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Cost Benefit Analysis and the Art of Motorcycle Maintenance*

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ABSTRACT

Partially as the result of consumer and environmentalist pressure, proposals for large-scale government and private projects are increasingly coming under the scrutiny of cost-benefit analysis, decision analysis, risk assessment and related approaches. This paper presents a critical overview of such analyses. It discusses (a) their rationale; (b) their acceptability as guides to decision making; (c) the problems such analyses encounter; (d) how they may be misused; and (e) what steps are needed to increase their contribution to society. The discussion is illustrated with a variety of examples, drawn, in particular, from the evaluation of new technologies.

Whatever their flaws, such analyses appear to have a critical role in guiding social decision making. It is important, however, for both the analyst and the nonexpert consumer of such analyses to understand the errors to which they are prone in order to maintain a critical perspective. Indeed, the institutionalization of such criticism is essential.

Additional research is needed to clarify psychological (subjective) aspects of the analytic process in order to (a) reduce the errors and omissions made by analysts and (b) help policy makers and the public understand the results and the assumptions under which they were reached.

Consumer and environmentalist pressure over the last decade has dramatically opened the process of technology regulation in this country to public scrutiny. To some extent, this opening has consisted of bursting through doors that were already ajar. Interested citizens now attend public hearings that fifteen years ago would have drawn only government regulators and industry representatives. Another aspect of the change is the emergence of new forms of technology management, the most

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This paper was stimulated by my participation in "Risk Benefit Methodology and Application," a conference held September 21–26, 1975, in Pacific Grove, California, and chaired by Dr. D. Okrent. Many of the ideas expressed could be attributed either directly or indirectly to a variety of conference participants. I would like to particularly thank Philip Bereano, Joseph Coates, Barbara Combs, Ward Edwards, Lewis Goldberg, Paul Hoffman, Jack Hirshleifer, Robert Kates, Sarah Lichtenstein, Joanne Linnerooth, David Okrent, Leon Rappoport, Peggy Roecker and Paul Slovic for help in stimulating and clarifying my thinking. The title is borrowed in part from *Zen and the Art of Motorcycle Maintenance* by Robert Pirsig (New York: Bantam, 1974) where some related lines of thought are developed.

visible of which are detailed analyses of the anticipated impact of proposed developments. Risk assessment, cost-benefit analysis, decision analysis, and the environmental impact statement are among the generic names of such analyses.[1] Some of the better known examples are the Rasmussen study of nuclear power plant safety (Atomic Energy Commission, 1975), the National Academy of Science study of the impact of supersonic transports (SST's) on the stratosphere (1975a), and the Stanford Research Institute study of the effects of seeding hurricanes to reduce their intensity (Howard et al., 1972). The preparation of such analyses has become a growth industry, as government agencies comply with the National Environmental Policy Act of 1969 requiring impact analyses for all major federal actions significantly affecting the quality of the environment.

These analyses are tools for regulatory openness because they force the parties concerned to make explicit evaluations of the risks and benefits to be expected from technological enterprises. The assumptions on which these analyses are based, and the numbers used to derive summary cost-benefit estimates, must also be open to public scrutiny. The criticism which greeted the initial draft of the Rasmussen report (Atomic Energy Commission, 1974) and the changes made in the final draft are good examples of how the public can challenge these numbers and assumptions and help produce more adequate estimates (Primack, 1975).

Like the technologies they are meant to assess, these analytic techniques have both inherent limitations and potential for misuse. They will increase the accessibility and sensitivity of the regulatory process to the interested public only if that public understands the techniques and their foibles and monitors the way the analyses are performed. In addition, special efforts must be taken to insure that the techniques are used when necessary and their conclusions heeded. To this end, the present article describes some of the goals of cost-benefit analysis, the problems encountered by attempts to perform such analyses, and the ways in which specific analyses may be led astray and produce erroneous results. It ends with some suggestions about how to maximize the social benefits of cost-benefit analysis.

Basic Approach

The rationale of cost-benefit analysis is that when considering a proposed technology, we should assess in advance the costs and benefits to be expected from its implementation, and then adopt it only if the anticipated benefits outweigh the anticipated costs.

The expected cost of a project is determined by (1) enumerating all adverse consequences that might arise from its implementation (e.g., increased occupational hazards); (2) estimating the probability that each will occur; (3) estimating the cost or loss to society should each occur; (4) calculating the expected loss from each possible consequence by multiplying the amount of the loss by the probability that it will be incurred; and (5) computing the expected loss of the entire project by summing the

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[1] Explication of the differences between these approaches is beyond the scope of this paper. The term "cost-benefit analysis" is used here to refer to the broad spectrum of such techniques.

expected losses associated with the various possible consequences. An analogous procedure produces an estimate of the overall expected benefits.[2]

Performing a full-dress analysis assumes, among other things, that (1) all significant consequences can be enumerated in advance; (2) meaningful probability, cost and benefit judgments can be produced; (3) the often disparate costs and benefits can somehow be compared to one another; (4) people really know how they value different consequences today and how they will value them in the future; and (5) what people want, or should want, is to maximize the difference between expected benefits and losses.

Acceptability

A normative decision-making model, such as cost-benefit analysis, is useful only if it is acceptable to those whom it is supposed to guide. At first glance, cost-benefit analysis does not seem to play favorites. Although the decision reached in any specific analysis will depend on whose values are assigned to the various costs and benefits, the technique itself is designed to accommodate anyone's view of what is good and bad for society. If the results of a cost-benefit analysis seem to favor unfairly one group in society over another, the problem would appear to lie not with the technique, but with the way in which it is used.

There do, however, appear to be a number of issues which may render the approach itself, as described above, unacceptable to some members of the public. One is that cost-benefit theory is concerned with the total costs and benefits accruing to society from a project and not with their distribution. For many projects, however, the risks accrue to different people than do the benefits. A mountain village may find itself downstream from a dam constructed to provide electricity for consumers many miles away. Residents of the Hanford, Washington, area sit atop nuclear wastes produced by power plants of many states. Users of aerosol products may be increasing everyone's chance of getting skin cancer for some dubious benefits.

The cost-benefit analyst typically deals with this problem by saying that if a project's benefits outweight its costs, then, in principle, the losers could be compensated by the gainers. Although attractive in theory, such compensation may be exceedingly difficult to carry out in practice. Often it is impossible even to identify the losers, for example when they are members of future generations. Even if identification is possible, the costs or political difficulties involved in making compensatory payments may be prohibitive (Graaf, 1975). Unless adequate payback mechanisms can be guaranteed, people may have little patience for analyses assessing net benefits (Portney, 1973).

