Jean-Jacques Salomon

SCIENCE POLICY AND ITS MYTHS

The destiny of our age is rationalisation, intellectualisation and, above all, a disenchantment with the world.[1] The success of science as a technique of forecasting has broken a spell; there is no longer a complete image of the world that can be revealed by rational knowledge. Scientific activity is no royal road to the gods, nor to the essence of the world, nor to the truth of nature lying hidden behind its outward appearances; the spell of mysterious powers to be discovered or invoked has died away with the multiplication and the success of objective, neutral, purely technical instruments for the mastery of natural phenomena. If science robs the world of enchantment, it is because it offers only instrumental answers to the questions we ask of it.

But science itself becomes involved in this process of disenchantment. One technique among many others, it cannot escape the measures which have enabled it to rationalise nature as quantifiable and foreseeable matter, the subject of control and organisation. Just as the scientific project tends to reduce uncertainty, it must itself submit to the mathematical apparatus which is to reduce the uncertainties of research work or institute some coherence among its different directions. The time has gone when Benjamin Franklin could retort to the sceptical witnesses of an early balloon flight by asking "What is the use of a new-born baby?" Science no longer has to justify its researches, it has given proof of its utility—it has produced the results. But perhaps it has proved too much; the new-born child

[1] See Max Weber, "La vocation du Savant," in *Le Savant et le Politique*, Paris, Plon, 1959, pp. 105-106.

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is looked at only in the light of the productive function it will fulfil as a grown man and the care lavished upon it is motivated only by the hope of quick and easy rewards in the shape of results.

If the political authorities are concerned with knowledge, its sources and genesis, it is purely from the angle of extent; intervening by capital investments designed to increase skilled manpower, equipment and research potential, they are interested less in the progress of knowledge for its own sake than in the goods by which that progress is translated into new or more powerful instruments of action. It is the last stage of the research process that they have in mind, when the discovery or the invention is exploited and becomes an innovation, and is given reality in the products or processes which are added to the arsenal of means of production or of destruction. The scientific project is kith and kin to the mode of thought and action which conceives that the aims of society can be expressed in economic language in terms of the resources placed at society's disposal to achieve them. Efficiency, productivity, maximisation, are the frame of reference by which the research system must justify itself to society. When the resources assigned to Research and Development reach two per cent of the gross national product or more (in the United States and the USSR, more than three per cent), the individual adventure of research quite obviously involves collective decisions; the scientist is no longer answerable only to his peers or to scientific truth, he must plead before the tribunal of the community, which determines budget appropriations and controls the use made of them. Thus, the research system itself is no more than a sub-system of the network of economic relations over which the State exerts its authority. Even if the "goods and services" which it provides do not fit neatly into the classical definitions of national accounting, the investments from which it benefits are essentially a matter of public accounts.[2]

[2] "The national accounts only show goods and services which are effectively exchanged or capable of being exchanged, on the market." (*Comptes de la Nation*, Paris, Ministère des Finances, 1960, Vol. 2, *Méthodes*, p. 150). The American definition is not very different: "The fundamental criterion used to define an activity as economic production is that which is reflected in the transactions of buying and selling on the market." (*National Income Supplement 1954 to the Survey of Current Business*, Washington, p. 30).

It is intellectually tempting—it is the whole temptation of rationalism—to imagine that the ways in which the political authorities concern themselves with science are themselves scientific, that science, in its turn, governs the methods of the support given to knowledge whose progress depends more and more closely on the State, that rational technical action determines the procedures and the aims whereby the adventure of discovery is embodied in a collective institution. If the demand for rational management is one of the characteristics of modern societies, this demand will seem all the more legitimate when it relates to a social system whose men, values and institutions are, by definition, dedicated to reason.

