation of our own professional role. [Harberger, 1978, p. S119]

To suggest a social consensus for efficiency analysis on the basis of his unsuccessful attempt to discover proper distributional weights for certain public programs may charitably be thought of as an instance of the "wish fathering the thought."

Many economist will insist that it is not a value judgment to assume that income is properly distributed and that therefore they can ignore it in their efficiency analysis. The rationale for this position is as follows: the current distribution of income must be the appropriate one for otherwise the politicians would change it.² This convenient declaration of faith in politicians is the only time that an economist will admit to any confidence in the outcome of the political process. On all other matters politicians are said to cater to the special pleadings of all manner of ne'er-do-wells. Why, in this isolated instance, do we suddenly regard politicians to have made the correct choice? I suggest that the answer is found in the fact that it manifestly serves our very special interests to make that assumption. More specifically, the assumption then allows us to proceed with the delusion that we are being objective analysts within the context of our model and acting consistent with prevailing social preferences.

While this confusion is unfortunate among economists, I regard its perpetuation and transmission to students of economics as far more serious. I am aware of few other scientific endeavors (or disciplines) that seem to hold so much appeal for the uninitiated, yet deny them as professionals the opportunity to indulge those very interests that attracted them to the

field in the first instance. I know of few students of economics who were attracted to the discipline by the appeal of a life spent proving the efficiency of thoroughgoing competitive equilibrium. Nor can there be very many among us whose call to economics came in the form of compelling devotion to the contemplation of fixed point theorems, bordered Hessians, Cournot-Nash equilibria, fuzzy sets, or quasiconvexity. While there are a few so motivated--and let us be grateful for them--the vast majority of practicing economists were attracted to our discipline by other considerations. Simply put, we thought that economics held important insights for the way in which the world was organized, and for the way in which it might otherwise be organized.

We somehow imagined that economics had something to say about the organization and performance of a nation's ability to provide a particular standard of living, about the choices between a clean environment and one that threatens human survival, about the contrast of Mercedes-Benz's and rickshaws in the streets of Bombay, about farmers in Africa plowing with crooked sticks and broken-down bullocks, and about American farmers--claiming to believe in free enterprise--cashing their government checks. These are the issues, I suspect, which attracted the great bulk of us to this science of choice, only to be told by the self-appointed guardians of truth that we have nothing to contribute to these issues as scientists other than to pronounce on what would be efficient. We may talk, as scientists, about the efficient number of homeless people on America's streets, about the efficient level of environmental destruction, and about the efficient number of boys and men pulling rickshaws in Bombay as they dodge chauffeur-driven Mercedes-Benz's. Let us not, however, as econo-

mists, denounce the existence in America of half a million homeless individuals. Moreover, as economists, we may not despair of environmental degradation, the particular output mix between Mercedes-Benz's and rickshaws, nor do we have any comment on American soybean producers who fear for their world market if African farmers should become competitive in soybeans.

My position, by now, ought to be clear. Policy analysis in economics, if it is to be meaningful, must be redefined so as to confront those who believe that efficiency as indicated by the potential Pareto improvement criterion is an indication of good policy advice, and it must be liberated from the equally false notion that to counsel efficiency is to behave as an objective scientist. Economics is still plagued by the idea that any body of doctrine which can claim--legitimately or otherwise--to be scientific automatically acquires respectability and authority. Since some of the conclusions of economics are unpalatable to many, we are often under extreme pressure to claim our value neutrality and to emphasize the inevitable nature of economics in that certain conclusions follow ineluctability from widely regarded premisses. Mark Blaug, a reasonable enough historian of economic methodology, argues that:

The concept of Pareto optimality and the associated concept of . . . [Potential Pareto Improvements] . . . should not be confused with theorems of positive economics. If this implies that economists must give up the notion that there are purely technical, value-free efficiency arguments for certain economic changes, and indeed that the very terms "efficient" and "inefficient" are terms of normative and not positive economics, so much the better; immense confusion has been sown by the pretense that we can pronounce "scientifically" on matters of "efficiency" without committing ourselves to any value judgments. [Blaug, 1980, pp. 147-8]