Cost-benefit analysis is also mute with regard to the distribution of wealth in society. Therefore, a project designed solely to redistribute a society's resources would, if analyzed, be found to be all costs (those involved in the transfer) and no benefits (since total wealth remains unchanged). This balance would only change if it were

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[2] The interested reader may find more formal discussions of the decision model described here and other related approaches in Bereano et al., (1973), Brown et al. (1974), Coates (1976), Gardiner and Edwards (in press), Howard (1975a), Mishan (1972a), Peskin and Seskin (1973a), and Siebert and Zaidi (1975), as well as in many of the references cited in this paper.

shown that redistribution itself might produce tangible benefits (e.g., reduced crime (Danziger and Wheeler, 1975)), or if equity itself were valued. People dissatisfied with social inequities may also find cost-benefit analysis unacceptable because of its heavy reliance on current market prices (reflecting current economic arrangements) for evaluating costs and benefits.[3]

Another issue is whether people really do strive to maximize expected net benefits in their own decision making. There is a good deal of evidence that the basic cost-benefit model is not an accurate description of how people make decisions in actual practice (Slovic et al., 1976; 1977). One possible explanation is that people try to follow the model but the calculations and evaluations required are too arduous to implement. If this is the case, then cost-benefit analysis might be seen as a formalized procedure designed to help people make the kinds of decisions they cannot reach unassisted. On the other hand, people may be trying to do something quite different than that which is prescribed by the model. For example, perhaps they are most interested in making decisions that are readily explained to themselves and others in common sense terms. It may be easier to live with a good justification (e.g., "That's the way we've always done it") than with the dictates of a complex and perhaps unintuitive model.[4]

Applicability

Assuming that we want a cost-benefit analysis, we must still ask whether it can be performed in any given situation. Specifically, can we do a good enough job of enumerating consequences and estimating probabilities and values to justify the enterprise? The best way to answer this question would seem to be by considering some of the difficulties encountered in making such judgments. We will consider, in order, the enumeration of consequences, the judgment of probability and, finally, the judgment of value or utility.

Enumeration of Consequences

In order to list all possible consequences, analysts must consider not only the performance of individual components in the system they are studying but also interdependencies between those components and the way the system itself interacts with the surrounding human and physical environment. As one indication of the level of complexity that may be encountered, some of the analyses produced in the Rasmussen study were so large that they exceeded the capabilities of the computer program designed for the study—and had to be analyzed by hand (Weatherwax, 1975). To reduce these problems to manageable size and comprehensibility, a sophisticated technology of reliability assessment (Coates, 1974; Green and Bourne, 1972) has been developed in recent years.

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[3] There has been some theoretical work on how to incorporate equity consideration in cost-benefit analyses (e.g., Haveman and Weisbrod, 1973; Hettich, 1976; Hochman and Rodgers, 1969; Mishan, 1972b; Raskin, 1975). This research has, however, had little impact on the way analyses are performed.

[4] Nash et al. (1975) provide further discussion of the moral basis of cost-benefit analysis and of analysts' apparent failure to understand the value-laden assumptions of their craft.

Two key tools in this technology are fault-tree and event-tree analysis. Each uses a tree structure to show the interrelations between the components of the operating system. A typical branch point might have a safety system either operating or not operating in response to an emergency situation. The "safety system fails" branch might lead in turn to a branch point for "plant evacuation alarm sounds or fails to sound." A "pathway to disaster" is a chain of events in which the wrong branch is taken every time—that is, everything goes wrong and there is a major system failure. The probability of such a pathway is computed by considering the probability of each of its constituent failures. The risk associated with a pathway is determined by multiplying its probability of occurrence by the magnitude of the consequences should it occur. Adding the risks associated with each of the different pathways produces an estimate of the riskiness of the entire system.

Event trees start from a particular undesired initiating event (e.g. a break in a pipe or a sudden stoppage of electricity) and project all possible outcomes of that event. Fault trees start with a particular undesired final event (a failure of the system) and work backward to identify the component failures needed for it to have happened. Essentially, the two techniques build the tree from opposite ends.

The major danger in designing a fault or event tree is leaving things out, and thereby underestimating the true risk. The criticisms leveled at the Rasmussen report, one of the most thorough risk assessments done to date, suggest that this danger may be substantial (e.g. Primack, 1975).

Several kinds of pathways seem to be particularly prone to omission. One type is those pathways involving human error or misbehavior. The Rasmussen study concluded that human-initiated events were both the greatest source of danger and the one most poorly understood (Weatherwax, 1975). How can we ever be certain that we have enumerated all of the important and imaginative ways in which we, the people (as opposed to they, the machines), can mess things up? Consider the Browns Ferry fire, in which the world's largest nuclear power plant came close to causing "many casualties and radiation contamination of a large part of Alabama and Tennessee" (Comey, 1975a). The fire was started by a technician checking for an air leak with a candle, in direct violation of standard operating procedures. The fire got out of control, in part because plant personnel were slow to sound alarms and begin the reactor shut-down. Disaster was averted finally when plant personnel "managed to jury-rig pumps normally used to drive control rods into the reactor to pump water (to cool the reactor core) instead" (*Business Week*, 1975a). It is a moot point whether such human error—or ingenuity—can ever be adequately enumerated and quantified for the purpose of accurate risk analysis. As difficult as it may be to quantify human frailty, these risk analysis problems may be simple compared to trying to pin a number on human malice (i.e. sabotage).

A second source of omissions is failure to consider unanticipated changes in the world in which the technology functions (Coates, 1976; Hall, 1975). Risk assessments are always predicated on some assumed constancies in the external environment. These assumptions may, however, prove to be erroneous. For example, nuclear power plant design assumes the availability of back-up electrical power sources should the reactor fail and need to be shut down. It seems unlikely that any reactor fault tree designed before 1965 would have included as a possibility the great blackout of that year. Omissions may also arise from assumptions whose failure to hold is much less surprising than the great blackout, but whose validity was simply never questioned. The continued availability of properly trained personnel is the sort of assumption that a tree's designers might not even realize they are making.

