

**Kontextmaterialien zu**

**Band I/2 Das Prinzip Verantwortung**

**Zweiter Teilband: Tragweite und  
Aktualität einer Zukunftsethik**

**hg. von Dietrich Böhler und Bernadette Herrmann**

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# 1. Abraham Edel: Ends, Commitments, and the Place of Ignorance

A general philosophical reflection comes to mind, which I would like to raise before going on to my assigned role of commentator on Professor Jonas' paper. There is, I think, increasing recognition among philosophers that even the most general categories arise and function within a context of theoretical problems, and are subject to modification if they complicate rather than help resolve those problems. Perhaps then it is the means-ends cut itself that needs central reconsideration. Perhaps it is this categorial structure itself that is cracking under the strain of the difficulties it generates. It seems to me that Professor Jonas' paper can be interpreted as underscoring the cracking by carrying to the limit the dilemmas in which we are placed. If so, it is implicitly inciting us to revolt against the tyranny of the means-ends model as applied in social science, and in this respect I find myself in a large measure of agreement. But, of course, among philosophers it is not only agreement but the ground of agreement that is significant – for different grounds, like different formulas, may soon incline in different directions beyond the range of the immediate data. And so just as Professor Jonas will not let the economist be quit of his commitments *qua* economist – indeed this is a central part of his argument – so I cannot be quit of my critical responsibility *qua* critic.

In pursuit of the general reflection, let me first make clear my own perspective on the fact-value problem that lies at the base of the concerns before us, because it has hardened the means-ends distinction as a theoretical framework and often has even been assimilated into it. My attitude is not without ambivalence. On one hand, I regard the fact-value distinction as a categorial cut into two quite obscure categories, of which the first is highly metaphysical and the second is an artificial construct that puts into one bag an extremely varied set of human phenomena, ranging from bare approval to complex judgment. On the other hand, I do not mind using the distinction for any job that it does efficiently. It is highly useful for preventing the smuggling of purposes and interests into an apparently neutral concept or field – for example, it prevents speaking of

»needs« as if they were a factual question rather than the discovery that certain nonsatisfactions produce certain undesirable consequences; or again, it prevents speaking of »adjustment« without recognizing the implicit approval of the state of affairs to which adjustment is desired.

But customs inspection is no substitute for production, and the categories of customs inspection will not run a productive enterprise, either ethical or economic. I want to emphasize three points:

1. The distinction between science and ethics is not equivalent to the one between fact and value; science and ethics each contain both facts and values.
2. The distinctive problems of establishing policy or justifying action rarely raise the question of drawing value conclusions from purely factual data. Typically, they involve going from some facts and some values to *other* values. The unbridgeable gulf may exist, but we rarely have to go where it lies.
3. Some other categorical cuts may be more important for the problem of the relation of science to ethics. For example, the cut between the determinate or definite and the indeterminate or indefinite (in the sense of the extent to which answers can be furnished to questions) runs across the fact-value dichotomy; while many questions of fact are no doubt more easily answered than questions of value, some questions of value are more easily answered than some questions of fact. I therefore do not have to destroy the value-fact dichotomy, although future analysts may decide in the long run that it was just another dogma, of anti-empiricism this time, mistakenly popularized by empiricists. I simply find it less relevant to our problems.

These considerations will indicate the large measure of my agreement with Professor Jonas. He too finds value commitments on both sides of the science-ethics fence. He also finds factual difficulties on both sides. Moreover, his argument leans very heavily on the question of the extent of the indeterminate, both in the picture of the end and in the ascertainment of the means. His three topical arguments are as follows:

1. Economics is not without certain basic commitments which open the way readily to vast responsibilities.
2. Even then it cannot really do the factual job because the goal state it requires is too elusive.

3. We must therefore adopt a policy of caution for our journey into the unknown.

My comments will raise the following corresponding questions:

1. Who defines the economist *qua* economist?
2. If the means-ends model is so helpless here, cannot some other model be found to do better?
3. How do we decide whether the dangers of cautious inaction are greater or less than the dangers of bold action?

## I

Economics, we are told, no longer offers us simply the means of subsistence; the growth of novel powers has reached the point where we have the massive offer of all sorts of goals – we have but to choose and economics will be our willing servant. But, Professor Jonas argues, philosophy in fact cannot furnish the wanted list of ends. He urges us, however, to take heart: at least some commitments are inherent in the economic enterprise as an instituted system of human activities.

The first such commitment, which was imperative in the age of scarcity, is »the provisioning of its members with the physical goods necessary to sustain their lives.«<sup>A</sup> With affluence there came a pluralism of elective goals and so an absence of criteria of choice in economics itself. Professor Jonas goes on to argue that the original categorical imperative expresses the basic self-affirmation of life, and he pushes on from the provisioning of the present generation to making provision for the next generation. It is easy, especially in the integrated large-scale operations of the modern world, to show the growing scope of these responsibilities once they are assumed. Care for the existence of future generations makes us afraid of handing on a plundered planet; once human nature is recognized as affected, criticism of workability and viability comes to embrace the whole humanistic aspect of man's well-being.

With the values that come marching on apace, I have no quarrel. But in this case I want to be the customs inspector. For there is a touch of the

<sup>A</sup> See p. 75. *Die angegebenen Seitenangaben beziehen sich auf:* H. Jonas, *Economic Knowledge and Critique of Goals*, in: R. L. Heilbroner (Hg.), *Economic Means and Social Ends. Essays in Political Economics*, Englewood Cliffs, New Jersey 1969.

old economic appeal to reasonable self-interest in the way Professor Jonas constructs a categorical imperative for the economist: »Act so that the effects of your action are not destructive of the possibility of economic life in the future.«<sup>A</sup> Again we are told, »The rule that economics must watch over its own continued possibility is therefore decidedly nontrivial.«<sup>B</sup> For a bit it sounds almost as if the physicist were being told that he could not do research on a cobalt bomb *qua* physicist since if it were made and exploded it would make research into physics impossible! But it will not help us much to go into such questions as to whether it would be a logical contradiction for an economist reading Professor Jonas' paper to decide that economists interfere too much in the order of things and that therefore as an economist he is committed to working for the abolition of economics! For Professor Jonas is really talking about economists only in relation to the economy itself.

Who or what constitutes the field of economics? Is it a »subject matter«, in whose account we will find defining postulates with value presuppositions? Or is it professors in the universities taking care that their departments do not shrink? Or a minister of the economy in a government, carrying out dominant party policy? Or a U. N. division such as the Food and Agriculture Organization, seeking to encourage production and distribution on a global basis?

And what is »the economy« – a set of activities of people? Where are we extracting the responsibilities? The »field of economics« is a very strange abstraction. Nor will the history of definitions of economics help us; they have at times confined themselves to the laws of the distribution of scarce means to ends, and have not been incompatible with a Spenserian sacrifice of the subsistence of a large part of a population in the name of evolution, or with leaving the subsistence of many to religious charity.

In fact, Professor Jonas is arguing not from the existence of economists but from economic enterprise as an instituted system of human activities. Now human activities express purposes, and so we must either find out men's purposes in these activities or, if we already know them, evaluate them. Presumably descriptive economics, with the aid of anthropology and social psychology, could tell us why men work at what

<sup>A</sup> See p. 79.

<sup>B</sup> See p. 80.

tasks under what conditions and with what expectations. (To a large extent, such aims might be culturally variable.) They also could tell us some of the consequences of these aims. Many of the commitments Professor Jonas describes as inherent in economics could be established equally and even more firmly for all men, not only economists, by a search into the conditions necessary for all or most of the values men hold even when the values differ. Thus the same type of argument that establishes law and order as a minimal goal in political science, or shows the imperative character of peace under conditions of contemporary warfare (however diverse and competitive are the goals men will pursue in a peaceful world), can in the present state of the globe justify most of the responsibilities Professor Jonas wants assumed.

But such an argument does not preclude an attempted *evaluation* of goals held. And here I should like to recast Professor Jonas' initial description of the situation that generates the problem. Economics, we were told, had furnished subsistence, but now, with the development of greater power, it needed to be told what goals to pursue, because it lacked criteria for choice. Let us say instead that economics did not merely furnish subsistence; rather it gave us the value of freedom of choice among many possible ends. Thus it contains the positive criterion of free individual choice. Now it is described as a value, not as a problem or the lack of a criterion. Professor Jonas criticizes the value because of its consequences – perhaps he does not think there is wise use for this freedom at present, just as economists themselves may criticize the situation in which consumer demand, stimulated and controlled in effect by unregulated manipulators, produces undesirable results.

This shift in description is not trifling. The same point arises in Professor Jonas' picture of the traditional removal of natural ends in the eclipse of teleology. Somehow this is always construed as the removal of value from the account of the processes. But actually it is a shift in the locus of value, from a presumed species-end to the will-acts of individuals. The rise of individualism is a different value-selection, not a metaphysical conjuring away of value.

I see the strength of Professor Jonas' position, then, not in the proof that economics *qua* economics has certain commitments on pain of self-contradiction, but in the exhibition that in the modern world, empirical relations can be found which show that some minimal human aims entail

wider commitments as a consequence than they used to, and that the purposes for which we carry on various types of activity (including specialized study of those activities) must be reconstructed because, at present, the consequences of some of the built-in aims have been empirically discovered to be disastrous.

## II

Professor Jonas' attack on the means-ends model is in many respects a crushing one. We can never, in fact, have an adequate account of the end in terms sufficient to weigh all the means and alternatives and choose the best. The goal state that is to be the terminus is »elusive«; the terminus »is spotlighted for our vision out of a darkness of collateral unknowns with which it is inextricably intertwined, unlike the terminal states of isolable and repeatable physical sequences.«<sup>A</sup> This theme is developed precisely after Professor Jonas has persuaded the economist to acknowledge responsibility. What would he answer if the economist now invoked Kant's »ought implies can« to reject the responsibilities so elaborately fastened upon him? If he cannot carry them out, how can they be his responsibilities? Professor Jonas argues that the only laboratory for testing hypotheses about the totality is reality itself. The usual scientific method of dealing with isolated parts will not do. And the experiment changes reality: »When we have run our experiment, the deed is done and we cannot return to the initial state.«<sup>B</sup> Our knowledge comes too late.

Some of the points Professor Jonas is making here, in his attack on the applicability of the means-ends model, could be restated in Deweyan language. We never really deal with ultimate ends but with focal aims or ends-in-view that are set within a problem context, so that our very pursuit of the end-in-view rests on the hypothesis that its pursuit will help solve the problem. Dewey offers an altered model in which proposed goals are tested on definite criteria posed by the situation, rather than serving as ends in unalterable blueprints; fresh ends emerge in the process, to which the model of policy formation must be constantly sensitive. Whether such a model is helpful depends on how well structured the

<sup>A</sup> See p. 70.

<sup>B</sup> See p. 86.



situations turn out to be, how extensive our theoretical knowledge is of the area, and how manageable the novelties are. Dewey's pluralism would prevent him from seeing the whole human life as the integrated situation which Professor Jonas' dire speculations envisage.

