POLICIES AS THEORIES

Giandomenico Majone

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FOREWORD

The International Institute for Applied Systems Analysis (IIASA) conducts studies relating to problems in a number of areas of wide concern: energy, food and agriculture, human settlements and services, resources and the environment, and management and technology, as well as the analytic techniques required to address these problems.

To support these activities the Institute also addresses questions relating to the scientific philosophy that should underlie this work, to the crafts involved in addressing real problems, and to how the quality and relevance of such work should be judged.

This paper by Giandomenico Majone, who has been associated with the Institute from its early days, addresses some important philosophical questions relating to systems analysis.

HUGH J. MISER
Head
Survey Project

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Policies as Theories

GIANDOMENICO MAJONE

International Institute for Applied Systems Analysis, Austria

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The received view of the scientific method, as represented for instance by logical positivism, has only historical interest for the specialists, but it is still widely, if implicitly, held by decision and policy analysts. On the other hand, recent developments in philosophy and the history of science, which stress the fallibility of theories and the social and historical character of scientific knowledge and criteria, have not yet been assimilated by analysts. This paper argues that these recent methodological developments offer important insights into many theoretical and professional problems facing students of policy-making. Thus, an appreciation of the craft aspects of scientific inquiry not only clarifies the subtle relationship between theory and practice in any type of systematic analysis, but also suggests a conceptual model of the analyst's task that is quite different from the conventional decision-making paradigm. Again, Popperian and post-Popperian views of the evolution of knowledge are shown to be relevant to the evaluation of policies and to the study of their development. Particularly important in this respect is the notion, due to Lakatos, of problem shifts in competing research programmes. Even the role of advocacy in policy arguments appears in a new light after we realize the importance of persuasion and propaganda in the history of scientific development. There are reasonably well-defined situations in which the use of persuasion, far from violating the analyst's code of professional behavior, is not only unavoidable but also rationally justifiable.

1. INTRODUCTION

IF SCIENTIFIC knowledge is in fact, in Popper's phrase, "common sense knowledge writ large," what can we learn, as systems or policy analysts, from recent developments in the philosophy and history of science? Methodological debates cast long shadows over the most pragmatic domains, though the images are often fuzzy, and sometimes reflect patterns of thinking that are already obsolete.

The received view on scientific method, which in one form or another has dominated the philosophy of science from the 1920s to the 1950s, has by now only historical interest for the specialists; but it is still accepted by many researchers as a general scientific ideology. In particular, the influence of logical positivism—a key component of the received view—has been felt throughout the social and behavioral sciences, and nowhere more strongly than in the study of decision-making.

Appeals to the scientific method (rather glibly equated with problem-solving-through-mathematical-modeling) preface textbooks on operational research and systems analysis, and figure prominently in the programmatic statements of the professional societies. For many of its advocates, systems analysis is nothing more than the scientific method extended to problems outside the realm of pure science. Cost–benefit analysts stress the scientific virtues of their methods: quantification, formalization, explicitness, objectivity. Evaluation researchers are supposedly engaged in the scientific assessment of public programs. Even the rational–deductive ideal for ranking policy alternatives according to a strictly defined hierarchical system of values is said to represent an ideal of science transferred to the field of values.

In reality, these references to science and the scientific method are not so much methodological commitments as they are ideological props. They do not direct attention to any deep affinities, but are attempts to increase the collective confidence of a group of new disciplines striving for academic and social recognition. It could not be otherwise, for the conception of science implicit in such proclamations of method lacks historical reality and epistemological substance. Few scientists and philosophers of science still believe that scientific knowledge is, or can be, proven knowledge. Many would, in fact, agree with Toulmin that "if we wish to understand how actual sciences operate... we must abandon the assumption that the intellectual contents of natural sciences actively in debate have 'logical' or 'systematic' structures: we must instead consider how such sciences can succeed in fulfilling their actual explanatory missions, despite the fact that, at any chosen moment in time, their intellectual contents are marked by logical gaps, incoherences, and contradictions" [17, p. 605].

To repeat the initial question: what can students of policy-making learn, not from the idealized textbook image of science that in the past has been foisted on them, but from the 'unsystematic', 'illogical', and all too human activity which is the daily experience of the working scientist? This is a very broad question, and no more than a partial and tentative answer will be given in this paper. The selection of themes necessarily reflects my personal interests, and specific results in the philosophy or history of science are presented in capsule form. I hope not to have misrepresented the views of the scholars whose works I have used. At any rate, the reader is urged to check the correctness of my interpretations through the references given in the text.

In section 2, I consider the craft aspects of scientific inquiry and argue that this perspective, derived mainly from the works of Polanyi, Kuhn and Ravetz, throws new light on some important, but generally overlooked, characteristics of policies and of policy analysis.