When the individual economist acknowledges the overwhelming burden of the evidence, he or she still faces an awkward problem of the shared belief system in the profession. There remains the persistent belief that adherence to efficiency, variously defined, constitutes the necessary condition for an objective and value-free approach to policy science. This means, among other things, that policy analysis that is not strictly efficiency driven runs the risk of being regarded as unworthy of serious notice by other economists. However, it seems safe to insist that Congress, when it first called for an assessment of the benefits and costs of public works projects, had more in mind than an analysis of potential Pareto improvements. Policy analysis has been distorted by those who imagined that welfare economics could bring a satisfactory reductionist decision rule to something as complex as collective action. Dorfman argues that the history of benefit-cost analysis demonstrates the futility of a simple economic criterion for guiding political choice [Dorfman, 1976]. If benefit-cost analysis is no broader than the Pareto test then, in the interest of intellectual honesty, we should refer to it as potential Pareto improvement analysis. Otherwise, the term "benefit-cost analysis" is an elaborate pun.

The identification of benefit-cost analysis with Pareto improvements has come despite overwhelming evidence from within neoclassical economics of the logical fallacies. These theoretical problems are thought to be minor in comparison with the loss of scientific objectivity should the Pareto test be abandoned. However, that argument too is false. It is a value judgment to claim that economic efficiency ought to be the decision rule for collective action. Just as Arrow showed that a consistent set of rankings for alternative social states was impossible starting from

individual valuations, so it follows that there can be no individually-based consensus for economic efficiency as a decision rule [Mishan, 1980]. In the absence of this consensus, efficiency via the Pareto test is advocated by economists without support from the collective unit onto which it is being imposed. It is, therefore, a value judgment made by economists under the false belief that they are, thereby, acting as objective scientists.

In Dorfman's terminology, benefit-cost analysis has evolved as an effort to impose an economic approach onto a political problem. Economists who persevered in this endeavor overlooked the logical inconsistencies in welfare economics. This position apparently being justified on the grounds that a little economic discipline--even if insupportable on theoretical grounds, and therefore bogus--was better than a political process left to its own devices. Bad economics was offered up as being superior to politics. While the intellectual arrogance is not surprising, it is more than a little startling in view of the large number of social phenomena that economists are said to be unable--as objective scientists--to comment on.

What then is to become of policy analysis if it is freed from the fallacy of economic efficiency by means of the Pareto test? We must start by understanding that the term "analysis" does not mean that the economist must produce an objective truth rule whereby good decisions can be differentiated from bad ones. Analysis means to elaborate and to study the different parts of something--in this instance a proposed institutional change. To analyze something is not to reduce all of its components to dollar values or to changes in net national income. To analyze a proposed

policy is to attempt to understand who the gainers and losers are, how they regard their new situation in their own terms, and what this means for the full array of beneficial and harmful effects. If a food safety issue is under consideration the last thing that ought to be done is to determine the willingness to pay for certain levels of risk--or the compensation demanded to be free of certain levels of risk--and then to decide the issue by balancing these suspect dollar amounts. The economist ought to elucidate the full array of impacts arising from different risk environments and let the political process determine--on the basis of economic analysis and other input--what will be done about Alar, atrazine, or aldicarb. Economic analysis can be informative in that choice, but it cannot expect to drive the choice. In the domain of biotechnology it would seem that the Pareto test is the last place one ought to look for guidance on particular issues. Even the more simplistic efficiency analysis--in which private benefits and costs are calculated--will be of only limited help in determining public policy toward such products as bovine somatotropin, and agricultural and industrial chemicals.