Yet science policy only creates myths when it confuses in this way the technical representation of knowledge with the human scene of power; the universe of scientific discourse does not make the political discourse applied to science any more rational than any other political discourse. Myths or mystifications of the technocratic ideology of management? Some of the most frequently recurring themes of science policy discussions—planning, forecasting, the criteria of choice—might create the impression that the will to rationalise which is peculiar to the government of modern societies depends for its realisation only on the perfection of knowledge and techniques. It is not that, as Marcuse would understand it, the triumph of positive thinking in this case makes the rational support the irrational; there is not, at this level, any turning of rationality into its opposite,[3] but the demand for an accomplished rationality, accessible or operational in its very accomplishment—in other words, scientism in its most traditional sense. The limits and deficiencies of decisions are attributable to defects in our information and instruments of action, not to the nature of things, of men and of society; if we cannot do more today, it is because we do not know enough; tomorrow, management techniques will have progressed to such a point that we shall be able to master the most complex systems and storm the last redoubt of the unforeseeable... However, it is enough to review some of these themes and the way in which they have aroused the hope of

[3] Herbert Marcuse, *One-Dimensional Man*, Boston, Mass., Beacon Press, 1966, especially Chapter 7.

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a "political technology" of science, to realise how far the research system, even regarded as a productive force among many others, is still recalcitrant to efforts to rationalise it.

PLANNING AND PROGRAMMING

It is idle to ask whether science can be planned or not: the question is purely rhetorical, as Harvey Brooks said, since "science *is* planned, whether implicitly and largely as a by-product of decision-making processes external to science, or explicitly and consciously."[4] From the moment that the State steps in as the main supporter of research, the allocation of appropriations among disciplines, projects and researchers reflects an orientation which is not based solely on scientific skills. Theoretically, science planning aims at "the best adjustment between the need of science for internal autonomy and the desire of society for the fruits of science."[5] But, in the first place, scientific research is only one dimension of planning and, in the second place, the aspirations of the research system must take account of the aims pursued by the State, that is to say, of criteria which relate not to science itself, but to its applications.

In the best of all possible worlds, even plenty would not do away with the problem of choice. Between the competing demands of different sectors of activity, public support is determined not only by the amount of resources available, but depends also on the orientations desired or imposed by the economic situation. The dilemma of Buridan's ass is not a government problem; one way or another decisions are taken, even if it is only a decision not to decide. The aims of the State should, so far as possible, be mutually consistent, but they are no more homogeneous than the needs they are designed to meet. Defense, transport, social security, education, scientific research, and so forth, all define heterogeneous and incommensurable demands, whose pressure on the public budget is uneven, varying according to circumstances and the short term targets set. Furthermore, within each of these sectors, there is an open option between

[4] Harvey Brooks, "Can Science be Planned," *Problems of Science Policy*, OECD, Paris, 1964, pp. 97.

[5] *Ibid.*, p. 97.

alternatives which are no less heterogeneous and incommensurable. It is impossible to do everything at once, or for the same reasons, since at any given moment the resources are always less than the needs which emerge. What passes for consistency in a policy is never more than a precarious balance between widely differing needs.

The search for such a consistency characterises the economic administration on a large scale which, as much in the public sector as in the private, has not ceased to widen in the post-war period. It shows itself in the most evident way in the long-term planning.[6] Whatever the differences in planning institutions, techniques and styles among different countries, planning everywhere is designed "to reduce the realm of the unforeseeable to a series of clear and manageable options."[7] A "calculated risk, a fail-safe device," in the words of Pierre Massé ("*aventure calculée, anti-hasard*"),[8] planning holds itself out, in every case, whether it be imperative or indicative, flexible or inflexible, as the instrument of rationally formulated choices. And yet, the rationality of planning experiments is never established—if at all—until after the event.