Section 3 is concerned with the implications for policy evaluation of Popperian and post-Popperian views on scientific knowledge and its evolution. Of particular importance, here and in the next section, is Lakatos' notion of 'research programmes'.

Section 4 makes use of Popper's 'World 3' to introduce the idea of an autonomous policy space (an idea which can be found, in more or less explicit form, in the writings of a number of students of policy). Strictly related to the notion of an autonomous policy space is that of problem-shifts (Lakatos). I argue that explanations of policy development in terms of policy-space constructs are more satisfying, and potentially more powerful, than other types of explanations.

Finally, in section 5, I discuss the role of persuasion and propaganda in science (taking my clue from Feyerabend's discussion of Galileo's advocacy of Copernicanism). Policy advocacy and 'rationalizations'—far from being, under all circumstances, capital sins against the analyst's professional integrity and intellectual honesty—are not only unavoidable but also rationally justifiable.

2. THE ART OF INQUIRY

Fallibilism and its consequences

Logic shows the inconclusiveness of the basic patterns of argument used in science; history testifies to the possibility of genuine scientific knowledge. This apparent paradox cannot be resolved within the traditional epistemological concerns of the older philosophy of science. Since individual endeavors are fallible, the emergence of a (provisionally) accepted body of knowledge must be explained in terms of social mechanisms. "Nature," writes Ravetz, "is not so obliging as ever to give marks of True or False for scientific work, and so a scientific community sets its standards for itself" [15, p. 82].

Some form of conventionalism is the inescapable logical consequence of fallibilism. If there is no demonstrative certainty for the conclusions of science, their 'truth' or, at any rate, their acceptability as scientific results, must be established by convention: through a consensus of experts in the field, and the fulfilment of certain methodological and professional canons—the rules of the scientific game.

The problem of testing the correctness of a policy or decision is analogous to that of determining the truth of a scientific theory. According to the received view of policy analysis—in which policy-making is equated to decision-making, and the latter is formalized as a means–end relationship—a decision is correct "if it can be shown to attain some specific objective, where the objective can be specified without describing the decision itself" [12, p. 49]. Such a proof requires that three conditions be simultaneously satisfied: (a) it must be possible to detect the specific effects of the decision against a noisy background of concomitant decisions, measurement errors, and random fluctuations of the system; (b) a well-tested theory connecting means and ends is available; and (c) there is agreement on goals.

If even one of these conditions is violated, no objective test of correctness is possible. It has been argued that in such a case agreement on policy is the only practicable test of 'good' policy: policy-makers may agree on policy itself, even if they do not agree on goals or theory. But even more important than such direct agreement on policy as a test of correctness, is agreement on procedures, on the rules of the policy game. In the law, in public administration and, to an increasing extent, also in business administration, decisions are accepted not because they can be shown to produce desired outcomes, but because of a generalized agreement on decision-making procedures. Reliance on detailed procedures, whether in environmental regulations, in the licensing of nuclear power plants, or in industrial quality control, greatly increases the costs of decision-making, but it is also an unavoidable consequence of the cognitive and social complexity of today's problems. As long as the correctness or fairness of the outcome can be determined unambiguously, the manner in which the decision is taken is largely immaterial: only results count. But when the factual or value premises are moot, when no objective criterion of truth exists, procedural aspects acquire special significance.

Belief in the possibility of discovering correct solutions for a wide variety of problems has probably served a useful ideological function in the early stages of development of systems and policy analysis. Today, the traditional preoccupation with analytic methods stressing outcome rather than process must give way to a broader approach, in which procedural design (alternative methods of structuring the decision process) assumes primary importance. Naturally, the criteria for choosing among decision-making procedures are not the same as those used in choosing among alternatives for a particular decision. While in the latter case effectiveness and efficiency are the dominant criteria of choice, the metacriterion for evaluating procedures is legitimacy—the capacity of a procedure to elicit generalized acceptance for the decisions it produces, regardless of their substantive consequences. One hopes, of course, that good procedures will increase the probability of good decisions, but, as Popper has often remarked, it is impossible to justify the rules of a game (including the rules of the scientific game) only in terms of success.

Science as craft work

The work of the scientist requires knowledge that is acquired only through practice and precept and which therefore is not scientific in character. Earlier traditions in the philosophy of science, being mainly concerned with the epistemological problem of truth, have ignored the craft aspects of scientific knowledge. Yet, without an appreciation of these aspects "there is no possibility of resolving the paradox of the radical difference between the subjective, intensely personal activity of creative science, and the objective, impersonal knowledge which results from it" [15, p. 75].