But clearly the issue need not be all-or-none. There are many situations that exemplify the Deweyan model. Some diseases have been intelligently eliminated; in others we find that we have cleared the way for more virulent strains. The life-span has been extended, though fresh problems emerge. It is true that there is greater danger in large-scale experiments, for there is much that we do not know. In really total experiments, there are the dangers of setting off utterly destructive chain reactions. Life is becoming increasingly precarious. But not all experiments are equally precarious. Many have a margin of maneuverability in case they begin to go wrong. A kind of operations-research model is possible in which many fields cooperate and the dangerous variables are kept under constant scrutiny. Perhaps this is like driving a car on rough terrain, as against an automated train on a prepared roadbed.

Professor Jonas might well point out that this begs the question by assuming the reliable knowledge at issue. But how far is he prepared to carry his argument? I am reminded of G. E. Moore's definition of »right« in *Principia Ethica* as that act which makes the whole world better off than any alternative possible act would have done. Since the definition poses impossible conditions for the knowledge we must have, Moore quite consistently decides that we cannot really furnish any evidence that would be adequate to change any current moral rule. The outcome is that he advises utter conformity to any rule that happens to exist in any community, but only within that community! Professor Jonas' recommendation is not conformity, but neither is it the hope of improving our knowledge and expanding the area of careful experiment. It is rather that the fact of ignorance be incorporated in the imperative itself. What this portends, we must now ask.

### III

I am not sure that I really understand how ignorance can be incorporated into the economic imperative. There are several possibilities.

1. The proposal may be recommending a virtue of humility. This is generally said to be characteristic of our greatest scientists who know all the difficulties and shortcomings in what they know.

2. It may be urging caution about specific undertakings. For example, the use of nuclear weapons in war today might unleash uncontrollable disastrous consequences. That we cannot foresee them is part of the grounds for not going ahead. But what about a guaranteed annual wage in America? Would it be irreversible if we adopted it, irrespective of the goodness or badness of consequences – unlike Medicaid in New York State (which proved to be reversible in part) or the nationalization and subsequent denationalization of steel in Britain? Surely the kinds of consequences anticipated play a large part in deciding what risks to take; if so, then knowledge is involved in estimating the extent of ignorance.

3. Perhaps the proposal calls for incorporating only some particular sense of ignorance. There may be various kinds of unknowns – variables in equations that may turn out to have several values, questions not touched by a particular system that is being used, unknown state-conditions to which a well-established theory is being applied, variables whose values cannot be predicted in advance but can be decided in time by an on-the-spot inspection at a given point, theoretical limits indicated by a theory itself, unknowns we do not even know are unknown. The haunting fear that some undiscovered variable is at work may have adequate grounds in some areas and only an obsessive quality in others – but to distinguish the latter we must have some knowledge about obsessions.

4. Perhaps the proposal is directed only against certain types of experiments – Utopian, as against middle-sized projects. But perhaps it is not the size but the oversimplification that is the issue. Then we should do a study of Utopias to learn the lessons of planning. However, the theory of planning is itself a form of knowledge.

5. Perhaps the proposal may be put in positive form by incorporating not ignorance, but something like *openness*. There is considerable similarity between the proposal and the kind of argument Julian Huxley gives in *Touchstone for Ethics* – that a central task of ethics today is to maintain openness against the kind of hardened closure that makes readjustment on a large scale of changing conditions impossible. We do not want to die out like the dinosaurs. In a given scheme of human self-

regulation, this means freedom of criticism, wide participation of various points of view in decisions, flexibility of goals over different generations in a changing world, and so on. Such openness is a characteristic of even some deterministically inclined philosophies. Remember Marx's objection to Utopian socialism and his belief that quantitative changes may produce qualitative leaps, so that we cannot rest on plans but have to be constantly on the watch.

Perhaps in these arguments I am binding or taming what is offered as radical ignorance. I do not think that Professor Jonas is offering »I disbelieve because it is possible« as a latter-day substitute for »I believe because it is impossible«. But there is one argument against his proposal that seems to me to be decisive. He appears to be assuming that the consequences of caution are less far-reaching than the consequences of considered action. In human life it is not always so. Perhaps the timid drivers cause more accidents than the rash ones. Those who were so busy arguing against socialism, and who assumed that without intervention the old ways would prevail, missed the impact of the rise of corporate organization. But once we recognize this, then the answer is unavoidable – we need knowledge to certify that doing nothing will not have more disastrous consequences than major experimentation. An efficient administrator may decide to make no decision so that a situation will mature, but it is not ignorance but wisdom that he incorporates into his decision not to decide on that occasion.

I conclude then that as far as the unknowns are concerned, we need to incorporate not ignorance but the study of different kinds of unknowns and their roles; that insofar as attitudes are concerned, both humility and sharp critical power are essential; that insofar as models of decision are involved, we should use those that allow a maximum of flexibility and maneuverability. But as for ultimate attitudes, I think the stress on courageous responsible experiment has more to be said for it than general warnings of caution.

## 2. Adolph Lowe: Economic Means and Social Ends

### A rejoinder

The essays assembled in this volume raise a number of challenging questions. Limits of space make it impossible for me to discuss all of them in an adequate manner. Therefore, I will apply myself in the following observations to a few fundamental problems, a satisfactory answer to which the theoretical soundness and practical relevance of a Political Economics as outlined in my introductory paper may well depend.<sup>A</sup>

My ideas as originally formulated in the book *On Economic Knowledge* have certainly not escaped the fate described by Professor Lerner – that of being rejected on the one hand as quite wrong, and, on the other hand, being played down as affirming only what is general knowledge and accepted practice. But whereas these critical responses usually follow each other as time passes, I have been exposed to them simultaneously. I shall try to defend myself to the best of my ability against the first charge. The second, however, about the absence from my »message« of any revolutionary discoveries, I have myself stressed from the outset. Apparently the detailed description in my book of the gradual emergence of a Political Economics in the history of economic thought has not sufficiently clarified this point. So I should like to state once more and most emphatically that, notwithstanding certain reservations against

<sup>A</sup> In doing this I will not confine myself to commenting on the papers printed here but also, at least implicitly, I will refer to the discussion that followed their delivery at the two Symposia. It is only natural that my rejoinder should deal mainly with my critics, referring to affirmative voices only occasionally when they improve on my own argument. The text itself follows the line of several statements I submitted during the proceedings. But it tries to present the issues in a more systematic fashion independent of the chronology in which they arose at the time and also of the order in which they are taken up in this volume<sup>1</sup>.

<sup>1</sup> this volume] *Es handelt sich um folgenden Band, auf den sich auch alle Seitenangaben in den folgenden Fußnoten beziehen, sofern nicht anders angegeben*: R. L. Heilbroner (Hg.), *Economic Means and Social Ends. Essays in Political Economics*, Englewood Cliffs, New Jersey 1969.

conventional techniques of analysis and traditional economic policies, my entire undertaking aims at little more than a systematic formulation of the major trend in contemporary theory and practice. To make this implicit trend explicit, and to generalize and deepen the insights that we owe to the »new economics« of Keynesian provenance, to the decision models of the Dutch school of econometrics, and to other modern toolmakers – these are the intentions that underlie my project.

As indicated earlier, even this modest endeavor is far from accomplished. It is the significance of these essays that they contribute notably to the furthering of this task, especially by scrutinizing closely my methodological approach, by pointing out the deeper implications, political and philosophical, of my position, and by the general fairness of their critical stance. As I did on the original occasion, I wish again to express my genuine gratitude for so productive a response to my ideas.

## The Problems under Dispute

In order to locate the principal points at issue I will take my bearings from a comment with which Dr. Machlup introduced the oral presentation of his paper. He found it characteristic of my approach that I am, at one and the same time, concerned with both the science of economics and the actual state of the Western economic systems. It is indeed true that I see the realms of economic theory and practice more closely interrelated than is customarily recognized. Of course no one denies that theoretical knowledge translated into rules for the framing of policy greatly affects economic reality. But there is a reverse and less obvious relationship, in which the actual states and processes of an economy influence our capacity for theorizing.

As I said above,<sup>A</sup> the link in this reverse relationship is the notion of *order* in the inclusive meanings of regularity of state and motion on the one hand, and of a satisfactory and stable level of provision on the other. The former trait defines a »positive« concept of great generality, since it states a precondition for theoretical reasoning – that is, for generalizing

<sup>A</sup> See pp. 4-7.

(*Es handelt sich um Seiten aus Lowes Beitrag »Toward a Science of Political Economics« im o. g. Band.*)

explanations and predictions – in all sciences. The latter is a »normative« concept and, as such, is limited to social research where human actions and their purposes are in the center.

This difference in the logical status of the two constituents of economic order seems to suggest that they are also causally independent of each other. Certainly, as I pointed out earlier, one can conceive of regular and thus predictable economic movements which would result in what by modern standards would be regarded as unsatisfactory provision levels. However, the converse is by no means true, because, when judged by the same standards, satisfactory provision, far from being independent of »orderly« motions within the system, is conditional on a high degree of regularity and thus of the predictability of such motions.

This condition holds for all types of economic systems, centralized or decentralized, but it has a particular relevance for market systems. There predictability is a concern not only of the scientific observer but, before that, of the economic actors themselves who, in the absence of authoritative guidance, must be able on commonsense grounds to foresee the tendencies in their fields of action. Therefore, in an uncontrolled market, movements must be regular enough to enable the individual marketer to predict, at the least, all major changes in their *direction*. In the absence of such autonomous regularity, stable provision perforce depends on the contrived adjustment of these movements with the help of public control. But in order to choose, in a given instance, what measures of control are appropriate, we must be able to predict their effect – a task for the framer of economic policy and his scientific helpmate, the economist. Thus we arrive at the general conclusion that only economic systems whose movements are or can be made sufficiently regular to permit the prediction of major changes, will be efficient engines of provision.

On this basis we can now summarize the gist of Political Economics in the following propositions:

1. The autonomous markets of industrial capitalism lack the required minimum of order. Their uncontrolled movements are too irregular for the individual marketer to predict major changes correctly and thus to achieve the interlocking patterns of behavior which assure stability of aggregate provision.

2. As a consequence, these markets have been progressively subjected to public control, however without as yet displaying a satisfactory degree of stability.
3. The main reason for this failure is the limited range of conventional market controls. These controls confine themselves to altering the micro-units' field of action by opening or closing marketing opportunities, without, however, controlling their responses. I shall call such controls »primary«.
4. If orderly states and processes are to be brought about, to safeguard the viability of a system based on decentralized decision-making, additional or secondary controls must be introduced. These must bring the response mechanisms of the marketers in accord with the behavioral requirements for stable aggregate provision.
5. In order to discover what controls are likely to bestow order in any given instance, the first step is to specify the level and composition of aggregate provision to be obtained in a consistent set of macro-goals. From the knowledge of these macro-goals, of the initial state of the system, and of certain technological constraints, it is possible, with the help of a particular research technique called »instrumental analysis«, to determine the goal-adequate movements of and within the system, as well as the goal-adequate motivational and behavioral patterns on the part of the micro-units, and the goal-adequate public controls that may be needed.
6. The findings of instrumental analysis in terms of goal-adequate controls must then be applied as measures of economic policy, so that the actual motion of the system is transformed into goal-adequate motion.
7. To the extent to which such transformation is successful, the practical demand for a satisfactory and stable level of provision will be met, as will the theoretical demand for such regularity of motion as permits generalizing explanations and predictions.