At the level of a business firm, it is relatively easy to query the object of the costs to be incurred, to set priorities among targets and to acquire the means of achieving them; the cost of the various methods envisaged and the estimate of the possible return are circumscribed within the micro-economic area of the exclusive interests of the firm. Similar methods may even be applied to the choices which must be made by a specialised Ministry, as witness the success of the "functional" budget techniques introduced by the United States Defense Department. These methods make it perfectly feasible to compare the relative cost and benefits of different programmes designed to achieve the same end.[9] But as soon as one tries to generalise this management

[6] Andrew Shonfield, *Modern Capitalism*, Oxford University Press, (Translated from the French version)

[7] *Ibid.*

[8] Pierre Massé, *Le Plan ou l'Anti-Hasard*, Paris, Gallimard, 1965.

[9] See in particular David Novick, *Program Budgeting: Program Analysis and the Federal Budget*, Boston, Harvard University Press, 1965. *The Planning-Programming-Budgeting System* (PPBS) is now being applied in France under the name of "Rationalisation des choix budgétaires" (RCB).

technique by comparing the advantages of programmes designed for different ends, the choice comes up against the limits inherent in all decisions relating to the macro-economic area. Not only does the calculation lose in strictness by introducing a constantly growing number of variables, but above all, it meets an insuperable limit; you cannot measure the incommensurate.

A public authority, such as the French Electricity Board, EDF, may try to analyse with mathematical precision the cost and benefits of a specific type of power station, thermal, hydro-electric or nuclear, or the French National Railways, SNCF, the cost and benefits of various types of line, even calculating their marginal return and the financial incidence of variations in fares and freight rates as a result of road competition. So long as we remain in this way within the limits of allied activities, connected with a common purpose, the search for consistency is all the more likely to succeed, since it is limited in practice to defining the numerical incidences which it is the whole object of planning to maximise. It is not surprising that these budgeting techniques should have developed as a consequence of strategic arms programmes, where it is possible, for a given objective, to translate into formulas and mathematical models the means which will provide the desired product in the fastest and cheapest way. In such a context, say the theorists of *The Economics of Defense in the Nuclear Age*, "the choices that maximise the attainment of an objective for a given budget are the same choices that minimise the cost of attaining that objective."[10] In other words, there is no incompatibility between the economic, technological and strategic considerations which enter into the finalisation of a weapons system; "the strategy which is most efficient being the most economical."[11] The problem of security being defined as an economic problem among others, the search for consistency is so completely merged with the desire for efficiency that efficiency ends by taking the place of consistency.

On the other hand, when it comes to comparing different

[10] Charles J. Hitch and Roland N. McKean, *The Economics of Defense in the Nuclear Age*, Harvard University Press, 1960, re-edited by Atheneum, New York, 1965, p. 2.

[11] *Ibid.*, p. 3.

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activities, whose purposes do not appear to converge in any way, the chances of achieving this consistency are already lessened owing to the greater number of variables to be dealt with, just as the games theory finds itself doomed to greater uncertainty with the introduction of new players. But, in addition, and above all, the lack of uniformity in the terms fed into the comparison rules out the presentation as a rational calculation of what will always remain in the field of subjective or ideological, that is to say, political choice. What is there, for example, in common between the construction of a power station or a railway line, and that of a school or hospital, support for wheat producers or growth industries and the budget for cultural activities or for military programmes? Everyone knows, without being an expert in mathematical formalisation, that the problem of consistency is never that of the system in which one places oneself, but of the links to be established between different systems. It is useless for the search for economic consistency to profess to be the formulation of a social rationality, since mathematical calculation is powerless to translate that formulation into fact on the pretext of giving the most objective or the strictest expression to the search for consistency. The incompatibility between the rival needs and aspirations of the different systems cannot be cured solely by the criteria of economic efficiency.