A third kind of omission arises from overconfidence in our scientific and technological knowledge. An assumption of most analyses is that the system has been designed correctly and will work if none of its components fail.[5] To the best of the designers' knowledge, this is always the case. But the knowledge of even the best engineers is limited. Certainly it is not impossible that there are chemical and physical effects yet to be discovered which might threaten a system's operation. For example, despite the extensive study of possible environmental problems that preceded its construction, the Alaska Pipeline venture is now threatened by the sudden and unforeseen retreat of the Columbia Glacier near Valdez Harbor. As the glacier retreats, it discharges large numbers of icebergs, many of them undetectable, in the direction of the shipping lanes for tankers coming for North Slope oil (Carter, 1975b). A remarkably candid acknowledgement of the limits of one branch of scientific knowledge may be found in Weisbecker (1974, p. xv): "The available knowledge concerning environmental requirements of biological communities cannot be used to predict with any precision the ecological effects of a WOSA (snow enhancement by cloud seeding) program."[6]

A fourth type of omission results from failure to see how the system functions as a whole. For example, the rupture of a liquid natural gas storage tank in Cleveland in 1944 resulted in 128 deaths, largely because no one had realized the need for a dike to contain spillage (Katz and West, 1975). The DC–10 failed repeatedly in its initial flights because none of its designers realized that decompression of the cargo compartment would destroy vital parts of the plane's control system running through it (Hohenemser, 1975). Green and Bourne (1972, p. 547) caution us not to forget that systems may well be dysfunctional when needed because they are undergoing routine maintenance and testing or because they have been damaged by the testing process.

Another example of such omissions is provided by a National Academy of Sciences study of the effects of thermonuclear war. The Academy panel decided that the anticipated reduction of the earth's ozone shield would not imperil the survivors' food supply because many crops could survive the increased ultraviolet radiation. The study failed to point out, however, that increased radiation would make it virtually

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[5] For example, with the publication of the draft of the report bearing his name, Professor Rasmussen noted that the possible presence of fundamental design errors in safety systems could not be predicted (Gillette, 1974).

[6] One example of the sort of surprise that may arise in the wake of scientific ignorance is provided by Philip Handler (1973, cited in Green, 1975a). In 1938, on the basis of research into the causes of pellagra, he recommended that nicotinic acid be added to corn. "Pellagra disappeared within two years, in no small part because of the fortification program. It did not occur to me until some time thereafter that I had no idea whether there might be any ill effects" from the fortification. Two years later, he discovered such ill effects in rats fed large doses of nicotinamide, the form in which the vitamin occurs in coenzymes. For a further example, see Hammond and Maugh (1974).

impossible to work in the fields to raise those crops. "How was this overlooked? Because . . . it fell between the chinks of the expert panels. The botanists who considered the effects of ultraviolet radiation on plants didn't think to worry about the workers" (Boffey, 1975, p. 250).

A fifth sort of error, and one that the Rasmussen study group took great pains to avoid, is overlooking what are called "common mode failures." To insure greater safety, many technological systems are built with a great deal of redundancy. Should one crucial part fail, there are others designed either to do the same job or to limit the resulting damage. In a nuclear power plant, for example, there are many pipes carrying coolant to the reactor core. Should one spring a leak, others will take up its load until it can be replaced. If all fail, then the reactor can be shut down by other means. Since the probability of each individual pipe failing is very small, the probability of all failing and the shut-down mechanism failing, thereby creating a major disaster, would seem to be extremely small. This reasoning is valid only if the various components are independent—that is, if what causes one pipe to fail will not automatically cause the others to fail. "Common mode failure" occurs when the independence assumption does not hold. As an example, the discovery that a large set of pipes in several nuclear plants were all made from the same batch of defective steel (*Eugene Register Guard*, 1974), suggests that a situation threatening one pipe in a plant might threaten them all. At Browns Ferry, the same fire that caused the core to overheat also damaged the electrical system needed to shut the plant down. Constructing a tree that considers all such contingencies may be very difficult.

Assessment of Probabilities

Assuming that we have constructed the best tree possible, we still must estimate the probability associated with each of its links. Such estimates are most believable when based on extensive experience. If we have observed a particular piece of machinery do its thing thousands of times, we can normally produce a confident estimate of the likelihood that it will fail next time around. If we are looking at a different, but related, piece of machinery or at the same piece of machinery in a new environment (e.g. under extreme pressure or cold), we would have less confidence in the original estimate. Our confidence would also be reduced if we had never seen the entire piece of machinery in operation but knew a great deal about the reliability of its components. If many of these components themselves were untested, our assessment problems would be greater still. If the machinery depended on its human operators reliably performing complicated operational and maintenance procedures, or if sabotage attempts were a real possibility, we might be quite hesitant about putting much faith in our estimates.

A further complication arises from the fact that the systems whose riskiness we are most eager to assess are those with potentially the most disastrous consequences. Such systems are typically designed to the highest standards of reliability. Unfortunately, the more reliable an element is, the larger the sample of its operation we must have to accurately estimate its failure rate. "This means that proof of low reliability may be relatively easily obtained, but that proof of high reliability may be much more difficult" (Green and Bourne, 1972, p. 533). Thus, while we now know that nuclear power is "pretty safe," whether it is "extremely safe" remains something of a mystery.

To provide valid estimates in lieu of appropriate historical data, the estimators must be experts in both the topic in question and in the making of probability estimates. There is no guarantee that these two forms of expertise go together—that is, that those who understand a system best are able to convert their knowledge into valid probability estimates and to assess the quality of their estimates. In our own work (Lichtenstein and Fischhoff, 1976), we have found that people who know the most about various topics are not consistently the best at expressing the likelihood that they are correct. It is important to know how general this result is. Murphy and Winkler (1974) have found moderate, but systematic biases in the probabilistic predictions of experienced weather forecasters. Performance was improved somewhat with intensive training, although the training appeared not to be readily transferable to new tasks (Winkler, 1975). Training has not been tried with professionals in other fields, nor with people trying to estimate the probabilities of extremely unlikely events, the type that recurs in risk assessments. Indeed, we know little about how, or if, people distinguish between probabilities such as 1 in 100,000 and 1 in 1,000,000. Psychological research is just beginning to show how to accommodate the fact that the way in which probabilities are elicited affects the estimates that are produced (e.g. DuCharme, 1970; Pitz, 1974; Selvidge, 1975; Slovic et al., 1977).

As an example of the sort of problems that may be encountered when making probability estimates, consider estimating the distribution of failure rates for various machine components. This distribution shows what proportion of the components of a particular type will fail once in 1,000 operations (or hours of operation), once in 10,000 operations, once in 100,000, and so on. When extensive historical evidence is not available, there are a variety of judgmental techniques for estimating such distributions. The Rasmussen group used a variant of the "extreme fractiles" method, asking their experts to choose one number so extreme that only 5% of the components would have lower failure rates, and another number so extreme that only 5% would have higher failure rates. If these extreme fractiles are properly estimated and the actual failure rates can be measured, in nine cases out of ten the observed failure rate should fall between the two estimates. Fractiles that are close together indicate that the failure rate for the component being considered can be predicted with great precision. Lichtenstein et al. (i.p.) have reviewed some two dozen experiments testing the appropriateness of people's estimates of extreme fractiles. These experiments, using a variety of problems, a variety of ways of eliciting the extreme fractiles, and a variety of subjects (including stockbrokers, weather forecasters, and Harvard MBA students) consistently found that people's extreme fractiles were much too close together; that is, the true value was much too often either lower than the low fractile or higher than the high fractile. If these results may be generalized to the estimation of fractiles for failure rate distributions, they suggest that the Rasmussen report's experts may have systematically overestimated the precision with which they could estimate failure rates, which may in turn have led them to be overconfident in the precision of the conclusions based on those estimates.