In the preceding essays, almost all of these propositions have, in one form or another, come under critical fire. They therefore offer a convenient framework for my defense. I will begin by restating the *facts* (propositions 1 and 2) that call for a transformation of traditional theory and practice in the direction of a Political Economics. Next comes a brief

discussion of the nature of *macro-goals* and of the manner of their political stipulation, succeeded by an elaboration of the methodological principles underlying *instrumental analysis* (proposition 5). This will be followed by a review of the relationship between primary and secondary *controls* and their practical application (propositions 3, 4, and 6). Some reflections about the ultimate *criteria of goal-setting* – a topic only lightly touched upon in Chapter 12 of *OEK*<sup>1</sup>, but thoroughly explored in some of the foregoing writings – will bring my apology to a close.

## The Factual Background

My advocacy of a Political Economics and of instrumental analysis as its principal research technique derives from a particular evaluation of certain strategic facts. These facts concern the tendencies of the autonomous movements of modern industrial markets, which seem to me in conflict with the requirement of order in the sense defined above. In fact, in my book and in my position paper<sup>A</sup> I have gone farther by suggesting that, owing to the gradual relaxation of certain natural and social pressures and to the disappearance of some automatic escapements, and in the wake of the progressive immobilization of the industrial structure coupled with ever more rapid technological changes, these »disorderly« tendencies are on the increase when compared with the competitive era of expanding capitalism.

It stands to reason that my diagnosis of the present state of affairs cannot be refuted by a different reading of the historical trend. Yet I readily admit that, as far as the latter is concerned, not only is the available factual material scanty but, as Dr. Wallich rightly stresses, much that appears as »new« may only reflect the growing sophistication of modern analysis. Perhaps my hypothesis will sound more plausible if one keeps in mind that it refers to the tendencies of *uncontrolled* industrial markets and should therefore not be judged by the manifestations of organized capitalism after the Second World War. Do we trust the »self-

<sup>1</sup> *OEK steht als Kürzel für: A. Lowe, On Economic Knowledge: Toward a Science of Political Economics, New York 1965.*

<sup>A</sup> See *OEK*, Chap. 3, and pp. 11-14.



equilibrating« market enough to acquiesce confidently in the abolition of all the micro- and macro-controls that have been installed since 1929?

But since historical evidence is inconclusive, let me concentrate on modern experience, especially since I include the recent past in my pessimistic diagnosis in spite of the widening range of public controls.

The case for a high degree of stability and predictability in modern business behavior has been stated above by Dr. Wallich,<sup>A</sup> and there is no better test for my own views than a confrontation with his. For this it is important that we seem to agree about the facts themselves, our disagreement beginning only when it comes to their interpretation.

A growing variety of action directives, among them the progressive substitution of »satisficing« for »maximizing« behavior; uncertainty of expectations; prevalence of oligopolies and generally monopolistic manipulations and, as a consequence, a narrowing of price and wage fluctuations; rapid technological progress – all these characteristics of the modern scene which Dr. Wallich stresses also underlie my own argument. Dr. Wallich concedes that some of them, in particular oligopolies and satisficing tendencies, reduce rather than enhance the predictability of business behavior. But he sees compensating factors at work in the advance of professional management, interproduct competition, and other antimonopolistic forces. And if administered prices and wages make the structure of the market more rigid, this should in his view only facilitate prediction.

If Dr. Wallich and I draw such different conclusions from similar premises, the likelihood is that we focus on different aspects of the same phenomena. His is mainly a micro-economic discourse which studies the effect of the modern market organization on decision-making in the individual firm. More precisely, his emphasis is on the professionally managed corporation, and he aptly describes the behavior of what Professor Galbraith has labeled the »Technostructure«.<sup>B</sup>

There is no reason to impute to Dr. Wallich the exaggerated notion Galbraith has of the significance of these oligopolistic corporations in the totality of modern business organization. If we give its due to the stratum of middle-sized enterprises, for whom the corporate form is little more

<sup>A</sup> See pp. 156-158.

<sup>B</sup> See J. K. Galbraith, *The New Industrial State* (Boston: Houghton Mifflin, 1967).

than a legal convenience and whose transactions remain market-bound, the range of managed predictability of costs, prices, and sales shrinks considerably. More important – and this is my major objection to Dr. Wallich's optimism – business planning, even at its most comprehensive, is still inadequate to establish *macro-economic* order.

The decisive point has been well stated by Professor Meade in his review of Galbraith's book:<sup>A</sup> »Professor Galbraith asserts that each modern corporation plans ahead the quantities of the various products which it will produce and the prices at which it will sell them; he assumes ... that as a general rule each corporation through its advertising and other sales activities can so mould consumers' demand that these planned quantities are actually sold at these planned prices. But he never explains why and *by what mechanism these individual plans can be expected to build up into a coherent whole* [my italics]. ... In short, if all individual plans are to be simultaneously fulfilled they must in the first instance be consistent.«<sup>B</sup>

In a competitive system it is, of course, the market mechanism, operating through price changes, which is supposed to bring about this consistency of business plans. Unfortunately this result can as a rule be achieved only *ex post*, and the respective adjustment processes are themselves a main cause of aggregate instability. Moreover, with the elimination of price flexibility and the weakening of competitive pressures, this adjustment mechanism is rendered inoperative, without there being any substitute as long as corporate behavior is left to its own devices. And this all the more so since, as even Professor Galbraith admits, »there is no *a priori* reason why the policy pursued by any two mature corporations will be the same, for there is no reason to assume that the goals or intensity of commitment to goals will be the same in any two cases.«<sup>C</sup> Therefore, in contrast with a widely held opinion also voiced during our discussions, a privately planned capitalism is by no means superior to a competitive market organization so far as predictability and macro-economic stability are concerned.

<sup>A</sup> J. E. Meade, »Is 'The New Industrial State' Inevitable?« *Economic Journal*, LXXVIII, No. 310 (June, 1968), 372-392.

<sup>B</sup> *Ibid.*, pp. 377-378.

<sup>C</sup> See Galbraith, *op. cit.*, p. 159.

The remedy is, of course, public control, to which Dr. Wallich refers only in passing. This now leads to the cardinal question of whether existing controls, mainly derived from the »new economics«, have succeeded in overcoming the difficulties inherent in the modern industrial structure. With my critics I would agree that nowhere in the Western world has the economic process during the last generation exhibited the excessive fluctuations characteristic of the era preceding the Second World War. Moreover, a major share of fiscal and monetary controls in this achievement cannot be doubted, though rising military expenditures may have been the principal force of stabilization. For this reason neither national nor international experiences during the postwar era give any cause for complacency, not to mention the fact to which Dr. Wallich and others have rightly pointed – namely, that as public insight into the man-made nature of most of our economic ills grows, we become much more politically sensitive to their social impact.

I have dealt with the relevant postwar events at length in my book,<sup>A</sup> and I want to refer here to only one further instance which may still be topical when this book appears. Earlier I discussed the serious gamble that was involved in the Kennedy tax reduction.<sup>B</sup> We are now engaged in the reverse experiment of the surtax of 1968. In rising protests over the tardiness of Congress in enacting the necessary legislation, it has been widely forgotten that, up to the end of 1967, economic experts were deeply divided as to the wisdom of a tax increase, because they could not agree on whether the likely consequences would be stabilizing or deflationary. And even now, in the summer of 1968, he would be a bold man indeed who dared to predict the ultimate effects of this surtax on the level of output and employment in 1969, leaving out of consideration any exogenous influences arising from military developments. But what I wish to stress is that this uncertainty on the part of scientific observers, as well as of investors, arises from the unpredictability of the responses of producers and consumers to this type of control. This uncertainty of response is the very basis of my argument.

As was shown earlier,<sup>C</sup> the same obstacle hampers experimentation with econometric prediction models. If it is true that the weak link in these

<sup>A</sup> *OEK*, Chaps. 2 and 3.

<sup>B</sup> See p. 10.

<sup>C</sup> See p. 8.

models is their behavior equations, no refinement in research techniques can yield a serious improvement in scientific macro-prediction. Only a modification of the research object itself – that is, the regularization of market behavior – can achieve this professed aim of Political Economics.

## The Nature of Macro-Goals

Regularization of market behavior is a function of public control. But one cannot exert control without being aware of the specific aims these controls are to attain. This introduces macro-goals as the fulcrum of the analysis, and leads to the »inversion« of the conventional procedure in which terminal states are treated as unknowns to be derived from known patterns of behavior. Now, however, order-bestowing behavior patterns and the controls that are to establish them have become the major unknowns, which can be established only in relation to a stipulated terminal state or macro-goal.

I shall revert to the details of this »regressive« method of analysis and its methodological justification in the following section. Here, in enlarging on my earlier remarks,<sup>A</sup> I should like to add some comments on the general nature of macro-goals, on the question of whether »ends« can be stipulated independently of the »means« with whose help they are to be realized, and on the different categories of possible »goal-choosers«. Discussion of the most fundamental problem – namely, the ultimate criteria that are to guide us in selecting the »right« macro-goals – will be taken up later.

1. My first concern is with dispelling a misunderstanding in Dr. Machlup's rendering of my views. It relates to the question of whether pronouncements on macro-goals should be placed in the category of »positive« or of »normative« statements. Fortunately there seems to be agreement between us as to what the two critical terms are to mean in order to be useful in scientific discourse. To me the most plausible distinction between the two, which apparently Dr. Machlup also accepts,<sup>B</sup> refers to statements about »what is« as contrasted with statements about »what ought to be«, leaving alone for the moment the further questions of

<sup>A</sup> See p. 34-36.

<sup>B</sup> See p. 103.

from whom the »ought« is to issue, and of the criteria for his choice. In this interpretation we can also speak of the difference between factual statements and value judgments, one essential characteristic of the latter being that they »cannot be tested by empirical procedures and cannot, therefore, be admitted into the body of positive science.«<sup>A</sup>

Now in the face of what I thought were unambiguous formulations to the contrary,<sup>B</sup> Dr. Machlup takes me to task for overlooking the »unquestionable plurality of macro-goals«, choosing among which »will always force us to engage in value judgments.« He also questions my treating these goals »as legitimate data in positive analysis as long as they are clearly stated and are examined only in relation to the means suitable for their attainment.«<sup>C</sup>

The core of the misunderstanding is obviously the notion of »data« and the precise sense in which value judgments and the macro-goals derived from them cannot be »admitted« into the body of positive science. If Dr. Machlup means to say that determination of the »rightness« or »wrongness« of a macro-goal is no task for positive science and that such rightness cannot be the subject of observations to be tested in accord with acknowledged scientific procedures, we are in full agreement. This does not, however, exclude any macro-goal, once it has been stipulated by some »nonscientific« procedure, from serving as a »premise« from which scientific reasoning can derive testable conclusions. Its logical status is then no different from that of any proposition serving as an axiom in a particular realm of knowledge. This, and this alone, is the use which instrumental analysis makes of the »datum« macro-goal, a procedure that in Dr. Machlup's own words »does not involve the analyst's value judgment and is not normative in character.«<sup>D</sup>

This does not, of course, preclude further examination, logical or otherwise, of such axioms, by submitting them to the critical principles of some different field of knowledge, and as I have made clear earlier,<sup>E</sup> I fully agree with the call for such a »vindication of goals«. However, in line with a tradition which, I thought, had been abandoned as a result of the work

A See p. 111.