It is juggling with words to talk of planning where, in reality, there is still only programming, and *a fortiori*, of rationality when it is only a question of eliminating those public expenditures which are less justified or more costly than others. In this sense, "rationalising" the national budget is merely a management problem. However, perfected they may be, management techniques are never enough to transfer the rationality of the means adopted to the ends which they are supposed to ensure. There is no conflict of interest, say the authors of *The Economics of Defense in the Nuclear Age*, between budget considerations of economy and military considerations of effectiveness, "except in the determination of the *size* of the budget or the magnitude of the objective to be achieved."[12] But is not this the whole problem of the consistency at which planning aims? Inside a

[12] *Ibid.*, p. 2.

given system (defense, education, research, etc.), planning takes the form of programming the resources and means which will allow a given objective to be achieved most effectively rather than of setting those objectives. As soon as a number of systems are confronted with each other, the choice of "clear and manageable options" to which planning aspires, gives rise to an arbitration between conflicts of interests, in conditions of imperfect information, ambiguity, negotiation and divergent pressures, which inevitably characterise the political web of the process of resource allocation.

Just as the choice of the best means is no guarantee that the end pursued is the best, so the refinement of quantitative procedures cannot eliminate the element of irrationality involved in every political decision. Even the consensus of the partners on the value they attach to the enterprises they decide upon, is not enough in itself to establish their rationality. For example, what are the criteria for saying that President Kennedy's decision to send men to the moon was rational? "It is good that we should know what we mean," writes Raymond Aron, "when we say that going to the moon is irrational. In terms of the progress of science, it is clearly irrational; in terms of economic goals, in all probability, it is irrational; in terms of national security, it is in all probability irrational; in terms of prestige, you have to measure prestige, and you have to ask the President of the United States what he means by that. If he says that to go to the moon before the Russians would be a first-class victory, and that he puts the highest value on that, you may say he is crazy, but he has a case."[13] The perfecting of budgeting techniques may give the resource allocation process more sophisticated, more objective and stricter programming instruments, but it no more institutes planning, in the sense of social rationality, than it replaces the political process whereby conflicts of interest are settled by negotiation—or by unilateral decision.

The limit of these techniques is all the more manifest when the attempt is made to apply them to scientific research. It is still a matter of "reducing the domain of the unforeseeable to

[13] Raymond Aron, "Applying First Principles," in *Decision Making in National Science Policy*, a CIBA Foundation and Science of Science Foundation symposium, London, J. & A. Churchill Ltd., 1968, p. 288.

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a series of clear and manageable options," but this unforeseeable is inherent in and consubstantial with the very nature of the activities which it is professed to plan. Not that scientific activities must be deemed to be so specific and original that they have no counterpart in the productive system. After all, as Christopher Freeman has recalled, agriculture, like science, depends on a probabilistic factor of production, since its results are no less subject to substantial deviations from the norm in the light of seasonal variations, weather conditions, epizootic diseases and the like. If the farmer is no more master of the accidents of nature than the researcher is of the hazardous advancement of his discovery, *a fortiori*, the planner who acts as though an increase in the resources allocated to either sector must necessarily mean increased production and a cut in those resources must mean less production.[14]

In the case of science, however, the planning effort is subject to this special constraint that *it must set itself the unforeseeable as an object rather than an object that can be foreseen.* If, in the case of agriculture, a deviation from the norm involves no surprise as to the nature of the product expected, in the case of scientific research, this little more or little less makes the whole difference between all and nothing; no discovery, no invention is strictly speaking inevitable. The special role of economic forecasting is to look for ratios between certain variables which have proved stable in the past, so as to infer the constancy of these ratios in the future. To take the example of national income, the economist can, in a certain manner, calculate it for future years, assuming a constant growth rate. But it is precisely this postulate of constancy which is lacking in scientific research; there are no statistical series of discovery, invention or the scientific and technical breakthrough, which lend themselves to such extrapolation.