The immediate response to my position will be that to abandon efficiency is to lose control of the policy process; a position that presumes economics was ever in control of the process. Many economists, in spite of a professed desire to avoid indicating what ought to be done (since it is normative), do not hesitate to suggest the decision rule that ought to be used in differentiating good policies from bad ones. It is to be expected that they will not sacrifice--without a struggle--the high-priest role of passing praise or scorn on policy choices. It will be said that without the discipline of efficiency, the government will enact all

manner of controversial and inefficient policies. But the burden of proof does not lie with those of us who expose the fallacies of the Pareto test; it lies, instead, with those who persist in defending it in the face of overwhelming evidence of the logical contradictions and inherent problems with the approach.

To deny the existence of an objective truth rule in policy analysis is different from denying that individual scientists can operate responsibly as they do policy analysis. The implications of this will vary somewhat, depending upon the nature of activity being pursued. The objectivity of the analyst lies in the extent to which independent investigators can reach similar conclusions. Policy analysis in economics is about discovering the preferences of individuals and groups--and the relevant policy makers--so that chosen policies accord with this preferences. The policy analyst ought to adopt a research program that will maximize the probability that the policy recommendation to result from the exercise corresponds to what the individuals affected by the policy problem want to achieve. Policy analysis is not about economists imposing an objective function on the political choice process.

In policy analysis the economist will function as an objective analyst, but will also be conditionally normative. That is, the economist must first ask (or determine) the goals and objectives of those affected by a policy--an activity that requires the greatest possible level of objectivity. Then we must draw on theory and our practical understanding of economic processes to indicate which alternatives will maximize the chances of attaining those objectives. Objectivity in policy analysis is concerned with independent researchers reaching similar conclusions with respect to

what the target population says it hopes to accomplish. It is not the science--nor the conclusions--that are objective but rather the economist who stands between theory and those who must make a decision with economic content and implications. This critical difference between the objectivity of the scientist and the science has been muddled in much of the literature on research philosophy in economics. Glenn Johnson comments on this unfortunate confusion by noting that:

Two kinds of objectivity can be distinguished - the objectivity of propositions or concepts and objectivity of investigators. A proposition or concept can be regarded as objective in a particular context if it has been subjected to and has not failed tests of coherence, correspondence, and clarity sufficient for the purposes at hand A researcher or investigator can be defined as objective in a particular context if he is willing to subject his statements to tests of coherence, correspondence, and clarity sufficient for the purposes at hand and to abide by the results. [Johnson, 1986, p. 51]

The economist engaged in policy analysis is not an apologist, nor an advocate, for the dictates of economic theory. That is, the objective policy scientist should be the last to denigrate those objectives of the citizenry that do not happen to accord with the economist's view that people should do what is "efficient." After all, if economists are serious about the sanctity and autonomy of the individual then it is inconsistent to disregard the wishes of those affected by collective choice as unscientific and to advocate, instead, the Pareto rule. We cannot logically venerate individual preferences as expressed through volitional choice in markets, but belittle individual preferences as expressed through collective action. The economist as policy analyst is concerned with problem solving and helping to do what is desired by those affected by the particular event under consideration, not with advocating what is said to be right (or good) by the postulates of welfare economics.

Just as with the Pareto rule, the policy process is still end-result oriented [Tribe, 1972]. But the end results pursued are not necessarily (nor restricted to) present-valued net benefits to the exclusion of other results, nor are they necessarily concerned with potential compensation tests. Rather, the end results to be pursued are those defined as important by individuals involved in the process.

In conclusion, let me note that economists adopted logical positivism just as it had been discredited by philosophers of science. The positivist's dream of a clear demarcation between the meaningful and the metaphysical was soon to be regarded as a false dichotomy. The idea of an objective scientist, as opposed to an objective science, however can still be regarded as pertinent to economic theory and economic policy. Economics should require no less than principled adherence to high standards of observation, interpretation, and synthesis. But the persistent belief that economists who advocate efficiency are being objective scientists is simply wrong. If one seriously believes in consumers' sovereignty then it follows that the analyst must become concerned with the goals and objectives of individuals and groups, even when those goals and objectives are expressed in terms other than that of the Pareto test, or of improving the net social dividend as measured in monetary terms.