B See pp. 18, 24, und *OEK*, Chap. 12.

C See p. 128.

D See p. 116.

E *OEK*, Chap. 12.

of Kenneth Arrow and others, Dr. Machlup assigns the function of a »justification of values« and thus of the establishment of criteria for a choice among rivaling goals and means to a realm of knowledge called »normative economics«. There indeed we disagree since, as I am going to explain in the last section of these comments, vindication of economic goals must be based on criteria which are relevant for every kind of social action and which therefore far transcend any field of inquiry that could legitimately be labeled »normative *economics*«. Still, remembering Dr. Machlup's skeptical verdict on welfare economics earlier in these pages, I wonder whether, notwithstanding our verbal contradictions, we are not in substantive agreement after all.

2. In some remarks of Drs. Edel, Nagel, and Wallich, another doubt has been cast on my procedure of treating macro-goals as »givens« in means analysis. Do we not, in our policies, as a rule, simultaneously pursue several macro-goals, whose feasibility and compatibility cannot be taken for granted? Do not such goals frequently change their role, so that what appears as an end in one context becomes a means in another? Moreover, do we not also apply value judgments to the selection of *means* irrespective of the instrumental test of their suitability? And more generally, does not the interdependence of all social phenomena nullify all specialist borders, thus on principle depriving the ends-means distinction of operational significance?

I have dealt with some of these questions before,<sup>A</sup> and will confine myself to indicating the direction in which the answers must be sought.

No doubt a set of macro-goals cannot be stipulated, as either a scientific premise or a political act, unless their feasibility and mutual consistency is assured. Mathematical programming was cited above as one of the techniques for investigating the feasibility of goals relative to the available resources, and thus as an auxiliary tool of instrumental analysis. The question of consistency raises subtler issues. To tackle them one must realize that, in the realm of economics, macro-goals are rarely incompatible in any absolute sense. What often makes them appear so is our reluctance to apply the specific means necessary for their joint realization. Take as an example the stipulation of full employment coupled with price stability, two goals which in our experience have so far proved

<sup>A</sup> See *OEK*, Chap. 12, especially for the reversibility of ends-means relations.

irreconcilable. But were we willing to introduce severe wage and price controls accompanied by rationing or, in the extreme case, by the nationalization of key industries, the apparent contradiction might disappear. So it is ultimately our negative valuation of certain instrumentally adequate means that creates the semblance of incompatibility.

For this reason the real issue is how to relate the value judgments that we attach to certain means to the value judgments that underlie the selection of our goals. This is the *locus* of most practical conflicts, conflicts that can be resolved only by *another* value judgment – that is, which is of greater significance in a given instance: attaining the goal or preserving the integrity of our original evaluation of the means. Differently stated, the value criteria for means selection enter as criteria of optimization into the stipulation of the macro-goals themselves.<sup>A</sup>

Feasibility and compatibility studies are thus indispensable preliminaries of goal stipulation, and as such are a legitimate part of Political Economics. However, at the present stage in the development of social research, I expect little help in this or any other pursuit of Political Economics from what is widely advocated as »interdisciplinary« work. In this respect I associate myself with a communication received from one of the participants in our discussions who himself is an advocate of a »synthetic« approach. According to him, »we do not appear to have a viable language or translation devices by which the different social sciences can be brought together into systematic cooperation.« This is not to deny that economic processes are embedded in a comprehensive social and cultural system and are interdependent with the latter's motion, nor that the eventual scientific conquest of this wider territory should enable us to extend and refine our specialist investigations. At the same time it should not be forgotten that all so-called meta-economic influences can affect economic processes only through the channel of market behavior. In other words, by ascertaining and controlling motivational and behavioral patterns as they operate *within* the economic sphere, we implicitly take care of the effects exerted by meta-economic factors. Therefore, concentration on these intra-systemic phenomena is a legitimate shortcut.

<sup>A</sup> *OEK*, pp. 260-261.

3. A shortcut of a different kind becomes necessary when we have to decide to whom we are to entrust the stipulation of the macro-goals. We are indebted to Dr. Machlup for having shown once more in his lucid summary of the basic tenets of modern welfare economics that any attempt at deriving such goals from the social preferences of the individual members of a larger community must fail. Even the »heroic assumption« that we could determine the trade-off rates between all conceivable social goals acceptable to each member cannot bridge the gap between conflicting objectives. Therefore, a political decision must be »imposed« in some sense – except in the case of a more or less perfect *consensus*, and even then the achievement of such a *consensus* is mainly a political task.

Dr. Kaysen has presented us with a comprehensive survey of the political processes through which macro-goals are actually established in the framework of American institutions and, in particular, of the various roles which the »economist as adviser« can play in this context. His paper fills a serious lacuna in my own work, and I gratefully accept it as an exemplary demonstration of the manner in which the level of abstraction of a theory can and must be lowered if it is to serve the framing and implementation of policy. But there are some issues in Dr. Kaysen's paper on which I should like to offer a few supplementary rather than critical comments.

The first concerns the distinction between the stipulation of overall goals, such as full employment or a certain rate of growth for the system at large, and the specification of such goals, if possible, in terms of concrete, quantifiable targets. Dr. Kaysen describes from experience the role, more often than not a clandestine one, which the professional economist plays in his capacity as adviser to promote overall goals. Frequently this task devolves upon him by default, when no other authoritative voice can make itself heard in the crosscurrents of the democratic process. Still, as a matter of constitutional principle, one may doubt that this is a legitimate function of an expert who »represents« only himself.

However, no one will deny the economist a major part in translating a politically accepted goal into concrete targets. To decide whether full employment should be spelled out as not exceeding a 4 per cent level of unemployment compatible with price stability, or as a maximum level of 3 per cent even if this has inflationary repercussions, requires an understanding of the remote consequences of both of the two states, as



well as knowledge of the means required to achieve them – requirements which only professional competence can satisfy. At the same time we saw above that, in most of these decisions, more than detached analysis is involved – namely, a value judgment on the respective means to be applied. Therefore one must never forget that, even when he acts in what appears on the surface as a purely technical capacity, the economist is likely to step over – indeed, will have to step over – the boundaries of his »positive« science.

All this fully agrees with Dr. Kaysen's views. But the question remains as to what political criteria are to guide the value judgments of the advising economist or, for that matter, of anyone who stipulates macro-goals and specifies targets. Dr. Kaysen himself recognizes a »natural bias of economists ... toward believing that consumers ought to get what they want, in some ethical sense of the word.«<sup>A</sup> A generation ago such may indeed have been the bias of the large majority in our profession, but I am not sure how true this is in an age so conscious of the frequent clashes between social and private benefits, and of the grave undersupply of public services. In any case, at this stage we are not looking for ultimate ethical criteria, but for a lodestar of political decision-making.

The answer seems to lie in another distinction that Dr. Kaysen introduces: the distinction between *settled issues* and *live issues*, with antitrust policy and international monetary policy as his paradigmatic examples. Issues are settled or live according to the degree of public *consensus* concerning the means by which we deal with them, and the economist looks like a merely technical adviser to the extent to which the value judgments underlying his decisions reflect the prevailing political aspirations. Take the example of fiscal and monetary controls. Since both are by now fully accepted by American public opinion, tax increases to fight inflation can be presented as merely technical advice derived from the new economics, whereas control of prices and wages – even if they were more effective as instruments of price stabilization – would still be treated as the offspring of a dubious ideology.

This leads us to the fundamental problem. The distinction between settled and live issues is equally applicable to the overall goals themselves from which all specific targets emanate. To clarify this point I must first

<sup>A</sup> See p. 149.

introduce another distinction – that between *order-protecting* and *ameliorative* goals. The former express the minimum conditions for satisfactory provision as understood by majority opinion; the latter are propagated by reformers or revolutionaries as conditions for a provision optimum. Historically considered, the distinction is by no means rigid. What in one era is an ameliorative goal of a struggling minority, may well be regarded in the next generation as a minimum condition for social survival. Still, for any given period the distinction seems precise enough to serve as a point of orientation.

In my previous writings I have proclaimed stabilization and balanced growth – that is, the full utilization of available resources and the steady absorption of resource increments – as the major order-protecting goals of our age. No one familiar with the postwar history of the West will doubt that these goals are today settled issues that express the aspirations of the overwhelming majority. Their authoritative stipulation thus seems to be in full accord with our constitutional principles. Certainly this in itself does not confer upon them any »absolute« dignity as expressions of a »general will« based on some ultimate ethical standard. But when judged by the maxims of political practice, a macro-goal supported by public opinion at large can legitimately claim the place of an empirical »datum.«

## **Instrumental Analysis once more**

Datum for what? With this question we re-enter scientific territory which, in submitting my position paper based on the methodological and substantive analyses in *OEK*, I thought I had exhaustively explored.<sup>A</sup> However, the challenging questions Dr. Nagel has raised leave no doubt that neither my own statements nor Dr. Gurwitsch's perspicacious exposition of my views, with which I fully concur, have succeeded in breaking down all barriers to a full understanding. Instrumental analysis being the core of Political Economics, I am most anxious to achieve a degree of clarification of its principles which will not only communicate its aims but also demonstrate its concordance with the accepted tenets of the philosophy of science.

<sup>A</sup> See pp. 14-32 and *OEK*, Chaps. 5, 10, 11.

Dr. Nagel asks two questions that go to the heart of the matter. Why should the conventional procedure of scientific inquiry, the hypothetico-deductive method, prove inapplicable in economics, considering its uncontested usefulness in the natural sciences? And second, once we probe the alleged »regressive« procedure of instrumental analysis to the bottom, does it not reveal itself as another version of »progressive« – that is, deductive – analysis?

In trying to give precise answers to these queries I shall also comment on the comparison between instrumental analysis and the technique recently used in constructing so-called »decision models«. Furthermore, I want to enlarge on some earlier remarks concerning the »knowledge-action« issue – namely, my contention that at the present stage of development the object of economic research can no longer be grasped by passive observation alone but must be »created« by political intervention into the actual economic process.

1. The reason that I find the hypothetico-deductive method inapplicable to the solution of the contemporary problems of economics is implied in my above diagnosis of the relevant »facts«. It is that we do not possess any safe hypotheses or major premises from which we could deduce theorems capable of explaining and predicting the processes of industrial capitalism. This is only a formalistic restatement of the substantive assertion that neither the macro-movements of modern markets nor the underlying micro-patterns of behavior exhibit the degree of orderliness that is essential for scientific generalization.