How good are typical risk assessments? Greene and Bourne (1972, p. 551) report that "in a typical example of about 50 different system elements" assessed failure rates were within a factor of four of observed failure rates (i.e. between one-fourth and four times as large) for 96% of the cases, with no systematic tendency to over- or underestimate. Similar results are reported in greater detail by Bourne (1971, 1973), Eames (1966), and Hensley (1968). Whether this degree of accuracy is adequate depends, of course, on the magnitude of the possible consequences involved, the specific components for which the largest errors are incurred, and the way in which errors in the estimation of failure rates for components accumulate to affect the estimated failure rate for the entire system.

Values

The costs and benefits emerging from most technologies are quite a varied lot, measured in units such as dollars, aesthetic value, and freedom to adopt new policies in the future. In order to compare the expected costs and expected benefits associated with a proposed technology—to see if it is worth our while to adopt it—the cost-benefit analyst must find a way to express these different consequences in some common unit. This is most apparent with technologies like nuclear power, automobiles or the storage of liquid natural gas, in which the major expected benefits are measured in dollars, while the major expected losses are measured in reduced life expectancy and increased susceptibility to disease or violent accident. To know whether or not we want these technologies, we must decide how much a human life is worth.

An intuitive response is that there is no way to put a value on a human life. Yet, in a sense, we do it all the time. Whenever we decide not to install fire detection devices in our homes or air bags in our cars, or we let a higher-paying job draw us to a city with a higher crime rate or greater earthquake danger, we are allowing some monetary reward to compensate us for a slight reduction in survival probability. In a sense, we are assigning a value to a slice of our own lives and those of our families. Although these trade-offs are seldom made consciously, for most of us there probably is some explicit gamble with a very high prize for winning and a very low probability of losing on which we would be willing to stake our lives (say, a one in a million chance to lose one's life against a 999,999 in 1,000,000 chance to win $100,000). Howard (1975b) has argued for offering people a series of such gambles in order to determine the value they place on their lives and then using this figure where needed in cost-benefit analyses. Unless we are certain that such hypothetical choices correspond to people's real preferences; that the way in which we pose the gamble will not affect its acceptability; and that people can meaningfully distinguish between probabilities like 1 in 100,000 and 1 in 1,000,000 (or that our conclusions are unaffected by large errors in estimation), this procedure will provide a shaky basis for important decisions.[7]

A related proposal is to look at the values people set on their lives in the implicit

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[7] Zeckhauser (1975) reports that "Jan Acton (1973) prepared and disseminated a questionnaire which attempted to determine how much individuals would pay for a mobile cardiac unit that would decrease the probability that they would die if they had a heart attack. His results suggested that individuals had difficulty responding to the types of questions he posed, though they provided answers that were not obviously unreasonable."

gambles they undertake daily. For example, Thaler and Rosen (1973) have found that an increase in salary of about $200 per year was required to induce men in risky professions to accept an increased annual probability of 0.001 of accidental death. From this, they inferred that the value of life, at the margin, is equivalent to $200,000.

The validity of this approach depends upon the validity of a number of not immediately obvious assumptions upon which it is based: (1) that past preferences are valid indicators of present and future preferences; (2) that people accurately perceive the magnitude of the risks they accept; (3) that people make decisions accurately reflecting their true preferences without being overwhelmed by the complexity of the decision problems and therefore opting for suboptimal solutions; and (4) that the marketplace is responsive to people's desires and provides them with choices that allow them to express their true preferences. As a case study in the tenuousness of these assumptions, consider the problem area of auto safety. Before the publication of Ralph Nader's (1965) *Unsafe at Any Speed*, and to some extent today, most drivers had no idea of how safe their cars were, nor how safely they could be designed, nor what safety would cost, nor how to go about getting the auto makers to provide them with the choices they wanted (see also Fischer and Kerton, 1975).

Another popular approach for setting a value on people's lives is to calculate the "net benefit to society" of having them alive. This figure is derived by subtracting the dollar value of their lifetime consumption from their lifetime earnings. However, as Bishop and Cicchetti (1973) note, "Under this approach extending the lives of the non-working poor, welfare recipients, and retirees is counted as a *cost*, not as a benefit of a health program" (p. 112).[8]

Assessing the value of a human life is not the only problem facing analysts in their quest for a common measure for all costs and benefits. Consider the difficulties of trying to measure the value of a particular landscape, or of the knowledge that a landscape is in its original form (and not reclaimed), or of the preservation of options for future generations, or of reductions in noise level (Bishop and Cicchetti, 1973; Fischer, 1974; Fisher and Krutilla, 1973; Peskin and Seskin, 1973a). Many cost-benefit questions are so complex that even when dollar values can be assigned to different aspects of a project, it may be extremely difficult to compute the project's total value. Hanke and Gutmanis (1973, p. 262) compared two industry-by-industry studies of the costs of water pollution control. Although derived only a year after the first study, the estimates from the second (1973) study showed a mean absolute change per industry of 80%. The apparent source of these differences was the number of manufacturing establishments included and the distribution of their sizes.

How well are analysts able to overcome these difficulties? Tihansky (1973) surveyed 200 studies of the benefits of water pollution controls and found but a handful that he felt were methodologically valid. Hanke and Gutmanis (1973) cited serious shortcomings in estimating the costs of water pollution controls, the "easy" part of cost-benefit analysis for water pollution policy. According to Rowen (1973) many analysts adopt the easiest approach of all for dealing with hard-to-measure costs and benefits: they simply omit them.

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[8] An excellent discussion of this and other "Procedures for valuing lives" may be found in Zeckhauser (1975). Also Hirshleifer et al. (1974); Linnerooth (1975); Zeckhauser and Shepard (1976).

An alternative proposal that has gained considerable support in some quarters is the divide and conquer strategy of multiattribute utility theory (Edwards, 1971; Huber, 1974; Raiffa, 1968; von Winterfeldt and Fisher, 1975). Instead of trying to assign a holistic (dollar) value to a set of objects, followers of this procedure first decide which attributes (or dimensions) of these objects are most important to them and then evaluate each object on each dimension. These judgments are then aggregated by some formal algebraic rule that typically reflects the relative importance of each attribute in order to produce an overall evaluation (or utility) of each object. These "objects" could be a set of houses that one is considering buying—with the attributes of price, location, etc.—or future worlds relying on different energy sources—with the attributes of pollution, interruptibility of power, etc.