Craft knowledge is a repertoire of procedures and judgments that are partly personal, partly social. Thus, when a scientist decides whether a batch of data is of acceptable quality, he applies standards that derive from his own experience, but also reflect the professional norms of his teachers and colleagues, as well as culturally determined criteria of adequacy. Personal and social judgments are also involved in data manipulation, in the choice of tools and models, in the selection of evidence, and in the construction of an argument.

The importance of craft knowledge and experience is even greater in policy analysis. Because the conclusions of a policy study cannot be proved in the sense in which a theorem is proved, or even in the manner of the propositions of natural science, they must satisfy generally accepted criteria of adequacy. Such criteria are derived not from abstract logical canons but from craft experience, depending as they do on the special features of the problem, on the quality of data and limitations of available tools, on the time constraints imposed on the analysts, and on the requirements of the client.

Craft knowledge—less explicit than formalized theoretical knowledge, but more objective than pure intuition—is essential in any kind of professional work. Aristotle's analysis of traditional craft work (in terms of the four constituents or 'causes' of the task: material, efficient, formal and final) has been applied by Ravetz to the task of scientific inquiry. He identifies the material constituent with the intellectually constructed things and events in whose terms the problem is formulated. The researcher, with his tools and methods, is the efficient constituent of the task. The formal constituent is the argument that supports the conclusion of the inquiry, the latter corresponding to the Aristotelian 'final cause'.

The same scheme can be applied to the work of the policy analyst. The material component of the analyst's task is represented by the data, information and other conceptual inputs used in formulating a problem. The technical tools and methods of analysis represent the efficient component, while the formal component is an argument in which evidence is cited and from which a conclusion is drawn. The 'final cause' is the conclusion itself, together with the related activities of communication and implementation.

This way of describing the fine structure of the analyst's task enjoys one considerable advantage over the more conventional decision-making paradigm. The categories suggested by this paradigm (goals, alternatives, constraints, and so on) focus attention on a rather narrow and specialized use of analysis as an applied 'logic of choice'. The craft analogy, on the other hand, suggests categories (data, information, tools, evidence, argument, conclusion) that are applicable to any type and style of analysis, and can be shown to be particularly useful for assessing the technical adequacy of analytic work [13].

Problem solving on artificial objects

For all its usefulness, the craft analogy conceals an important difference between the work of the traditional artisan and that of the intellectual craftsman. The artisan works with physical tools and materials to produce tangible objects having a certain form and capable of performing given functions. The intellectual craftsman works with conceptual tools and inputs; his end product is an argument leading to certain conclusions about the properties of the intellectual constructs that form the object of his inquiry. This leads to Ravetz's characterization of scientific inquiry as problem solving on artificial objects. The objects of scientific knowledge are not directly apprehended natural phenomena, as asserted by an influential tradition going back to Bacon and Galileo, but classes of intellectually constructed things and events: elementary particle, force, field, chemical element, and also the phyla and genera of 'natural' taxonomy.

Similarly, the objects of policy analysis and policy arguments are not directly perceived social events, but theory-laden constructs resulting from definition, convention, and abstraction. Terms like 'price', 'cost', 'inflation', 'GNP', 'standard of living', 'intelligence', 'crime', are used so often that we tend to forget their abstract and conventional nature. Even the most basic social and economic statistics—for instance, the unemployment and balance-of-payment data that make headlines and are discussed at length on radio and television—are extremely abstract things. In no way do they resemble "the measurements which arise from a direct apprehension of something, as when we measure a length. One cannot, even with good eyesight, go out onto the Treasury steps and observe the domestic level of economic activity" [4, p. 823].

The abstract character of the language of policy and of policy analysis can evoke two different, but equally mistaken, reactions. On the one hand, there is the ever-present tendency to mistake for concrete things what are in fact theories and abstractions—Whitehead's fallacy of misplaced concreteness. On the other hand, one finds the equally widespread suspicion of a general conspiracy of politicians, bureaucrats, managers, and experts to prevent citizens from seeing the true essence of social problems by some kind of verbal magic. But while sympathizing with the desire for a more direct and transparent official language, the philosopher of science cannot fail to notice the similarity between the necessary artificiality of a developed legal or administrative system and that of a developed science. Like the natural world, the social world is just too complex to be comprehended in terms of the concepts we build up in our ordinary experience [15, p. 114]. Naturally, the more elaborate policy constructs are ultimately derived from commonsense experience, but in concrete applications formal requirements of procedure and internal consistency assume greater importance than any desire for immediate intelligibility.

Since analysts with a technical or scientific background are often impatient of legal and administrative formalities, it will be instructive to give an actual example of the unexpected difficulties that may arise in the attempt to simplify a formalized language. In the early days of the Communist regime in Poland a sustained effort was made to draft the laws so clearly that they would be intelligible to the worker and peasant.