Think of the discovery and isolation of penicillin, the classical example of the role which hazard—or chance—can play in the process of discovery. Pasteur, it is sometimes said, could have identified the principle of action of *penicillium* some fifty years

[14] Christopher Freeman, "Science and Economy at the National Level," in *Problems of Science Policy*, OECD, Paris, 1968, p. 59-60.

or so earlier, if his experimental facilities had been less meagre and if he had not been absorbed in other problems: if Cleopatra's nose had been a little shorter... The conjunction of fortuitous circumstances which finally determined the discovery conforms so perfectly to Cournot's definition of hazard — "the combination of events forming parts of series independent of each other" — that it affords an unanswerable argument against any deterministic theory of scientific discovery; with a more modern laboratory, the culture slides which Fleming was studying might not have been contaminated by *penicillium*, which is, indeed, somewhat rare in the air; now, this contamination was favoured by the type of cultures which Fleming had chosen, colonies of staphylococci, which are particularly sensitive to the anti-bacterial action of this fungus. The imponderable element which dominated the discovery of the principle of action of penicillin was continued in the isolation of the antibiotic; the product obtained by Florey was so impure that the 99% of impurities which it contained would have produced toxic effects masking the therapeutic effects of the fungus; and if Florey had experimented with guinea-pigs instead of mice, the effect obtained would have been exactly the reverse, since penicillin is a deadly poison to guinea pigs.[15] The planning of research on antibiotics could not have started before the isolation of penicillin. It was only after that, and with the aid of the Second World War, that this research effort was the subject of programming designed to abridge the interval between discovery and large scale development.

If science policy does not offer "clear and manageable options," it is because this field remains, by definition, that of the uncertain. The objectives of free research cannot be clearly identified; *a fortiori*, the time needed to achieve these objectives cannot be precisely fixed. Even oriented research, however meticulously it may be programmed, is never certain of the time it will need to reach its goals. Thus, the programmes on controlled thermonuclear fusion, launched almost simultaneously during the 1950's in the United States, USSR and England, were scheduled for

[15] For this example and others, see René Taton, *Reason and Chance in Scientific Discovery,* New York, Hutchinson, 1957.

completion in five years. Even today, they are still far from reaching success.[16] The prestige of certain words should not blind us to the transference of concepts which they disguise; research programming should no more be confused with science planning than the certain with the uncertain.

No demonstration of the most deterministic conceptions of invention has yet succeeded in contradicting the words of the economist Jacob Schmookler: "All that our present knowledge permits us to say is that the probability that any given invention will be made varies between zero and one *inclusive*!"[17] The idea of the optimum distribution of resources among the different research activities presupposes that it is possible not only to increase this probability for a given research sector, but also to measure the advantages and disadvantages of making investments in one sector rather than another. Even at the level of a firm, where the risks involved in research can to some extent be controlled—since the calculation of probabilities defines the limits within which they are worth running or not—this calculation of the particular advantages of different projects or alternatives does not imply mastery of the hazards of the process of scientific creation: the risk here is always present under its other name of uncertainty. On the national scale it is clear that such a calculation is even more inaccessible, and it is hard to see how the perfecting of quantitative planning techniques could improve matters.

The techniques for the rationalisation of decisions have all sprung from the experience of programming modern arms systems, where the objectives and the time set for achieving them must be defined with the utmost strictness. The strategical stake is clear and it is given once and for all: it is a question of life or

[16] Not only were the technical difficulties under-estimated, but above all, the theoretical problems linked with plasma physics were ignored in the programming of projects (cf. H. Roderick, "Fundamental Research and Applied Research and Development," Introduction, in *Problems of Science Policy*, OECD, Paris, 1968, p. 91).

[17] J. Schmookler, *Invention and Economic Growth*, Cambridge, Harvard University Press, 1966, p. 215. On the determinist ideas of invention see especially S. C. Gilfillan, *The Sociology of Invention*, Chicago, Follet Publishing Co. 1935, and above all R. K. Merton's article, "The Role of the Genius in Scientific Advances," in *The New Scientist*, London, November 2nd, 1961, p. 306.

death.[18] If these techniques do not apply, or apply very awkwardly, to research activities, it is not so much because the stake is different—competition and the search for profit may decide a private firm to outdistance the others technologically for much the same reasons as the strategic and diplomatic motives of a nation—as because the whole nature of these activities differs from military operations: their objectives, just like their presumed results, are diffuse and open to challenge at any moment. One would hesitate to advance such a truism, if one did not find, in discussions on science policy, the fascination exercised by these techniques for certain scientists as well as for administrators. Systems analysis, PPBS, cost-effectiveness, trees of relevance and the rest, all this arsenal of quantitative methods is deemed to be a miracle procedure, which can be transposed with equal success from the realm of defence economics to that of research economics.