Although the multiattribute utility approach does not solve the problem of finding a common denominator for diverse attributes, it does tend to both make the trade-offs more explicit and put attributes that are difficult to express in dollar terms on a more equal footing. It can also help explain apparent inconsistencies in people's preferences. For example, Cohen (1974) has sarcastically noted that although the risks of nuclear power appear to be equivalent to those incurred by being 1/20 of an ounce overweight, people are much more willing to accept the latter risk than the former. Such preferences are necessarily inconsistent only if people evaluate their lives in terms of only one attribute: breathing—not breathing. Consideration of other attributes, like the quality of the life that people are left with and whether they must coexist with an entity they find utterly horrific (nuclear power), might make these preferences seem more reasonable (see also Pahner, 1976).

Societal Gambles

Implementing any new technology is a gamble of sorts, and like other gambles, its attractiveness depends on both the likelihood of winning or losing and how much will be won or lost. Once we have evaluated the risks involved with a proposed technology and the benefits that may arise from it, we must decide if it is worth our while.

Viewing technological innovations as gambles may help explain why the controversies surrounding them often appear to be irresolvable. Even when people agree on the risks and benefits associated with a particular gamble, there are substantial individual differences in general willingness to accept gambles—in "risk aversion" as it is usually called (Brown et al., 1974). We can speculate that one reason why people argue so heatedly about the probabilities and values associated with technological gambles is that were these issues to be resolved, they would have to confront the question of the sort of gambles that society should take. Arguing about how much risk-aversiveness is appropriate for society seems even less amenable to resolution than arguing about the facts of the gambles.

There appears to be a substantial aversion even to acknowledging that we face gambles in our societal decision making. Just prior to hearing a "blue ribbon" panel of scientists report being 95% certain that cyclamates do not cause cancer, Food and Drug Administration Commissioner Alexander M. Schmidt said "I'm looking for a clean bill of health, not a wishy-washy, iffy answer on cyclamates" (*Eugene Register*

Guard, 1976). Recently, Senator Muskie called for "one-armed" scientists, who do not respond "on the one hand, the evidence is so, but on the other hand . . ." when asked about the health effects of pollutants (David, 1975). Analysts must be very careful not to promise the public more certainty than their craft can provide. Such promises can produce an undue increase not only in the public's reliance on experts (and, perhaps, in the temptation for experts to pass opinion off as fact—Kantrowitz, 1975), but also in the belief that an analytic "fix" can be found that will relieve us of the responsibility of facing difficult societal decisions.

According to cost-benefit theory, decisions to accept or reject gambles should depend on those gambles' expected net benefits. Psychologists and economists have studied the gambling behavior of individuals to see if they do, in fact, adhere to that criterion. The evidence is mixed (e.g. Rapoport and Wallsten, 1972). Sometimes people are guided by expected net benefit; at other times they are influenced by other factors, such as the way in which gambles are presented. For example, Lichtenstein and Slovic (1971, 1973) have found that when asked how much they are willing to pay to participate in a gamble, people concentrate on how much they stand to win or lose; when asked which of two gambles they would prefer, they focus on the probability of winning or losing. Working both in psychological laboratories and at the Four Queens Casino in Las Vegas, they found that it is possible to construct pairs of gambles for which people prefer to play one, but are willing to pay more to play the other.

If "simple" casino-type gambles can lead to inconsistent behavior, we must expect even greater difficulties in evaluating technological gambles which are undertaken for society as a whole, often including future generations. Although such gambles are far from novel events (most decisions to go to war, for example, have fallen in this category), we have no clear-cut guidelines for making such decisions in a democratic society (Nash et al., 1975; Zeckhauser, 1975).

Chauncey Starr, a leading proponent of cost-benefit analysis, has suggested that we use the preferences revealed in past decisions to guide future societal gambling (Starr, 1969; Starr et al., 1975). According to this proposal, historical accident and fatality records reveal patterns of acceptable risk-benefit trade-offs. Acceptable risk for a new technology would be that level of safety associated with ongoing activities having similar benefit to society. The validity of this proposal rests on much the same assumptions as using current market values to determine the value of a life, and is subject to the same criticisms.[9]

Criticism and Self-Criticism

It might be tempting for nonexperts to gloat over the difficulties that risk assessors face and the potential flaws in their analyses. One reason why such gloating would be misplaced is that we have a lot at stake in how well the analysts do. A second is that we are, after all, equipped with the same fallible cognitive apparatus that the

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[9] According to the Committee on Principles of Decision Making for Regulating Chemicals in the Environment, "The argument for relying on free markets to allocate resources is based on the assumption that markets reflect individual values; but the very existence of government regulation denies this to the desired degree" (National Academy of Sciences, 1975b, p. 41). It is worth noting that Otway and Cohen (1975) were unable to replicate Starr's empirical results.

analysts have and, thus, probably could do little better in their stead. We have been gambling with people's lives for years. What cost-benefit analysis has done is bring the issues underlying these gambles out in the open so that we can make clearer, more willful choices. To a large extent, once the analysts have done their best, the ball is passed back to us, or our elected and appointed representatives. If we fail to understand the results of these analyses, and we hold some political power for acting on their implications, then all their sophistication may be for nought.

Starr, et al. (1975) suggest a variety of ways in which people's perceptions of risk are likely to differ persistently from those obtained by careful analysis. For example, they believe that the single most important factor in risk perception is risk controllability, an attribute which people have often been found to exaggerate (Vidmar and Crinklaw, 1974). Kates (1962) has found that flood plain residents often have very inaccurate ideas about the likelihood of floods in their area, despite first-hand experience and extensive exposure to media reports of flood prevalence. As a result, they often respond to flood dangers in ways that are not in their best interests (Kunreuther, 1976).

Research is needed to help experts structure problems and assign probabilities. It is also needed to show them how to communicate the results of their analyses to the public (Slovic et al., 1976).