"It was soon discovered, however, that this kind of clarity could be attained only at the cost of those systematic elements in a legal system that shape its rules into a coherent whole and render them capable of consistent application by the courts. It was discovered, in other words, that making the laws readily understandable to the citizen carried a hidden cost in that it rendered their application by the courts more capricious and less predictable" [8, p. 45].

3. EVALUATION

Traditional scientific methodologies

The different philosophies of science can be viewed as alternative methodologies for evaluating scientific theories. In this section of the paper, I intend to examine some implications of different forms of scientific appraisals for the evaluation of policies. More specifically, I shall argue that while the approach prevailing in policy evaluation (particularly in the United States) has been severely handicapped by outdated philosophical premises, recent work by Lakatos and other philosophers of science suggests a basic reformulation of the goals and methods of evaluation analysis.

Following Lakatos, three traditional methodologies for the evaluation of scientific theories may be identified: Justificationism; Dogmatic Falsificationism; Naïve Falsificationism.

Justificationism or inductivism—the doctrine that the only scientific propositions are those that either describe hard facts or are correct inductive generalizations from them—is logically untenable. As Hume argued more than two centuries ago, theories cannot be derived from facts.

Dogmatic falsificationism—science cannot prove, but it can disprove—must also be rejected since no conclusive disproof of a theory can ever be produced. Theories always involve hidden background knowledge, *ceteris paribus* conditions, and auxiliary hypotheses: it is not clear which part of a theory would be refuted by a negative result. There are no crucial experiments, except with hindsight: "nature may shout 'no' but human ingenuity... may always be able to shout louder. With sufficient resourcefulness and some luck any theory may be defended 'progressively' for some time, even if it is false" [11, p. 100].

According to naïve falsificationism, a highly corroborated scientific theory refutes a less corroborated theory which is inconsistent with it. But the history of science offers many examples of 'refuted' theories being resuscitated. This shows that refutation should not be confused with elimination. If most theories are born refuted, mere refutations can play no dramatic role in science: "if any and every failure to fit were grounds for theory rejection, all theories ought to be rejected at all times" [10, p. 145]. Naïve falsificationism uses a monotheoretical model of criticism; one single theory is confronted by potential falsifiers supplied by authoritative experimental scientists. This amounts to introducing an arbitrary dichotomy into the corpus of scientific knowledge between what is treated as problematic and what is regarded as unproblematic. But experiments, instruments, and observations are theory-laden, and such theories are often no more corroborated than the theory to be tested (as in the case of the physiological and optical theories involved in Galileo's reports of telescopic observations of Jupiter's planets [7]).

Lakatos' methodology of research programmes

If theories are falsified all the time, the important epistemological issue is not when an unrefuted theory is better than a refuted rival one, but when a theory is better than a rival one if both are known to be refuted. Moreover, since problems are not solved but only shifted, the basic unit of appraisal is not an isolated theory but a whole 'research programme'. A research programme is characterized by a *hard core* (conventially accepted and made provisionally irrefutable by a methodological decision of its adherents), a *positive heuristic*, which defines problems and outlines the construction of a belt of auxiliary hypotheses, and a *negative heuristic* indicating the paths of research to be avoided. Thus, the Cartesian research programme (the universe is a huge clockwork with push as the only cause of motion) tells us to look behind all natural phenomena, including life, for mechanistic explanations, and rules out Newtonian action at a distance.

Examples of competing research programmes in the social sciences are neoclassical and Marxian economics. The core of neoclassical economics is the notion that the economy is composed of free agents who perform different functions but are united by the common goal of maximizing individual utility. The idea that society is divided into competing social classes is rejected, being considered neither correct nor analytically useful. On the positive side, the object of economics is conceived of as the study of rational allocation of scarce resources among competing uses. This study can be carried out at an abstract level, regardless of specific historical conditions, since the goal of maximizing individual utility has universal validity. The core idea of Marxian economics, on the other hand, is that society is divided into classes pursuing different, and conflicting, goals. According to the negative heuristics of the Marxian research programme, economic phenomena cannot be explained in terms of individual behavior, and it is impossible to define a criterion of economic efficiency valid for an entire society. The positive heuristics of the programme direct attention to class struggle, exploitation, and a labor-theory of value as the basic categories of analysis. And because class struggle characterizes one particular type of society, an economic theory of capitalism can be valid only for this particular phase in the history of humanity.

Rival research programmes can be evaluated in terms of the problem-shifts they induce. A problem-shift is progressive if is has greater explanatory and predictive power than previous formulations. A research programme is progressing if it generates progressive problem-shifts, otherwise it is stagnating. Thus, the methodology of research programmes offers not only a rational reconstruction of scientific continuity as well as scientific change, but also a set of normative concepts to evaluate theoretical developments.