What this amounts to in terms of scientific methodology can be illustrated by drawing some extreme conclusions from a comparison of economic motion with celestial motion. An analog to the physical force of gravity has sometimes been seen in profit-maximizing behavior. But whereas the strength and direction of the force of gravity are *uniquely* and *invariably* described in Newton's formula, no equivalent statement can be made about the actual forces ruling economic motion. Profit-maximization is not the universal incentive in the era of organized capitalism, nor is its effect uniquely determined even where it operates as the dominating action directive. On the contrary, its effect on overt behavior varies with the simultaneous state of expectations, so that the *identical* profit incentive will give rise to *different* responses – meeting a price rise at one time with an increase in supply, and at another time with a decrease. Nor can this

uncertainty of prediction be overcome by inquiring into the determinants of the expectations themselves. For these in turn vary with the technical structure, the size, the financial commitments, and other attributes of the firm. What makes the situation still worse is the fact that expectations cannot be simply correlated with objective states of the environment, but ultimately depend on the manner in which the potential economic actor *interprets* these states and their future changes – a contingency which the student of planets and cells is fortunately spared.

Now Dr. Nagel is certainly right in insisting that in physics the relative strength of different forces also varies from field to field, and that, for example, gravitation is stronger in the solar system than in the atom. It is also true that, contrary to an oversimplified statement of mine, the interplay of several forces is not necessarily summative, but may result in very complex patterns. It is even quite possible that the content of the laws of nature themselves is subject to spatial and temporal variations. However, and this is the salient point, the structural order of these natural forces – though different in physics, electromagnetics, or genetics – is constant *within* any one of these fields, or, if it changes within them, does so at rates that are for all practical purposes negligible.

A logical parallelism would prevail – and from such an analogy Dr. Nagel's queries seem to spring – if, say, nineteenth-century capitalism had displayed *one* ruling type of incentive coupled with *one* type of expectations, while twentieth-century capitalism showed a different but also stable pattern. Though then we might not be able to discover transhistorical laws of economic motion, some laws with a limited historical validity might well be established. But if my diagnosis of the contemporary scene is correct, not one but many patterns of interaction are at work between a wide spectrum of both incentives and expectations, and worse, this continuing situation is without any *ex ante* clue as to which of the possible combinations will emerge in a given situation.

To drive this point home, let me illustrate it by a fictitious example from astronomy. Suppose that on Mars gravitation were to operate inversely with the third power of the distance, whereas on Jupiter it was directly proportional with it. Though we could no longer have a universal mechanics, we could still have a special mechanics for each planet. But now assume that on this earth gravitation were sometimes to operate according to Newton's formula, sometimes inversely with the third power

of the distance, and sometimes directly proportional with the distance, and that we could not know *ex ante* which of these alternatives would materialize at any given time. I wonder what sort of generalizations the hypothetico-deductive method could establish in the field of mechanics then. Under these assumed conditions, a »theory« of mechanics would have to be replaced by taxonomic description. But this is not so in economics where, within limits, we can *create* order out of disorder, once we have made up our minds as to what our macro-goals are. For once we have stipulated them, they can serve as the major data from which we can derive whichever of the many possible forces – behavioral and motivational patterns, public controls – are »orderly«: namely, goal-adequate.

2. The technique for such derivation is instrumental analysis. In discussing its procedure, Dr. Gurwitsch has drawn an interesting parallel with mathematical analysis. In both cases a certain state of affairs is posited – a macro-goal in economics, a geometrical figure with specified properties in mathematics – and the *quaesitum* is the set of conditions upon which the realization of the posited state depends. In both cases, in contrast to the »progressive« technique of the hypothetico-deductive method, the analysis is »regressive« – that is, proceeding from the knowledge of some terminal state back to its unknown determinants.

But is this true? In raising this question Dr. Nagel advances two seemingly grave objections. The first concerns the construction problem in geometry. To solve it he rightly insists that we must know more than the posited state – namely, the specification by a set of axioms of the properties of the respective figures. Must there not also be, he asks, a corresponding set of »axioms« for instrumental analysis? And if so, what else but some known laws of economic behavior can fulfill this function? This leads to his second objection. For if there are such laws after all, why use regressive analysis? Why not proceed by progressive deduction from the knowledge of the initial conditions and those axiomatic laws to the terminal state?

The reply to the first question, which has also been raised with great emphasis by Dr. Machlup, can only be an emphatic »yes«. There is indeed in instrumental analysis an analog to the axioms of mathematics: to wit, the engineering rules that tell us how, within the limits of our technical knowledge, the initial state of the system can be transformed into the goal-

adequate state. But this set of technical operating rules is *all* that is necessary. *We have no need to know any laws of behavior.* I have tried to demonstrate this above<sup>A</sup> through the fiction of a fully automated economy in which human action would be entirely confined to deciding the output menu, the technology to be applied, and the programming of the computers: that is, to goal-setting. All production and distribution processes would »move by themselves«, so that to plan these motions *ex ante* and to understand them *ex post* would require no more than knowledge of the apposite engineering rules and the underlying laws of nature.

The insights imparted by this fiction are also fully valid for our present economic organization in which behavior enters at strategic points. But the patterns of behavior that may be suitable at these points cannot be known before we know the path the system is to follow. Therefore the suitable behavior patterns are themselves among the unknowns of instrumental analysis that are to be derived from the technologically determined path.

To avoid any semantic misunderstanding I should like to clarify the distinction between an engineering rule and a law of behavior by referring to an earlier proposition of mine where I stated that »a rise of investment is a suitable means of promoting employment.« Dr. Nagel interprets this statement as a law of behavior. This would indeed be so if I were to assert that additional investment *will* raise employment. But all I claim there is that investment – that is, building more working places – will *create an opportunity* for more workers to be employed – a technical potentiality for, but no assurance of, subsequent economic action. In purely formal terms, an engineering rule says: If behavior *A* – a rise in investment – occurs, a technical state *B* will follow – namely, more working places. On the other hand, a law of behavior says: If behavior *A* – a rise in investment – occurs, another behavior *C* will follow – namely, more workers will be hired. Nothing about behavioral consequents following behavioral antecedents is pronounced in instrumental analysis, and therefore no laws or empirical generalizations of Positive Economics are implied.

<sup>A</sup> See pp. 24-25.

This provides me with the occasion for a brief comment on the similarities and differences between instrumental analysis and the modern decision models, to which Dr. Kaysen and Dr. Wallich have alluded. There is indeed a formal similarity insofar as both approaches derive the »means« – controls or »instrument variables« – from knowledge of the initial and terminal states by applying certain »structural relations« as constraints. The difference, and it is a fundamental one, concerns the nature of these structural constraints. In the decision models they are, above all, behavior equations, symbolizing the presumed or observed responses of the marketers to specific events. It should be clear by now that in instrumental analysis the structural relations are of a purely technological nature. True, the total set of all known engineering rules is also abstracted from observation, though obtained in the workshop rather than in the market place. But the criteria by which the *suitable* rules are selected, in any given instance, from the total set cannot themselves be »observed«.

3. This now brings us to Dr. Nagel's second question. If it is true that engineering rules are indispensable data for instrumental analysis, why bother with a regressive derivation of the suitable path instead of deducing it in the usual fashion from the knowledge of these rules and of the initial conditions? The answer is simple. Once we *know* which members of the total set of engineering rules are goal-adequate, we can indeed deduce the path in the conventional manner. The first step of instrumental analysis is to provide us with precisely this knowledge.

Thus instrumental analysis reveals itself as a search procedure through which the suitable means to the stipulated end – or, if you will, the suitable causes of a desired effect – are to be discovered. It falls within the category of heuristics or of what Peirce called »abduction«, a mental technique of problem-solving which is part and parcel of research in every field of science. Far from being in methodological conflict with deductive reasoning, it is the technique by which the premises of any deductive syllogism are originally established.

Though they are really the source of all scientific knowledge and are unlikely to be displaced by even the most sophisticated computer, heuristic procedures do not at present constitute a major theme of methodological

discussion.<sup>A</sup> Therefore it is difficult to deal with this aspect of instrumental analysis in abstract terms. This has been one of the reasons why I have supplemented my methodological exposition in *OEK* by a detailed description of some test cases in which the regressive technique is applied to substantive issues.<sup>A</sup>

Closer scrutiny of the manner in which, in these examples, the critical paths of the system are traced back to the pertinent technical rules of production, and these in turn to the macro-goal, should provide sufficient proof that heuristics has a logical and not merely a psychological status. It is quite true that there are no formal precepts whose observance would safely guide us to the solution. Ultimately we must »hit« upon it through what Polanyi calls a logical »leap«.<sup>B</sup> However, it is not a leap in the dark, but one directed by the nature of the problem, and by more or less rigid constraints which set narrow boundaries to the area within which a solution can be found.

We all have heard of Wolfgang Köhler's ape who longingly stares at a banana through the window of his cage, only to discover finally that if he wants to seize it he must move away from the window to the rear, which has an opening to the outside. In an analogous manner, an economic system in which the total capital stock is fully utilized can achieve growth (understood as an *increase* in the aggregate output of consumer goods) only if, to begin with, the current output of such goods is *reduced* so as to set free part of the available capital stock for expansion. There does not exist any technique of inference through which this conclusion could be reached. But the more precisely we circumscribe our problem – a purely logical task – the fewer the number of alternatives which include the solution for which we are searching.

We have now reached the point where my defense merges with Dr. Nagel's charge. Once the heuristic task of instrumental analysis is successfully completed and the goal-adequate forces, private and public,

<sup>A</sup> See, however, the enlightening comments in N. R. Hanson, *Patterns of Discovery* (London: Cambridge University Press, 1958), esp. Chap. IV; and the writings of G. Polya.

<sup>A</sup> See *OEK*, Chap. 11, especially the re-enactment of the »discovery« of the circular nature of an industrial structure of production, on pp. 266-271.

<sup>B</sup> Michael Polanyi, *Personal Knowledge* (Chicago: University of Chicago Press, 1958), p. 123.



discovered, the road is free for deductive generalizations of the conventional type. These generalizations extend the results obtained in the case analyzed to all similar cases. For this reason I have labeled my procedure »instrumental-deductive«, in full awareness that the level of »theory« is reached only when the instrumental findings can serve as highest-level hypotheses in the explanation and prediction of facts other than those which were the occasion for their discovery.

And yet an important reservation is in place. The apparent universality and constancy of forces in the world of nature permits, as a rule, a much wider range of theoretical generalization than is possible in the study of society. In the latter, the multitude of possible macro-goals makes it imperative to re-examine the conditions for suitability whenever a new terminal state is stipulated. It was with good reason that the question repeatedly came up in our discussions as to whether the instrumental-deductive procedure lends itself to the same inclusiveness of theorizing to which we are accustomed in the natural sciences, or whether it yields at best a number of unrelated sets of theorems, any one of which is applicable only to one class of cases.