For its part, the public must evaluate both the formal analyses presented to it—to see whether they provide solutions to the problems they address or "merely" articulate clarification of the issues involved—and the quality of its own decision-making skills, and take the steps needed to acquire the skills it lacks. One step forward would be to school ourselves in those aspects of cost-benefit analysis or decision analysis that do not require inordinate amounts of specialized training. H. G. Wells said once that "statistical thinking will one day be as important for good citizenship as the ability to read and write." That day seems to have come. We need these skills to influence intelligently the societal decisions that are being taken on our behalf, and to respond properly to those problems when the decision is our own. At some time in the not too distant future, those of us living in earthquake-prone areas may receive messages like the following: "There is a 50% chance of an earthquake of magnitude between 6.5 and 7.5 along a fault line of 10–50 miles centered approximately 50–100 miles south of town to occur 3 years from now, plus or minus 6 months." Will we know how to respond to the gamble this message implies?

Applications

Understanding the potential and limitations of cost-benefit analysis requires an understanding not only of the basic problems described above, but also of the difficulties that arise in actual practice. The problems tackled by cost-benefit analysis are so varied that no one technique is adequate for handling them all. Cost-benefit methodology provides the analyst with a general approach to technology assessment and a bag of tricks for measuring expected costs and benefits in individual situations. The validity of any given analysis depends on a variety of specific factors such as the messiness of the problem, the skill of the analyst, the way in which the analytic question is posed, the existence of appropriate techniques in the bag of tricks, and the analyst's ability to fashion new ones if the bag is empty.

This section considers problems with several specific analyses, chosen because of my familiarity with them, rather than because they are particularly flawed. They are, in fact, some of the best analyses done to date, performed by the most conscientious of analysts. If they are imperfect, it is only the reflection of the inevitable fallibility of ambitious intellectual enterprises.

Chemical Hazard

In 1974, the Environmental Protection Agency commissioned a research team from Stanford Research Institute (SRI) to develop a general methodology for analyzing the costs and benefits associated with regulating hazardous chemical wastes (Moll, 1975; Moll et al., 1975). The SRI group chose as "exemplary" noxious wastes, asbestos and cadmium, a byproduct of zinc smelting and tire manufacturing. Their procedure was to (1) identify the sources of asbestos and cadmium emissions in the U.S. and their place of initial deposition (air, rivers, solid wastes); (2) characterize currently available emission control technology; (3) estimate the direct costs of installing emission controls on pollution sources; (4) estimate the indirect costs of controls, primarily from loss of world market share due to the increased cost of U.S. products manufactured under tight emission standards; and (5) estimate the benefits in reduced death and illness that would be obtained by controlling emissions.

Performing this analysis required the talents of a multidisciplinary team of experts in engineering, economics, medicine and decision analysis. Assuming the competence of the component analyses, the overall plan seems quite reasonable. The generality of the report appears, however, to be limited by the analysts' policy of considering only presently available technologies and economic institutions. This restriction is in keeping both with the SRI group's mandate from the Environmental Protection Agency and with conservative analytic policy—base your calculations on realities, not possibilities. As a result, they may have produced a "worst of all possible worlds" scenario for evaluating the economic impact of pollution controls. It assumes that cheaper, more efficient pollution devices will not be developed, that other countries will not adopt similar controls and increase their own prices, that the U.S. will not restrict the import of goods produced by plants that do not meet U.S. environmental quality standards, that companies will not reduce their profit margins to maintain market share despite increased production costs, and that local areas will show none of the resilience needed to replace the jobs lost due to reduced markets.[10]

Certainly readers of the report are entitled to discover these simplifying assumptions, question their validity, come up with different estimates, and determine the generality of the report's conclusions. The question is, will they? Is any layperson without

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[10] This last assumption appears particularly unreasonable when contrasted with the assumption—used elsewhere in the study—that long-term buildup of cadmium in local residents is relatively small due to Americans' high mobility (i.e. few people will be close to the plant long enough to absorb a lot). This mobility is a reflection of the sort of responsiveness to changing economic conditions that presumably might provoke some response to lost jobs other than resignation. See, also, Hanke and Gutmanis (1973).

training in economics, engineering and medicine capable of realizing what the implicit assumptions are and of working through the results of rejecting them? Can even an interest group working with different assumptions afford to perform such reanalyses very often?

The SRI group's choice of exemplary pollutants suggests another way in which analysts' apparently arbitrary procedural decisions can have sizeable effects on the results of their efforts. Of the two pollutants, asbestos is considerably better known to the general public. While cadmium has actually been judged a somewhat greater hazard (Munn, 1973), it has never stirred the sort of controversy generated by Reserve Mining's dumping of asbestos-laden taconite tailings into Lake Superior (Carter, 1974) or by the high incidence of lung cancer in asbestos-plant workers (CBS, 1975).

Let us consider the possibility that the SRI group had, say, because of limited resources, been able to analyze only one of these two pollutants. At first glance, cadium might appear to be the better choice. According to Moll (1975), just as lawyers have a saying that good cases make bad laws, the task of developing a general methodology for evaluating the expected costs and benefits of pollution controls might be best served by chosing a noncontroversial example. This sounds like a reasonable rule of thumb. However, it is also the case that the public typically shows interest in only the most controversial environmental issues. An analysis of asbestos certainly would elicit careful scrutiny by both Reserve Mining and by its opponents. By its scrutiny, each side would attempt to eliminate erroneous material prejudicial to its position. Scrutiny from both sides is a valuable safeguard, likely to improve the quality of the analysis. If only one side scrutinizes, as seems likely with noncontroversial pollutants, the resulting analysis might be unbalanced.

Nuclear Power

The Rasmussen report in its draft form (Atomic Energy Commission, 1974) was one of the most ambitious and earnest efforts at risk assessment performed to date. Cogent criticisms of its methodology required an impressive marshalling of opposition, most notably by the Union of Concerned Scientists (Kendall and Moglewer, 1974; Kendall, 1975) and the American Physical Society's Study Group on Light Water Reactor Safety (1975). The revision of the Rasmussen report (Atomic Energy Commission, 1975) is largely an attempt to correct the errors found by these critics.

One main criticism was that the study had underestimated the consequences of a serious accident (should it occur), particularly in terms of genetic defects, nonfatal cancer, and groundwater contamination (Primack, 1975). A second was that the probability of failure had been underestimated (Weatherwax, 1975). A third, and perhaps the most discouraging, was that it is impossible to generate estimates of risk with the accuracy claimed by the report (Findlayson, 1975).

These inadequacies resulted from (1) omissions—for example, failure to consider the possibility of sabotage or of procedural violations by plant personnel except under conditions of stress; (2) oversimplifications—for example, the assumption that failure rates are constant throughout the life of a component, whereas many components have substantially higher failure rates at the beginning and end of their service life; (3) use of inappropriate scientific evidence—for example, an evacuation model based on experience with smaller numbers of people in a smaller area and with greater lead-time than is likely to be available in any real crisis;[11] and (4) lack of relevant data—for example, the absence of any full-scale simulation of safety system operation in the event of a loss-of-coolant accident.