Policy evaluation

Evaluators of administrative programmes are justificationists at heart—their working hypothesis is that the programme is accomplishing what it set out to do—but falsificationists by necessity. To get on with their work they must assume that their measuring techniques are unproblematic, or at least less problematic than the hypotheses incorporated in the programme they evaluate; they accept a monotheoretical model of criticism. Like good behaviorists, evaluators think of themselves as objective experimental scientists supplying the policy-maker with hard facts, the 'potential falsifiers' of the programme. In reality, they formulate goals, assign them relative weights, identify actors, define system boundaries and choose yardsticks. Paraphrasing Lakatos, we can say that the target of the arrow of evaluation is shaped while the arrow is already in the air. Evaluation does not assume a fully-articulated policy or programme; it creates it.

Traditionally (and quite understandably, given the expectations of their clients), evaluators have been concerned with goal achievement. But the hope of being able to justify or falsify an action programme by comparing results with goals has been dashed by the discovery of the ubiquitous phenomenon of 'little effect'. As Carol Weiss writes, one of the major obstacles to putting evaluation results to use is precisely their dismaying tendency to show that the program has had little effect [18]. For example, many careful evaluation studies have revealed that the effects of variation in school policies on students' performance are not significantly different, once the students' socioeconomic characteristics are held constant. Interestingly, organizations do not fare better: "Measured against the Olympic heights of the goal, most organizations score the same—very low effectiveness. The differences among organizations are of little significance" [6, p. 258]. Thus policies and organizations, like scientific theories, seem to be 'born refuted' and evaluation, as usually conceived and practiced, can play no crucial role in their development.

The phenomenon of 'little effect' becomes less surprising once we recognize that policy evaluation exclusively in terms of results is bound to be inconclusive at best, and must be supplemented by a careful consideration of process. This raises the question of the appropriate unit of evaluation. If policy problems are never solved but only shifted (and, hopefully, ameliorated), the objects of evaluation cannot be discrete decisions or actions, but related sequences of decisions and actions, together with their behavioral, cognitive and ideological supports. A set of dispositions, theories, proposals, decisions and acts will form a recognizable policy if they are held together by a central core of value commitments. In such a case, following Hylton Boothroyd [3], we speak of *action programmes*, the policy equivalent of Lakatos' scientific research programmes. The limitations of evaluation by results now become clear. If a programme is not to be abandoned at the first signs of difficulty, the core must be made (temporarily) immune to criticism by a common decision of the participants in the policy process. The core may eventually be overthrown, but this will signify a major change in policy—a revolution in some sense. Debate, controversy, and corrections are redirected on to the decisions and administrative arrangements that form the protective belt of the core. The effort to adapt the particular institutional embodiments of the programme to ever-changing economic, political, and technical conditions keeps the protective belt in constant flux, but such changes hardly affect the policy core.

The methodology of action programmes puts evaluation in a new perspective by explaining the apparent paradox of the surprising stability and continuous flux that characterize the life cycle of all major policies. The prescriptions of policy evaluators often appear irrelevant because they are directed at the wrong target. This point is well illustrated by the comments of Mark Blaug on some recurring criticicms of the British National Health Service. Blaug writes:

> "Whether we like it or not, the British National Health Service effectively replaced individual choice in the distribution of health services by collective choice. Thus, arguments about 'market failure' in justifying either government ownership or government finance are totally irrelevant in Britain, unless of course the thesis is that they ought to be made relevant by returning health to the market mechanism. It would seem that there is now a consensus among all segments of British society and among all shades of public opinion that health should be distributed in accordance with need rather than ability to pay, in other words, 'communism in health'" [2, p. 324].

Neither Blaug nor any other competent analyst would deny that there are serious problems of allocation. But as long as the consensus about the use of collective-choice mechanisms survives, solutions have to be found at the level of specific administrative measures (e.g. by selective charges within the National Health Services), without compromising the integrity of the policy core.

4. POLICY DEVELOPMENT

Policies versus decisions

In order to understand policy development it is necessary to draw a distinction between policies and decisions. A decision, in the sense of decision theory, is a choice or judgment made on the basis of available data among well-defined courses of action whose consequences under alternative 'states of the world' are reasonably well understood. The decision–theoretic paradigm does not recognize any essential difference between decision and action: if the decision does not lead to the corresponding act, it was because something occurred to prevent it, and a new decision problem arises. Nor does it differentiate between policies and decisions; policies are simply bigger, high-level decisions, or perhaps, sequences of such decisions. Consequently, good policy-making, rational decision-making, scientific problem-solving are, in this view, largely synonymous expressions. The same analytic categories are used indifferently as an idealized description of good policy-making or as prescriptions for conducting policy analysis.