4. The answer to this question is bound up with what is perhaps the most startling feature of Political Economics – namely, its assertion that only »prior ordering« of reality itself can provide us with a tractable object of theoretical investigation. As will be remembered, I have limited this thesis to the contemporary stage of organized capitalism, claiming that the environmental conditions of competitive capitalism exerted a regularizing influence on the behavioral forces sufficiently strong to render conscious control of their interaction superfluous. But the impression has apparently been created that I regard the new constellation of forces as permanent from now on. Indeed, for purposes of policy-framing, the present generation had better base its analyses on this assumption. But it would be rash to close one's mind to a possible future in which as yet unknown regularities might be discovered underlying the ostensible disorder of market movements, or in which even these surface movements themselves would again assume regular form.

The first alternative concerns an opening of scientific insight into new psychological laws that would permit us, after all, to predict which of the rivaling motivational and behavioral patterns will arise in each instance, and thus to construct an economic theory valid for all conceivable cases.

Even such a scientific advance would not do away with the practical need for the discovery of the means suitable to establish order in the real world, and thus for instrumental analysis. But its findings would then acquire a generality comparable to natural-science hypotheses.

The second alternative is even more interesting. It amounts to speculating about a future state of society in which the anonymous forces making for socialization of behavior become strong enough to bring about a spontaneous ordering of the behavioral field that assures the desired levels of provision. Under such conditions, observation of what actually occurs could, as in the natural sciences, lead to general hypotheses from which verifiable explanations and predictions might be deduced. A structure of this kind seems to prevail in traditionalist societies, though Dr. Lerner has rightly stressed the extent of conscious experimentation with institutions and rules of conduct, which defies any romantic notions about a pre-established harmony of interaction in these societies. Even so, the slow tempo of change allows both actors and observers to take the routinized patterns of stimulus and response for granted over long periods of time.

The image of such a stationary society has little in common with the political and technological dynamics of the modern world. But as victims of the disruptive tendencies of this dynamics, we are apt to underestimate the more temperate, but in the long run not necessarily weaker, forces of conciliation. The collectivist trend of the age may well bring about a new assimilation of incentives, while successive control of the environment may render expectations both more certain and mutually compatible. At the same time the cruder forms of command control seem everywhere on the wane. West and East show, as Dr. Lerner has properly emphasized, a structural convergence toward a type of social organization in which considerable autonomy of the micro-units is combined with order-preserving manipulative controls,<sup>A</sup> made effective through the spontaneous affirmation of the controlled.

5. These speculations about a possible future have a very practical bearing on the immediate present and, especially, on the usefulness of instrumental analysis as a tool for the framing of practical policy.

<sup>A</sup> For the distinction between manipulative and command controls, see *OEK*, Chaps. 5 und 12.

It will, I hope, be accepted by now that the suitability conditions for goal attainment can be determined without regard to the real social forces at work. But logically consistent as such a design is, it describes an imaginary world. To make the transition to reality – that is, to move toward goal-*realization* – the real world must be approximated to the imaginary one through political action. But to do this is possible only if the last step of instrumental analysis has successfully been completed – namely, if a link has been forged between the motivations of the economic actors (incentives and expectations) and the forces of the environment, particularly public control.

As I pointed out before,<sup>A</sup> the forging of this link is obviously not a technological task. It is a problem of social psychology, of determining the social nexus through which specific environmental stimuli evoke *ex ante* determinable responses. So whenever instrumental findings – themselves discovered without reference to any *social* cause-effect relations – are to be applied, we enter a border region in which »laws« or at least empirical regularities of a sociopsychological nature must rule. It is hardly necessary to emphasize once more that the *regularities in question are not the alleged laws of economic behavior postulated by Traditional Economics*. The former are of much wider generality, referring to the effect of environmental stimuli on any type of social response, economic or otherwise. Indeed, one might say that to exist at all, specific laws of economic behavior presuppose the logically prior existence of a lawful order of more comprehensive social relations.

The fact is (and there lies the connection with what was said above about anonymous forces making for socialization of behavior ) that the study of social causation has not as yet come up with safe generalizations.<sup>B</sup> Again I trace the reason for this failure to the state of the research object rather than to shortcomings of the research technique. At least in the so-called »free societies« of the modern world, the responses of randomly chosen individuals to the same environmental stimulus vary widely, as do even those of the same individual at different times. And yet no social organization, large or small, can survive without a minimum of conformity and stability in the motivational and behavioral patterns of its members.

<sup>A</sup> See p. 28.

<sup>B</sup> See p. 29.

Compared with most of the societies of the past and even with the contemporary societies outside of its boundaries, the »free world« appears as an extreme case of »nonconformity«. And yet its members, in their daily performances, continue to »interact«. They succeed in doing so because the psychological heritage from more highly socialized stages of Western history and new agents of the »public interest« sustain coordination.

In any event, it is this looseness of social structure that is mirrored in the vagueness of empirical generalizations, with whose help we try to anticipate the effect of social controls. If it is true that the trend is toward greater conformity, the chances for more accurate prediction of this effect will increase. To demonstrate that even under prevailing conditions these chances can be greatly improved will be the burden of the following remarks.

## **The Function of Public Control**

Public controls suitable to transform real economic states and processes into goal-adequate ones belong to two worlds. Their discovery is the final step of instrumental analysis and is thus part of the theory of Political Economics. Their application is a political act – the foremost practical task of Political Economics. But to make the theoretical findings amenable to practical application, the level of abstraction of the analysis must be lowered to the point where the general principles of controls can be specified in concrete measures of economic policy, taking into account the intangibles as well as the tangibles of the prevailing socio-political structure. This is an undertaking that calls for talents and experiences of which Dr. Kaysen's paper presents a rare display, but which are not the ordinary equipment of the economic theorist. So it should not cause surprise that both my book and my position paper in this volume show considerable gaps in this respect which I cannot even try to bridge in these summary remarks. Their purpose is rather to spell out in greater detail some of those general principles which underlie any specification of public controls.

1. In my programmatic statement of the major propositions of Political Economics,<sup>A</sup> I have distinguished between primary and secondary controls. My first concern is to give this distinction precision beyond the cursory comments made earlier.<sup>B</sup> There I have subsumed all conventional measures of economic policy – taxation, tariffs and quotas, currency control and interest manipulation, social legislation, etc., as well as the techniques of monetary and fiscal controls as advocated by the new economics, under the concept of primary control. In stressing their inadequacy for goal attainment, I did not mean to imply that Political Economics could in any way dispense with such controls. The contrary is true, and only in the context of these controls does the true problem emerge.

This problem was labeled above as the »response mechanism« of the micro-units of the system – that is, their reaction either to actions of other micro-units or to primary controls imposed by public authorities. Because of the unrealistic assumptions concerning the motivational and behavioral patterns ruling in a modern industrial market, traditional theory and policy alike take the responses to such stimuli for granted. Concretely, the supposition is that tax reductions will *always* add to aggregate spending whereas tax increases *necessarily* reduce it, that public spending *always* raises aggregate employment, or that increasing labor supply *invariably* stimulates private investment, etc. It is against such a mechanical interpretation of the effects of primary or conventional controls that Political Economics argues. To put it in more practical terms, these controls are not to be replaced, but are to be supplemented by another type of control which assures that the intent which led to the introduction of the former controls is realized.

It is the function of *secondary* controls to bring this about by eliciting the goal-adequate behavior of the controlled. In speaking of »eliciting«, I want to make it clear that we are dealing with a social and not a physical phenomenon, with a challenge that can be accepted or rejected. True, there are certain types of command control which, by threats to life and liberty, may perhaps evoke a pseudomechanical reaction. But, as a rule, responses to controls will be goal-adequate only if the controlled

<sup>A</sup> See pp. 168-170.

<sup>B</sup> See pp. 32-34.

*understand* and *affirm* both the macro-goals pursued and the policy instruments used in their service. This is even true in the case of »restrictive« controls such as taxation or quotas on imports. These erect boundaries which the micro-units cannot overstep. But where actual behavior will settle within these boundaries cannot be predicted *ex ante* unless the responses themselves are controlled. To promote such understanding and affirmation, and thus to give the impact of primary controls on micro-behavior direction and strength, this defines the role of secondary controls.

What then are the concrete measures of public policy which promise to improve this causal nexus? It was admitted earlier that we have at this stage no sociopsychological laws to guide us in this inquiry, and that we must rely on certain empirical generalizations and rules of thumb. Generally speaking, techniques of secondary control lie between two extremes. At one extreme there is the ideal but exceptional case in which the stipulated macro-goals and the primary controls chosen for their realization coincide from the outset with the aspirations of the controlled. Spontaneous micro-behavior is then intrinsically goal-adequate, and no secondary controls are required. At the other extreme, we find the situation already alluded to in which, for lack of understanding of or radical disagreement with the macro-goals or the primary controls applied, the micro-units act obstructively. There secondary controls are indispensable, assuming the form of circumventing obstructive behavior through compensatory public action, or coercion, or finally, by altogether supplanting private decision-making in a collectivist regime.

The secondary controls appropriate to the mixed systems of the West, which are committed to maintaining a broad sector of private decision-making, lie between these extremes. Their purpose is to »convert« the micro-units to the realization that both macro-goals and controls coincide with their own long-term interests. In other words, they treat private decision-making as open to a *learning process*.

I have shown earlier that, in particular, marketers' expectations are a highly promising object for goal-adequate restructuring through improved public information, in which the instrumental analysis of the given situation plays a major role.<sup>A</sup> Moreover, I could point to numerous

<sup>A</sup> See p. 29.

instances in which education in the wider sense of the word, including the formation of a more enlightened public opinion, has profoundly affected the attitude of business and of the community at large toward policies that initially were hotly contested. Recent examples are the universal acceptance of collective bargaining, and of public spending as a means to counteract recessions. Since Dr. Kaysen has placed such emphasis on the role of the economist as public adviser, I should like to stress his function as teacher and educator, a more subtle but over the long run even more effective instrument of secondary control.

How severe secondary controls should be in any given instance may be difficult to gauge from the outset. Incremental application of the respective primary controls can offer an important clue. If, for instance, in a state of depression, small doses of public spending lead to a rapid rise in private investment, the presumption is that the response mechanism in the private sector is goal-adequate. Conversely, as happened during the 1930's, a negative response of private investment demonstrates the need for supplementary secondary controls, a course of action which was not comprehended at the time.

2. From improved information, the enlightening of public opinion, and effective teaching through the numerous techniques of »persuasion« such as guideposts or indicative planning, to compensatory public intervention and finally outright coercion – the arsenal of secondary controls is large indeed. Hence it is not surprising that again and again in our discussions an anxious question was raised asking to what extent these weapons are compatible with decentralized decision-making. More than one voice exercised the fear that once they have such means at their disposal the controlling authorities will be tempted into collectivist adventures.

It cannot be denied that there is always the danger that the insolence of office will grow with the strength of the powers that be. But the real problem, one that goes beyond bureaucratic ambition and misuse of political authority, has different roots. Whether controls, primary or secondary, will in fact be harsh or lenient is not determined first and foremost by the caprices of the controllers. It is, rather, a functional problem in which the nature of the macro-goal and of the initial state plays the dominant role. As far as the latter is concerned, the prevailing social and technical structure of a society is a rigid constraint for the adjustment

processes that connect initial and terminal states.<sup>A</sup> From this it follows that certain goals are incompatible with the maintenance of the structure of the initial state, and can be attained only if this structure itself is altered. To give an example, an egalitarian distribution of income and wealth is not a feasible goal within a capitalist order of property relations. It can be accomplished only after these relations are abolished. This, however, would require the most extreme forms of command control.