It is important to note that these criticisms became apparent only after intensive study of the report by experts from a variety of disciplines. Presumably, a similar effort is needed to review any complex risk assessment. If we are going to rely on risk assessments by experts, we are also going to have to institutionalize their review by other experts (see also Rowen, 1973; Noll, 1976).

It is also important to note that the great attention spent on estimating the risks of nuclear power (three million dollars for the Rasmussen report alone) has not been matched by a like effort to assess the expected benefits of nuclear power. These benefits appear to be viewed as certainties, as if to say, "Of course, we want nuclear power, if only it can be made safe enough." A little reflection reveals substantial uncertainties in the economics of nuclear power. To list but four of the questions whose answers could have dramatic impact on the benefits to be expected: "Will OPEC fall apart, or will the price of oil for some other reason drastically increase or decrease?"; "Will a significant number of Americans substantially reduce their energy consumption?"; "Will there be sufficient capital to finance these enormously expensive plants?" (*Business Week*, 1975b); "Will there be sufficient uranium to keep the plants running?" (Day, 1975). On the other hand, earlier analyses of nuclear power are often faulted for having considered only the benefits and ignoring the risks (see also Dyson, 1975).

Earthquakes

In 1971, the Long Beach City Council commissioned the J. H. Wiggins Co. to analyze several proposed changes in the city's building code, each of which guaranteed different degrees of protection against earthquake damage at some price. As reported by Wiggins (1972, 1973), this project made a remarkable effort to involve the public in the process of preparing the report and to make the final recommendations comprehensible to that public.

Particularly notable was the analysts' realization that people have difficulty understanding very low probabilities in a meaningful and appropriate way. As a result, they used a technique which compared the risks associated with the various possible building codes with those associated with natural hazards. Although this technique might be useful in many situations, difficulties can arise from trying to implement it without a thorough understanding of the cognitive apparatus of the people making the judgments. For example, Hewitt and Burton (1970; cited by Burton et al., in press) had residents of London, Ontario, judge the probability of various natural hazards. Their results showed that while people's perceptions were quite accurate for

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[11] The Browns Ferry experience in which the county civil defense official was not notified until two days after the near disaster seems to cast further doubt on the validity of the model that was used (Comey, 1975a, b).

hurricanes and tornadoes, they typically overestimated the probability of floods; for ice storms, they were split between under- and overestimators. Thus, the natural hazard chosen as a reference risk can seriously bias, in either direction, people's perceptions of the building code risk they are asked to evaluate.

Misuse

All of the problems discussed above must be considered the result of honest mistakes, if mistakes at all. It would be naive, however, to assume that all parts of all analyses are performed to the best of the analysts' abilities. Certainly, it is possible to bias the results of an analysis in a variety of fashions. Some ways are fairly innocent like "a tendency of the analyst to concentrate on those aspects of the problem that are easier to treat" (Committee on Public Engineering Policy, 1972, p. 14) or a tendency of analysts to have too much faith in their product, and therefore to oversell it (Milch, 1976; Strauch, 1975). Other ways are more devious, as when experts submit scientific evidence of low quality or play "numbers games" to convince the public that what it wants is what the analysts want it to want (Boffey, 1976; Green, 1975; Kantrowitz, 1975; Peskin and Seskin, 1973b; Schindler, 1976).[12]

Short of deliberately slanting their results, analysts can mislead the public by presenting information in a form that is unusable. A 17-volume, 9,000-page Department of the Interior study of the impact of an Alaska gas pipeline has been called "a monument to irrelevancy. Nowhere in it can one find a succinct analysis of the choice that must be made" (Carter, 1975a, p. 363; also Carter, 1976).

Presenting information in a usable form may require a fairly deep understanding of the cognitive processes of the intended audience. As a further example of the cognitive problems raised above, consider the importance of analysts informing their readers about the reliability of their estimates. There is, however, abundant evidence (e.g. Gettys et al., 1973; Kahneman and Tversky, 1973) that were such information provided, people would not know how to use it. In particular, people seem to be just as confident making inferences from highly unreliable data as from reliable data, rather than less confident as statistical theory dictates.

Light's (1975) critique of a Department of Health, Education and Welfare report, *Economic Cost of Alcohol Abuse and Alcoholism*, provides a case study of how reliability information may be undervalued and even ignored. The report estimated that the economic cost associated with the misuse of alcohol was $25 billion a year, but hedged this conclusion greatly with references to the tenuousness of some of the assumptions that had to be made to complete the study. In the HEW press conference reporting the study, and in its subsequent citation by public figures like President Ford, this "admirable restraint" was absent, with the $25 billion figure acquiring the status of authoritative fact.[13]

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[12] In the context of studies assessing the safety of systems for transporting and storing liquid natural gas, Fairley (1975) presents an interesting compendium of ways for misinterpreting and misrepresenting accident statistics.

[13] Glenn Schweitzer, Director of the Office of Toxic Substances in the Environmental Protection Agency, (1973) has commented, "too often lawyers and economists seize upon (statistically derived) numerical risk factors forgetting that these experimentally derived estimates may in fact have a very shaky relevance to the real world" (p. 73). See also Lodge (1976).

Further problems may arise with those who commission the analyses and implement their results. The fact that the Atomic Energy Commission could order (in 1964) an updating of its 1957 reactor safety study (WASH–740) and then for nine years sit on the new results (which showed greater dangers than the original study) hardly inspires confidence (Ford and Kendall, 1975; Primack, 1975). Neither does the possibility that firms can avoid complying with pollution standards, determined by cost-benefit analyses, simply by having their effluents tested again and again until by chance one batch does pass the test (Downing and Watson, 1975). Much of the National Academy of Science's (1975a) study of the impact of the SST was devoted to finding a "technological fix" for the problems created by the SST (e.g. new engine designs). In the National Academy's (1975b) guidelines for evaluating the costs of regulating chemicals in the environment little attention is devoted to devising technological, social or legal "fixes" that would reduce the drag on the nation's economy produced by pollution control. As a result, the Academy's panels may have put the best foot forward for the SST and the worst foot forward for pollution control. Rowen (1973) had noted that "dominant alternatives are made, not born; they are usually crafted by designers who have a deep understanding of the relevant production functions, have thought hard about objectives and measures of effectiveness, and are able to shape and modify alternatives until one or more emerge as winners" (p. 368).