Now, public and private managers must often make choices in situations which closely approximate the decision-theory model: where to build a school or a hospital; which curriculum to adopt for the next school year; whether to expand an existing plant or build a new one. But policies do not live by decisions alone. The basic constituents of policies viewed as action programmes—dispositions to act, core commitments, theories, plans and their institutional embodiments—remain largely outside the pure logic of choice. In taking a decision, one simply tries to do one's best in the present circumstances, to choose wisely among the available gambles. Policies are characterized by a certain deliberate quality, a relative permanence, and the possibility of further development; they tend to become doctrines, directing future action and giving coherence to past actions. This gives policies an objective character which decisions do not possess.

The decision-maker of decision theory, like the consumer of economic theory, is the sole judge and executor of his own choices. But even in the most tightly centralized organization, few decisions are made and carried out by only one person. Hence the problem of communicating and legitimizing decisions in intersubjective terms. Subjective choices must be related to a plan, a doctrine, a strategic viewpoint, in other words, to an articulate action programme. It should be noticed that, while such post-decision developments are irrelevant to the logic of choice, they are an essential element of organizational policy-making.

As long as policies are identified with decisions, development can only be interpreted as decisionmakers changing their minds. This is a rather simplistic view of policy and does not explain many important events (such as the emergence of unanticipated consequences, and various forms of interaction and feedback in the policy space) that are neither planned nor intended.

The policy space

Lakatos and the later Popper concentrate on the growth and development of theories rather than on their refutation. Theories develop and grow in a quasi-autonomous space of objective intellectual constructs, of thoughts-in-themselves. Popper distinguishes three 'worlds' or levels of reality: first, the world of physical objects and physical states; second, the world of mental states, of subjective preferences and beliefs; and third, a world of objective structures that are produced by human minds but which, once produced, exist independently of them (theories, artistic creations and styles, norms, institutions, problem situations, critical arguments). This 'World 3' is autonomous from the other two levels of reality, though it is related to them by a number of links and feedbacks. Developments in World 3 occur largely as the result of unanticipated consequences of previous theories, and of the problem situations they generate. Lakatos' methodology of research programmes represents a fine-structure explanation of the process of theory development, and adds normative criteria to distinguish between positive and negative developments.

I will now introduce the notion of a policy space consisting of (actual and potential) policy problems, policy arguments, norms, constraints, tentative solutions and their institutional embodiments. The policy space is a subset of Popper's World 3 and, as such, it is largely autonomous though still interacting with the psychological second world of subjective preferences, beliefs, goals, and decisions. In fact, some of the most interesting phenomena appearing in the policy space arise as the unplanned consequences of men's decisions. They are, in Hume's words, the results of human action but not of human design (think of phenomena like traffic congestion, pollution, or inflation). Historian AJP Taylor expresses tersely the objective character of the policy space when he writes that statesmen take one step, and the next follows from it.

In policy-space terms, policy development is a sequence of partly overlapping action programmes. The focus of the analysis is not on individuals and groups as change agents, but on objective features like policy content, evolving doctrines and problem situations, changing constraints, and interactions among different policies. Ideally, one attempts to explain policy development by showing how some overall pattern, which one would have thought had to be produced by an individual's or group's successful attempt to realize the pattern, instead was generated and maintained by a process that in no way had the overall pattern 'in mind' [14]. Perhaps this approach is not as intuitively appealing as a 'second world' approach which, by focusing attention on the actors and the moves leading to specific decisions, seems to offer a more direct causal explanation of the dynamics of policy change. But numerous case studies provide evidence that the influence of particular decision makers and special interest groups on actual (as opposed to anticipated) developments is often over-estimated. At any rate, policy-space explanations have the methodological advantage of minimizing the use of notions constituting the phenomena to be explained; they do not explain complicated patterns by including the end-result as the object of people's preferences or beliefs.

As was mentioned above, Lakatos proposes some normative criteria for evaluating the development of research programmes: a research programme is progressing as long as it keeps predicting novel facts with some success; it is stagnating if it gives only *post-hoc* explanations either of chance discoveries or of facts anticipated by a rival programme. It is, however, very difficult to decide when a research programme has degenerated hopelessly, or when one of two rival programmes has achieved a decisive advantage over the other;

one must not demand progress at each single step.