Of greater practical importance at this historical juncture are goals, such as radical urban renewal, that in principle fit into the prevailing social structure, but run counter to the interests of powerful strata of society. These goals can be successfully pursued only if resistance can be circumvented, say, by attractive forms of compensation, or can be broken by more direct means. The primary controls required for this certainly lie outside the range of the manipulative controls of the new economics or of other conventional policies, and the secondary controls can hardly confine themselves to applying persuasion.

What we run up against here once more is the difference between »settled« and »live« issues. But now the context is much wider, including the popular attitude not only toward the goal itself, but also toward the means required for its attainment. In this wider context we can now define that difference more precisely. An issue is »settled« when the goal in question and the primary controls associated with it are approved of by a politically relevant majority. It is obvious that in all such cases any secondary controls, if required at all, can be of the lenient kind. It is our good fortune that, as was mentioned earlier, the major order-preserving goals of the present age fall in this category; it can be shown that purely manipulative controls are very likely to assure their realization.<sup>A</sup>

This is by no means true of what I called »ameliorative« goals, most of which are »live« issues – that is, the fighting concern of pioneering minorities. So long as they have not conquered public opinion, such goals can be accomplished only if the sponsoring minority succeeds in imposing its will on an antagonistic majority. If it were only a question of »extremist« groups pursuing sectional aims, the answer would not be too difficult, even in a democratic system. Alas, more often than not such dissent

<sup>A</sup> *OEK*, Chap. 10.

<sup>A</sup> *OEK*, Chap. 11.



reveals a serious dilemma. It arises wherever an enlightened minority perceives as a long-term necessity what to a majority blinded by short-term concerns appears as a violation of its interests. In other words, the distinction between order-preserving and ameliorative goals is historically fluid, especially when we remember that our sensitivity to »disorder« increases steadily. Under this aspect it is an open question whether stabilization and balanced growth still fulfill the minimum conditions of satisfactory provision, as I suggested earlier. Urban renewal, economic equality of opportunity among the races, pure air, clean water, and, last but not least, greater distributional equity are today proclaimed as preconditions for social survival by vocal minorities supported by serious experts, in addition to such »international« goals as population control and prevention of worldwide famine. If it is true that our very physical existence is threatened by shortsighted interference with ecological equilibria, can we wait with remedial action for a political consensus to be achieved through the democratic techniques of persuasion, or must such issues be »settled« by other means?

In placing these alternatives before us I am raising grave questions of a constitutional nature. I have never cherished any illusions about the efficacy of lenient (manipulative) controls in underdeveloped societies that are striving for emancipation from the tyranny not only of nature but of oppressive rulers, domestic and foreign. But in writing my book I was perhaps too optimistic in relying on the social consensus prevailing in the mature societies of the West as a safe basis on which Political Economics can build. The symptoms multiply that the mere attempt at preserving our accomplishments for future generations will involve us in social conflicts for the resolution of which many of our present institutions may have to be restructured.

However, we should be aware that in attributing to certain ameliorative goals the function of »preserving order«, we have extended the notion of »order« far beyond the original definition we gave it. It is now no longer a question of regularizing »economic motion« and thereby assuring predictability of the system. Rather, the aspect of »satisfactory provision«, which was then stressed only as a complementary constituent of order, now takes over, and Political Economics reveals itself as the instrument for the discovery and application of means suitable to the attainment of whatever goal our welfare judgments regard as worthy of pursuit.

## Criteria for Goal-Setting

This wider scope of application is implied in the very concept of a Political Economics which tries to supplant what Dr. Lerner calls the economic »process of natural selection« by goal-oriented planning. But it brings to the fore an issue that we could evade as long as we assumed that the stipulated goals were accepted by a more or less unanimous public opinion. Stipulation was then little more than articulation of popular aspirations, and coincided with the legalization of these aspirations in the framework of prevailing political institutions. This is no longer so when popular consensus gives way to dissent over what the »order-preserving« goals are in a given situation. The alternative to a struggle of brute force can then only be an appeal to criteria which, as the ultimate vindication of political action, are themselves above the power struggle.

There lies the crucial significance of Dr. Jonas' radical assault of the agnostic position I have taken in all my writings as to whether there are any *scientific* criteria to guide our choice of goals.<sup>A</sup> This is the position conventionally labeled as »scientific value relativism«, of which the work of Max Weber contains the paradigmatic formulation. It is important to stress that the relativism proclaimed there confines itself to what scientific inquiry or discursive thinking generally can contribute to establishing »intersubjective demonstrability« of norms. It remains open, and this is an important proviso, whether or not the choice among values and norms is yet amenable to a cognitive judgment, but in a realm in which we can communicate only by »pointing to«, as opposed to the realm of thought where propositions rule – a distinction which, in the words of Wittgenstein, is »the cardinal problem of philosophy«.<sup>B</sup>

Now what Dr. Jonas tries to do, and what to my knowledge has not been attempted in any other philosophical disquisition about economics, is to demonstrate that the essential criteria for goal selection can be explicated by rational analysis because they are intrinsic to the nature of economic organization as such. Of course, even if such a scientific explication of the »true« goals proved possible, this by itself could not

<sup>A</sup> See *OEK*, Chap. 12, and pp. 18, 34.

<sup>B</sup> In a letter to Bertrand Russell. See *Autobiography of Bertrand Russell*, Vol. II (London: George Allen & Unwin, 1968), p. 172.

assure their acceptance as rules of political action. But it would certainly elevate political debate above the mere airing of »opinions« and »subjective judgments«, and would provide us with a foundation for a self-contained science of Political Economics.

In my subsequent remarks, I shall have to raise a number of serious objections to Dr. Jonas' demonstration. But I would like it to be understood that, even if he has not spoken the last word, he has addressed himself to the fundamental problem underlying all social research in an age which tries to transform the historical process from blind motion into responsible action.

1. I will begin by briefly restating Dr. Jonas' major propositions: (a) Rather than being stipulated from outside the economic field, a definite goal-commitment is an indispensable condition for constituting this field. (b) This is so because economics deals with human institutions which cannot even be defined unless we include a »causality of purpose«. (c) The intrinsic goals in which this basic commitment manifests itself are two, *provisioning* and *providence* – namely, providing the members of a group »with the physical goods necessary to sustain their lives« and, in doing so, »looking and planning ahead«. These basic economic goals are themselves ultimately grounded in two biological constituents: metabolism and reproduction; they express the »basic self-affirmation of life«, »an *a priori* option«. (d) No extrinsic criteria are required to vindicate these basic goals. Rather they offer themselves as the criteria for the choice of more specific ones, at least by setting boundaries which separate legitimate goals from others. This yields us an »unconditional economic imperative«: »Do not compromise the conditions for an indefinite continuation of some viable economy.«

In trying to comment on these propositions, I find myself in a strange quandary. I am in complete agreement with Dr. Jonas' conclusion as stated in his economic imperative and, as a consequence, also with his counsel of caution when it comes to pursuing grandiose projects. But I cannot accept some of the premises from which this conclusion is derived, especially those which concern the intrinsic nature of provisioning and providence, as understood by Dr. Jonas, and the treatment of the basic commitment as an »*a priori* option«. Moreover, even if the economic imperative is accepted unreservedly, in telling us only what *not* to do, it fails to offer guidance for our positive decisions within the permitted range.

2. To vindicate my second and less fundamental objection, let us consider some of the topical choices among rivaling goals with which contemporary policy-framers are confronted. Is it »better« to reduce unemployment to the zero level even if this implies an actual price rise by  $x$  per cent to be borne by the recipients of fixed incomes – or to maintain price stability even if this will keep unemployment above the  $y$  per cent level? Or, as another example, »should« a developing country keep consumption near the subsistence level to facilitate investment and a rapid rate of growth, or »should« the present generation be favored with a rising standard of living at the cost of reducing the gains of future generations?

Obviously either of these decisions is covered by Dr. Jonas' viability norm, some provision and some providence being assured in each case. But how are we to evaluate the relative advantages which alternatively accrue to different income groups and different generations? True, my examples do not refer to the category of global goals with which Dr. Jonas is primarily concerned, but belong to what he calls »measured alternatives of short-range planning«. But as a matter of fact, the overwhelming majority of goal-choices that arise in Political Economics fall in the latter category, for which both the goals and the means can be spelled out with reasonable precision.

However, the same dilemma confronts our decisions on »long-range, large-scale perspectives«. We agree that the new opportunities for communal choices and also the dangers implied in a wrong choice both derive from the same factor: rapid technological change. What then should our attitude be toward further technical progress? The general norm that states: »Do not endanger economic viability«, yields no guidance. Perhaps technical progress should be stopped altogether in the interest of safeguarding »human wholeness« – a »value« which, according to Dr. Jonas, should form »a legitimate part of hardheaded economic reasoning«, making »viability ... rather a comprehensive concept in which the technical aspect ... tends to merge with the humanistic aspect of man's well-being«. <sup>A</sup> But how can we decide on this unless we have an image of man in which we can read what human wholeness and the humanistic aspects of his well-being are?

<sup>A</sup> See p. 82.

3. This question leads us to the fundamental issue of where to look for criteria – not only to help us choose among rivaling specific goals, but also to give precision to those basic goals: provisioning and providence. I agree with Dr. Jonas that the overall purpose which constitutes economic activity can be formulated in these concepts. But I must disagree with his claim that these concepts provide us with any criteria other than the *successful functioning* of economic activity. And »success« is measured here as the ability to make the »best« of an altogether bad job, that of allocating scarce human and natural resources to our wants, where »best« means »most efficient«, irrespective of any »humanistic commitments«.

To see this clearly we must disabuse ourselves of the widely held notion that economic activity can be placed side by side with activities such as politics, science, or religion that pursue intrinsic *substantive* goals. When Dr. Jonas speaks of provisioning as being concerned with supplying the physical goods necessary to sustain our lives, he comes dangerously near to the notion of there being a special type of wants called »economic« or »material« that concern *vital necessities*: food, clothing, housing, etc. In reality there are no particular wants that can justifiably be labeled »economic«. There is only an *economic manner or technique* by which we provide for the satisfaction of *any* wants – vital, political, religious, etc. – a technique that comes into play whenever satisfaction is conditional on the application of scarce resources. Therefore, the construction of a church or – *sit venia verbo* – of the gas ovens of Auschwitz poses no less an economic problem than does the production of bread und shoes. Or even more pointedly: economic activity is not at all concerned with the actual satisfaction of any particular wants, but with overcoming the resistance a stingy Nature opposes to the satisfaction of all means-requiring wants. Thus economics deals exclusively with the realm of means, and is as such both narrower and wider than the other realms of human action: narrower because it is bare of any substantive goal, wider because it is subservient to all the other realms insofar as they require means.