The National Academy of Science's study of the SST provides another illustration of the ways in which analyses can be used for political purposes. Shortly after the report was completed, the Department of Transportation produced a summary of the same data which completely neglected the conclusion that for each SST flying, we should expect three to four more cancer deaths per year worldwide, due to reduction in the earth's ozone shield. The Academy's scientists were outraged at this misinterpretation of their work. Since the details of the analysis were part of the public record, they were able to have the misinterpretation rectified—for the time being at least (Carter, 1975c). If we are going to rely on cost-benefit analyses to guide our decisions, we are going to have to be alert to cases of misuse. Peskin and Seskin warn us that "since in most cost-benefit analyses, there is considerable opportunity to make self-serving assumptions, it is fairly easy to doctor the analyses" (1973b, p. 30).

Conclusion

I believe that the benefits of cost-benefit analysis can substantially outweigh the costs. Properly done and used, it can open up the business of technology assessment and regulation to the public. It forces government and industry to consider societal costs and benefits in their planning and to do so in a way that allows the public to criticize their analyses. Those who find technological development and expansion repugnant may find it hard to imagine an ally in anything as technical as these analyses. Yet, it should be noted that even were a no-growth philosophy to win out, technology would still have to be monitored, and this seems to be one of the best ways of doing it. Indeed, in a no-growth society, it would take large quantities of the unpolluting brain power invested in cost-benefit analyses to use best the limited resources with which we would be living.

However, as with any technology or any component of the democratic process, eternal vigilance is needed to make cost-benefit analysis serve its public purpose. Analyses can be subverted both deliberately and inadvertently by those who order them, and by those who interpret them. Only when we see them as a part of the political process can we remain on our guard and see that they are used correctly (Green, 1975b; Majone, 1975).

If cost-benefit analysis is seen as a part of the political process, then its role must be formalized in order to minimize the dangers of misuse. Many observers have described, often in fairly diabolical terms, the collaboration and community of interests between government regulators and the industries they are supposed to regulate (Cramton, 1972; Lazarus and Onek, 1971; Mineral King Valley, 1970; Mitnick and Weiss, 1974; Noll, 1976; of Birds, Bees and the FPC, 1967; Pringle, 1968; Sax, 1970). If these descriptions are true, it is not hard to imagine cost-benefit analyses being quickly adapted to that collaboration, with regulators deriving the figures they need to perform their analyses directly from the regulated industries, with little public input (Keating, 1975).

We need clear rules for what issues merit cost-benefit analyses and how their findings are to be used. For example, a hearing should be granted not only for projects with influential backers, but also for what O'Leary (1975) calls "underpromoted priorities" that may be in the nation's interest. At present, even where environmental impact statements are called for, there is no stipulation that they may be heeded. Without regulations to the contrary, they may be used only when they serve the interests of the politically powerful.

The role of the public in these analyses must also be formalized. Obviously, when analysts assign values to the positive and negative consequences of technologies, these values must reflect the public's best interests—however sticky a notion that is. Perhaps less obviously, the public must be put in a position where it can criticize the technical aspects of the reports. This may require not only public meetings and free circulation of cost-benefit analyses, but also the hiring of public defenders. Such public interest advocates would be entrusted with scrutinizing all reports, not just the few that obtain national attention, from the public's point of view. These public defenders should be paid as well for their criticism as the cost-benefit analysts are paid for producing their reports.[14]

In the meantime the public must defend itself. One of the most effective means of defense in the past has been the recruitment of teams of scientists who in their spare time criticize the analyses produced by their peers. However, it seems unreasonable to count on irregular volunteer troops for the long run, considering the burgeoning number of analyses performed. One possible low-cost solution would be to establish a public-interest clearing house for cost-benefit analyses that would send out each analysis to several scientists for criticism. Because of the subtle and varied nature of the technical errors in and hidden assumptions of cost-benefit

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[14] Related, and somewhat more detailed, proposals for public interest advocates may be found in Carroll (1971), Ege (1971), Lazarus and Onek (1971), Mitnick and Weiss (1974), National Academy of Sciences (1975b) and Petak (1973).

analyses, these review panels would have to have talented and multidisciplinary membership. They should also be alert to areas outside the public eye that merit cost-benefit analyses. A case might even be made that scientists, in addition to their professional responsibility to review manuscripts and research proposals submitted by their peers, have a public responsibility to lend their technical skills to the public for reviewing cost-benefit analyses.[15] Again, it is hard to imagine that all the quality criticism that is needed can be obtained on the cheap. Until a cost-benefit oversight agency is created, funds for remunerating part-time public defenders should be written into every cost-benefit analysis budget.

Viewing cost-benefit analysis as a political instrument imposes a serious burden on the decision analysts. They must not only guarantee the technical correctness of their work, but also the validity of the way in which their research mandate is formulated and the way in which their results are used. They must, in the extreme, be ready to refuse to accept a project when they feel that the research question is loaded. For example, Fay (1975) has identified what he calls the "over-capitalization rip-off", in which an industry gets so committed to a project that the public cannot afford to let it go under. The analyst assigned to study such a project must be ready to show the public where and when the original erroneous decisions were made. While particular overcapitalized projects may have to be sustained, eventually the public will learn to identify such projects before it is too late.

When results they produce are in danger of misinterpretation (as most complicated findings are), analysts must monitor what happens to them once released into the public domain. For example, it might be tempting for polluters to seize upon the chemical hazards study and cry "See how much it costs to eliminate cadmium alone." The SRI group should then be watchful of such pronouncements and ready to remind listeners that the limestone scrubbers used to remove cadmium remove other pollutants as well. In controversial cases, this follow-up may require large amounts of unpaid time. However, it may be effort well spent. In order to maintain public confidence, it may be extremely important for those in the cost-benefit business to police their field voluntarily for inferior workmanship.

Beyond these precautions, analysts should do everything in their power to guarantee that the public is not only not misled but is actually properly informed. This means clarifying their assumptions and the way they get their figures, worrying about dissemination in comprehensible form to the widest possible audience, and making themselves available for public debate.

Perhaps the most important aspect of informing the public is for the analysts themselves to point out the limits of their craft. Although such humility may be painful, it protects the analysts from promising too much and losing credibility whenever their analyses prove flawed. Humility will also protect the public from the feeling that they must surrender responsibility for critical decisions to seemingly infallible experts. A public that recognizes these limits will turn to the analyst not for ironclad

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[15] The potential hazards and benefits associated with experiments in genetic manipulation (Echols, 1975; Garfield, 1975; Wade, 1975) is one example of the sort of problem that scientists could independently identify, analyze and bring to the public's attention.

solutions to problems, but for otherwise unobtainable understanding of their intricacies. If the analysts' best efforts at quantification prove inadequate this would be seen as a sign, not of failure but of the fact that some questions of quality cannot be incorporated into analyses, but must be studied in their own right and combined with the insights produced by cost-benefit studies.

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