Criteria for evaluating policy development are even more difficult to discover and to apply. Here I can only suggest some possible adaptations of Lakatos' criteria. First, an action programme may be said to be progressing as long as it succeeds in disposing of issues, i.e. in moving them from the stage of contention to a class of issues which the actors in the policy process judge to be in a state of satisfactory, if temporary, resolution. In comparing two action programmes, *A* and *B*, *A* is progressing if it succeeds in solving or ameliorating problems which proved intractable for *B* (the comparison between the Keynesian programmes of the New Deal and the laissez-faire approach of President Hoover comes to mind). Such criteria of progress may be further refined by distinguishing programme shifts that represent faithful developments of the policy core from those that do not. For example, since the core principle of a national health service is that health care should be distributed according to 'need', a return to the principle of ability-to-pay (e.g. through generalized user charges) would be a regressive move by this criterion, whatever its merits in terms of allocative efficiency. One could, of course, argue that an apparently regressive move (such as Lenin's New Economic Policy) may in fact be the best strategy for blocking serious threats to the integrity of the policy core, for gaining time, and for attracting new support. But this only proves Lakatos' point that, whatever criterion one adopts, one must not demand progress at each single step. Nor should we forget the role that persuasion can play in modifying the standards by which progress is assessed.

5. PERSUASION

Scientific advocacy

New ideas are even harder to sell than new products. Time is needed until favorable evidence accumulates and auxiliary ideas come to the rescue. The very criteria of evaluation have been patterned after the prevailing conceptions, and moulded by existing institutions; and what is counted as relevant evidence is determined by methodological rules distilled from past practice. Because established scientific paradigms tend to become parochial in their range of interests and intolerant of inconsistencies, ideas in agreement with accepted doctrines enjoy a considerable comparative advantage over unconventional proposals.

The Copernican heliocentric theory is probably the best-known example of an epoch-making idea gaining recognition only gradually and indirectly. Significant supporting evidence (stellar parallax and effect on falling bodies of the earth's rotation) could not be produced until about 300 years after the first announcement of the theory. According to Ravetz, "up to the early seventeenth century, a judicious astronomer who had no metaphysical bias in his assessment would return the opinion 'not proven' on the Copernican system, and treat it as an hypothesis" [15, p. 127].

Alexandre Koyré, and other historians of science after him, have likened to propaganda the work of Galileo in support of the Copernican hypothesis.

> "But propaganda of this kind is not a marginal affair that may or may not be added to allegedly more substantial means of defence, and that should perhaps be avoided by the 'professionally honest scientist'. In the circumstances we are considering now, *propaganda is of the essence*. It is of the essence because interest must be created at a time when the usual methodological prescriptions have no point of attack; and because this interest must be maintained, perhaps for centuries, until new reasons arrive" [7, p. 52].

As one would expect, the role of persuasion is even more significant in the social sciences. Thus, in discussing Adam Smith's principles of division of labor and free exchange, the authors of a well-known textbook write: "It is interesting that Smith's book did not contain a logically correct exposition; instead it contained a masterfully persuasive statement of the results of free exchange. It was Robert Torreus, who some forty years after the idea had been 'sold', demonstrated its logical validity. Possibly, had Smith tried to give a logically air-tight demonstration, instead of suggestive plausible interpretation, he would never have made his 'point' popular" [1, p. 211]. George Stigler adds Jevons and Böhm-Bawerk to the list of outstanding economists who "have employed the techniques of the huckster". According to Stigler, techniques of persuasion "have preceded and accompanied the adoption on a large scale of almost every new idea in economic theory" [16, p. 5].

If persuasion plays such an important function in the development of scientific ideas, can policy analysts afford to slight it in the name of an historically mistaken view of scientific method? The moral of our examples is clear: the question is not whether analysts should use persuasion in proposing new policy ideas, but which forms of persuasion may be used effectively and without violating basic principles of professional ethics.

Legitimate uses of persuasion

Analysts attempt to influence policy by modifying the attitudes of policy-makers. When they produce relevant information, check for feasibility, develop models, and compare profits and costs, they seek to change attitudes through cognitive means. But since attitudes do not depend exclusively on rational factors, cognitive means must often be reinforced by noncognitive modes of persuasion. Thus, style, elegance of expression, tension of plot and narrative may be needed to strengthen the effect of descriptive statements which, by themselves, would be incapable of altering prevailing attitudes. Indeed, in our culture maximum effectiveness in communication is achieved neither by purely rational, nor by purely persuasive means, but by a subtle blend of these two means of redirecting attitudes.