This concern for objectivity in assessing the relationship between theory and reality will require that more attention be paid to the nature of cost and benefit incidence of the status quo; it is, after all, the bearing of unwanted costs, or the perceived opportunity for individual gain, that animates most individuals in their daily lives. Once freed from the false belief that to worry about cost incidence or the distribution of

income is to abandon the rigors and purity of the detached and objective analyst, economists are then liberated to address the pressing problems of collective action and public policy with renewed interest and with justified intellectual legitimacy. That inquiry into collective action, and the process of helping to decide what is best to do, will necessarily proceed from a clearer understanding of the way in which the status quo magnitude and incidence of costs and benefits is an artifact of the prevailing institutional arrangements. It is these rules and conventions that determine what is a cost, who must bear those costs, and who will gain from an alteration in the institutional arrangements that define individual and group choice sets.

The economist as policy analyst will continue to face a difficult task. It is not always easy to maintain a sharp distinction between policy objectives and policy instruments. To the extent that this distinction seems to offer a safe haven for the policy scientist to choose instruments while avoiding objectives, we may be misled. This distinction presumes that decision makers first choose policy objectives, and only then begin to search for policy instruments to achieve those objectives. Blaug reminds us that decision makers often will start with existing activities and gradually define and formulate objectives in view of experience with policies. That is,

. . . decision makers do not try to get what they want; rather they learn to want by appraising what they get. Means and ends are indissolubly related, and evaluation of past decisions, or technical advice about future decisions, searches in vain for a social preference function that is not there. [Blaug, 1980, p. 151]

The feasible thing for the policy analyst, it would seem, is to become involved in the policy process in a way that will facilitate the dialectic evolution of both policy objectives and policy instruments. In some instances productive efficiency will be the objective, while in other settings economic opportunity will be purposely reallocated. Yet other situations will see conscious efforts to redistribute income. An objective scientist can further the cause of economic rationality given the evolved policy objectives of the collective and the decision makers therein. This neither suggests, nor requires, that false notions of scientific objectivity hamper or delude the economist [Bromley, 1989].

Let me close by returning to my earlier observation about the student of economics who is attracted to our discipline on the presumption that once educated in the jargon and arcane concepts a whole new world of insights and policy relevant opportunities would appear. I still share their optimism. If I did not believe that economics is about something more than conflicting ends and scarce means then I could not--in good conscience--have supervised over 30 Ph.D. dissertations since coming here just 20 years ago next week. When the graduate students responded to Ken Parsons's comments at the very first Hibbard Lecture some five years ago, they were apparently struck by his plea for an economics that did not reduce all choice to dollar values, and that did not squeeze the human spirit out of our analysis. I want to thank Ken for having inspired the students that April day, I want to applaud the wisdom and insight of those students who started this annual lecture series, and I especially want to thank those of you who have kept it going. It is often much easier to start something than to sustain it. I remain optimistic for applied

economics and policy analysis when I see the commitment students have made to this lecture series. And I particularly encourage all of you in your quest for--as Commons put it--a "rounded-out theory of Political Economy." My sub-title this evening is "searching for a theory of policy analysis" and I hope that my comments will give you the confidence to pursue that quest despite the probable charge from some quarters that you are not being an objective economist. Those who claim to be the guardians of truth are simply wrong and should not be allowed to deter our search for a more meaningful role for economics in policy deliberations.

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NOTES

1. See Bromley [1989] and Caldwell [1982] for a discussion of Friedman's position on "positivism."
2. There is a crucial difference between a distribution of income that is merely "appropriate" and one that is "optimal." To say that income is optimally distributed is to suggest that the marginal utility of income across all individuals is equal.