Now what difference does it make whether we delineate the realm of economics as standing on a par with politics, science, or religion, or as a subsidiary and auxiliary realm of means disposal? The difference is far-reaching indeed, because only in the latter conception does economic action reveal its historical relativity. All the realms pursuing substantive goals – vital, interpersonal, political, moral, etc. – are likely to remain

fields of action as long as man walks this earth. They sustain, to speak with a physical analogy, the »voltage« of civilization – namely, the positive forces of human society. Not so economic relations. They are the »ohms« of civilization, measuring the resistance of a stingy Nature to the fulfillment of our positive goals. They symbolize Adam's curse which, all through past history, has compelled men to sacrifice the potentialities of the »good life« to the toil and trouble of procuring the means necessary for our most primitive – namely, vital – ends. But contrary to the Biblical prediction, technology is gradually emancipating us from this bondage, by progressively reducing the obstacles to means procurement. At least asymptotically we are moving toward a state in which the significance of economic activity dwindles relative to the opportunities for pursuing genuine goals.

But so long as it is with us, economic activity as such is *goal-neutral*. The only imperative that can be derived from its intrinsic character commands us to apply the available resources as efficiently as possible to any extrinsically posited end. Even provision, in the sense of that which is to be provided, and providence, understood as the time span over which we are to provide, remain empty boxes unless they are related to a particular »menu« stipulated from without. »Indefinite continuation of a viable economy« may be an item in that menu, but need not be. If Hitler had decided in 1945 to bring about the final *Götterdämmerung*, the complete destruction of the German people and land, then the task of the economist *qua* economist, unmoved by extrinsic considerations, would have been to help in doing so most efficiently.

4. Now in insisting that economics can provide us only with a functional criterion, I am far from proclaiming that in the economic realm »everything is permitted«. But when I speak out against *autos-da-fé* and other destructive uses of »means«, calling them »mala« in accord with Dr. Jonas, I transcend the intrinsic neutrality of economics by an appeal to extrinsic moral norms. As a matter of fact, such transcendence begins already when I try to define what »legitimate« wants are. True, on the primitive subsistence level, »choices« and thus normative decisions are in practice almost excluded, because vital needs claim all the available resources. I say »almost«, because even there, »affirmation of life« and »interest in being« remain a genuine »option« and, contrary to Dr. Jonas, are no *a priori* of economic action. But certainly above that level, the

opportunities for choices steadily expand, and with them the need for criteria beyond the functional command of efficiency.

As a matter of historical fact, these criteria are rooted in the dominant system of cultural values, which determines the legitimate range not only of wants but also of means: to a pious Jew pork is no food. Thus only by surrendering its autonomy in favor of the rule of such extrinsic moral, aesthetic, and other values in which »human wholeness« comes to fruition, will economic activity remain within the boundaries of what is »constructive«, and will economics be able to fuse its criterion of technical efficiency with the »humanistic aspect of man's well-being«.

At the same time it cannot be the business of economics and its adepts to pronounce on these values. In spite of Dr. Jonas' modest disclaimer, this is a philosophical task, more precisely one of philosophical anthropology. Practically, if not in principle, the services might be dispensed with if our age were dominated by one and only one cultural value system. Here our present discussion merges with our prior reflections on the conflict of rivaling macro-goals in Western public opinion, not to speak of the ideological conflict between East and West or between traditional and modern societies generally. Seen in this light even terms such as »humanistic aspect of man's well-being« lose precision because they point to a very singular image of man, as it has been formed through the blending of the classical heritage with the Judaeo-Christian tradition. I sympathize with Dr. Jonas' fear and trembling when confronted with such a Promethean task – but to whom else if not to the philosopher can we appeal in our search for a »just« solution of these conflicts?

5. In conclusion, a word must be said about another objection which Dr. Jonas has raised. It concerns the feasibility of instrumental analysis, an objection which, if sustainable, would be truly crushing. Optimistic as he is when the establishment of ultimate criteria for goal-setting is at stake, he turns into a radical skeptic when these criteria are to be applied to spell out the concrete features of the terminal state in which the stipulated goal is to materialize, and also of the intermediate stages which represent the suitable path. This skepticism arises from our inability to foresee the long-term effects of any action, because any terminus projected today is »spotlighted ... out of a darkness of collateral unknowns with which it is inextricably intertwined.« Therefore, »in the last resort, the directed and

›controlled‹ alternative is cognitively little better off than the ›automatically self-realizing one.«<sup>A</sup>

It may be useful to emphasize that Dr. Jonas' reservations to instrumental analysis are different from and more radical than Dr. Machlup's stress of our ›ignorance of what bridges may be crossed‹ in the course of carrying out our instrumentally established policies. What bothers Dr. Machlup is not our inability to foresee the long-term effects of our present actions, but the multitude of alternative paths and behavioral patterns through which a stipulated goal can be reached, and between which the choice can only be made on the basis of a value judgment. In other words, Dr. Machlup's problem is the abundance rather than the dearth of our cognitive findings, to be solved by stepping over the boundaries of positive economics into the realm of what he calls ›normative economics‹. Though, as should be clear by now, I regard a normative economics in which values can be ›justified‹ as a scientific mirage, I fully agree with Dr. Machlup that we must transcend the realms of positive economics or of instrumental analysis if we are to find criteria for such choices.

Dr. Jonas, on the other hand, stops short from the very outset, because he denies the feasibility of any cognitive propositions on means. Fortunately, he confines his skeptical reasoning to ›long-range, large-scale perspectives‹, and thus opens the way for Political Economics after all. This is so because the collateral unknowns of a distant future play a minor role in the short-range projects with which Political Economics is mainly concerned. Certainly stabilization and balanced growth, our paradigms of order-preserving goals, do not refer to a Utopian future, but point to an ongoing struggle against ongoing threats of economic dislocation. And the darkness in which even the proximate future may be shrouded can be lightened by the trial-and-error technique of incremental control. All this has been stated very clearly and convincingly in Dr. Edel's comments, with which I fully associate myself.

I should even go farther and assert that the typical goals that Political Economics stipulates and the typical measures it advocates are in strict conformity with Dr. Jonas' cautionary warning, because they are all in the service of viability, which is only another term for what was defined above

<sup>A</sup> See p. 70.



as »order« in the comprehensive sense. On this level it would be a gross misreading of the facts were we to place the »automatically self-realizing alternatives« on the same footing with the planned ones. As the experience of the Great Depression has demonstrated only too forcefully, the former alternative threatens us with the very destruction of economic viability. Even if political control is, and to some degree will always remain, an imperfect tool, it would be a surrender to a negative eschatology were we to prefer the risks of »natural selection«, a view which again seems to accord with Dr. Edel's position.

But I must not end in this critical vein. As I said at the outset, Dr. Jonas' paper has opened a debate which is bound to challenge both economists and philosophers for some time to come. I admit that, with all his concern about the intrinsicities of economics, I still see him wearing the philosopher's crown rather than the bowler hat of the economist. But when he tells us that economics is »interdisciplinary by its nature«,<sup>A</sup> dealing with an indivisibly »compound situation« in which the physicist, the biologist, the anthropologist, the psychologist, etc., are also involved, I begin to wonder whether for him that crown and that hat are not really the same. I wrote some time ago that for the solution of its basic problems, economics is in dire need of another Aristotle,<sup>B</sup> meaning a philosopher sufficiently at home with the economic issues of his time to be able to provide it with its ultimate norms. Though for the time being Jonas' answer is not Aristotle's, he has retrieved Aristotle's quest.

<sup>A</sup> See p. 83.

<sup>B</sup> »The Normative Roots of Economic Values«, *Human Values and Economic Policy*, ed. Sidney Hook (New York: New York University Press, 1967), pp. 170-180.

**[Verweis auf:**

**3. Ohne Opferbereitschaft gibt es wenig  
Hoffnung**

**Gespräch mit Christine Claussen und Heinrich  
Jaenecke, 1988]**

[...]

*Siehe den editorischen Hinweis auf Seite 52.*

## 4. Editorische Hinweise

### 1. Abraham Edel: Ends, Commitments, and the Place of Ignorance

*Dieser Aufsatz findet sich in: R. L. Heilbroner (Hg.), Economic Means and Social Ends. Essays in Political Economics, Englewood Cliffs, New Jersey 1969, S. 89-97.*

*Der genannte Sammelband enthält Beiträge aus zwei Symposien von Sozialwissenschaftlern und Philosophen zu Ehren von Adolph Lowe, Professor Emeritus der Graduate Faculty der New School of Social Research. Die Symposien fanden im Februar und März 1968 an der New School for Social Research statt und thematisierten Adolph Lowes Buch On Economic Knowledge.*

*Da der Verlag Prentice-Hall nicht mehr existiert und sein Nachfolger Pearson das Buch nicht mehr führt, wir auch keine Erben von Abraham Edel ausfindig machen konnten, geben wir den Text hier wieder. (Sollte es gleichwohl noch einen Rechteinhaber geben, bitten wir diesen um Kontaktaufnahme.)*

### 2. Adolph Lowe: Economic Means and Social Ends. A Rejoinder

*Der Text ist publiziert in: R. L. Heilbroner (Hg.), Economic Means and Social Ends. Essays in Political Economics, Englewood Cliffs, New Jersey 1969, S. 167-199.*

*Der Band versammelt Beiträge aus zwei Symposien von Sozialwissenschaftlern und Philosophen, die im Februar und März 1968 an der New School for Social Research stattfanden und der Diskussion von Adolph Lowes Thesen aus seinem Buch On Economic Knowledge gewidmet waren.*

*Da der Verlag Prentice-Hall nicht mehr existiert, sein Nachfolger Pearson das Buch nicht mehr führt und sich auch keine Erben von Adolph Lowe bzw. andere Rechtsnachfolger ausfindig machen ließen, geben wir den Text hier wieder. (Sollte es gleichwohl noch einen Rechteinhaber geben, bitten wir diesen um Kontaktaufnahme.)*

### 3. Ohne Opferbereitschaft gibt es wenig Hoffnung

*Dieses Gespräch mit den beiden Redakteuren des STERN Christine Claussen und Heinrich Jaenecke wurde 1988 geführt und zuerst abgedruckt in: STERN, 23.6.1988. Das Gespräch wurde wiederveröffentlicht in: H. Jonas, Dem bösen Ende näher. Gespräche über das Verhältnis des Menschen zur Natur, Frankfurt a. M. 1993, S. 79-83.*

*Der STERN hat uns, und zwar mit Schreiben der Picture Press Bild- und Textagentur GmbH vom 20. Januar 2017, leider die Veröffentlichungsgenehmigung für das Interview verweigert. In dem genannten Schreiben wurde dafür folgende Begründung gegeben: »Der Stern legt Wert darauf, dass Inhalte aus Stern auch nur auf den Internet-Präsenzen des Verlags Gruner + Jahr erscheinen.«*

*Dort ist das Interview bislang jedoch nicht zu finden, so daß wir leider nur auf die beiden gedruckten Veröffentlichungen des Textes verweisen können.*