Philosophers like Charles L. Stevenson have called attention to the phenomenon of 'persuasive definitions'. Even specialized languages contain many terms that have both a descriptive meaning (sometimes made precise by a technical definition), and an emotive (laudatory or derogatory) meaning. The purport of a persuasive definition is to alter the descriptive meaning of a term by giving it greater analytical precision; but the definition does not make any substantial change in the term's emotive meaning. In the context of policy analysis, one needs only to think of terms like efficiency, optimality, rationality, scientific method, risk, pollution, and needs (as in 'medical needs'). The emotive meaning of such expressions cannot be obliterated by any technical definition, however precise. What is even more important, the definition is actually used, often unconsciously, in an effort to modify attitudes by the interplay of emotive and descriptive meanings. Even the term 'analysis' profits from the laudatory connotation derived from triumphs of the analytic method in mechanics. Thus, we continue to speak of systems or policy *analysis*, though most people agree that synthesis and design are actually more important.

Since persuasion is such a pervasive linguistic phenomenon, the practical question is not whether to reject it, but which forms of persuasion to reject. The history of science can help us in identifying situations in which persuasion can be used legitimately in support of, but not in place of, rational analysis. Consider first a situation in which the psychological effect of purely rational arguments is not strong enough to overcome the inertia of long-established patterns of thinking—even after the need for a change has become clear. For example, it is unlikely that Copernicans could have survived the long march through the social and scientific institutions of their time had they accepted battle on the grounds chosen by their adversaries (mostly entrenched in the universities). Their propagandist appeals to "a new secular class with a new outlook and considerable contempt for the science of the schools, for its methods, its results, even for its language" [7, p. 182] appear justifiable in this context.

Again, the impact of rational arguments on human minds may operate too slowly to bring about timely decisions. For instance, it is doubtful that sorely needed energy policies, requiring profound changes in values and attitudes, can be made acceptable by purely technical arguments. Not surprisingly, some recent proposals (such as Amory B Lovins' soft energy paths) owe their strong popular impact to an extremely sophisticated use of persuasive techniques—backed by some hard analysis.

As a third example, consider the case in which the persuasive support of a new idea is in advance of the rational support. It may be that full evidence is hard to obtain (as in Galileo's case); or that the technical tools for an adequate treatment of the problem do not exist; and it may be that experts disagree and science gives only ambiguous answers, as in many controversies over nuclear safety. In such cases, persuasive arguments (bolstered by whatever empirical and theoretical knowledge is available), may succeed in stimulating interest in the issue and keeping it alive until more adequate methods of analysis have been developed. More generally, since policy analysis cannot produce logically binding proofs but only more or less reasonable arguments, it is clear that persuasion can always play a significant role in increasing the credibility of the conclusions.

One particular form of persuasion is 'rationalization', which psychologists define as the attempt to explain *a posteriori* one's actions by means of rational motives rather than by the 'real' (unconscious) motives. One of the recurrent criticisms of policy analysis is that it provides 'pseudoscientific rationalizations' for politically or bureaucratically determined positions. Thus, former US Secretary of Defense, McNamara, has been criticized for using cost-effectiveness studies as ammunition against congressmen who opposed antiballistic missiles (ABM), even though his own opposition to ABM was based on other factors. Whether or not this particular allegation is true, it is important to understand that it is not necessarily dishonest to use justificatory arguments based on considerations different from those that lead us to the adoption of a given policy position. Policy arguments are always directed to particular audiences, and there is nothing intrinsically reprehensible in selecting the combination of facts, values, and analytic methods which seems to be most appropriate for a given audience. Indeed, there is no unique way to construct an argument: data and evidence can be selected in many ways from the available information, and there are several alternative methods for analysis and ways of ordering values. A policy, like a theory [9], is a cluster of conclusions in search of a premise; not the least important task of analysis is discovering the premises that make a set of conclusions internally consistent, and convincing to the widest possible audience.

In this paper I have discussed a number of methodological issues suggested to the student of decision- and policy-making by the recent literature on the history and philosophy of science. Some interesting analogies between the policy process and its cognitive supports on the one side, and the process of scientific inquiry, on the other, have emerged. Of course, it is not so much the analogies that are important (however striking and heuristically useful they may be), as the methodological approaches they suggest. While these remain still largely untested, it seems possible to assert that the ideas discussed here clearly point in the direction of professional attitudes and intellectual orientations that differ significantly from those associated with older views of scientific method.

One question that has not been explicitly discussed is, how scientific is policy analysis? For, as Professor Eilon has pointed out, one cannot debate such a question in the abstract, but only with reference to a particular conception of science [5]. With respect to Popperian hypothetico-deductive methodology, Eilon concluded that operational research is a scientific activity, but not in every respect. An analogous conclusion may be stated for policy analysis. In this paper I have stressed the similarities with the process of scientific inquiry, but the differences are also important and perhaps deserve a separate investigation.

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ADDRESS FOR CORRESPONDENCE: *Professor Giandomenico Majone, International Institute for Applied Systems Analysis, 2361 Laxenburg, Austria.*