In reality, these techniques are no more than tools of programming and management, which can serve to calculate the cost and the technical stages essential to achieve a given product or process; and they are so ill fitted to research and development activities that their underlying principles must in this case be inverted. The authors of *The Economics of Defense in the Nuclear Age* have themselves emphasised that: "In exploratory development and research, the precise identification of objectives and scheduling are less important than trying to cover all good bets, selecting first-rate scientists and productive laboratories, promoting competition, and preserving flexibility to follow up vigorously on breakthroughs."[19] This means, in short, that in this instance *the most effective alternative is not necessarily that which seems, at first sight, the most economical.* Some of the consequences they draw from the uncertain character of research may appear to be so many challenges to the rationalisation effort which is the aim of planning. They certainly run counter to the principles of economy which are the concern of all public admi-

[18] "It is misleading to say that primacy in military research and development can give us *only* lead time. This may be enough to prevent or 'win' a war, and, for a nation on the strategic defensive, it is essential to avoid defeat." (Charles J. Hitch and Roland N. McKean, *The Economics of Defense in the Nuclear Age* cit., p. 245).

[19] *Ibid.*, p. 249.

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nistration, and which are so often invoked by the science policy-makers to "rationalise" the allocation of resources to science. Since the result of research activities is uncertain, "duplication of effort"—their pursuit along different lines—is all the more desirable: the more important the result expected from research, the greater its uncertainties, and the greater the duplication needed.

No doubt these principles were formulated for military research and development, in contrast with other sectors of defense economics, and the example proposed, that of the "Manhattan Project," where six distinct and independent methods of separating fissile material were followed simultaneously, cannot be transposed into the field of civil research unless the institution concerned (firm or nation), knows the value it attaches to obtaining the result. But it can clearly be seen from this precisely that science planning depends less on the techniques for attaining an objective, however sophisticated they may be, and whatever their progress, than *on the actual setting of the objective, that is to say the priority assigned to it in comparison with other possible objectives*. The orientation of the scientific research system will not become any more rational as a result of the application of these techniques to the execution of programmes, however great the improvements introduced into their administration. Few projects, after all, have been more meticulously programmed than the "Apollo Project," more strictly subjected to systems analysis, and conceived precisely as one of the most complex undertakings which a research administration has ever had to handle. The rationalisation methods which made it possible to solve the economic and technical problems which had to be faced, once the project was decided upon, do not thereby afford any guarantee of the rationality of the project itself.

TECHNOLOGICAL FORECASTING

And yet, it is on the faith of these methods that the idea has germinated of a "policy technology for science," capable of scientifically overcoming the uncertainties of research and, in consequence, of planning science with quantitative formulas at least as consistent and effective as those on which economic

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planning is nourished. Not, once again, that recourse to these methods as management tools is not imperative and does not introduce greater enlightenment and therefore greater effectiveness into the process of making and implementing decisions. It is commonplace to say that the perceptible quickening of change and the growing complexity of their management problems compel modern societies to project themselves into the future. "On a familiar road" said Gaston Berger, pleading long ago for the prospective approach, "a driver travelling by night at walking pace needs no more than a feeble lantern, but a powerful car speeding through strange country needs powerful headlights."[20] The very success of certain long-term forecasts reflects the curiosity of the general public for an apparently new formula, quite as much as their apprehensions for the future, a future which is all the more disturbing since the speed and accumulation of technical progress seems to make it more imminent and more inevitable. But, if forecasting is recognised as a necessary discipline to the point of becoming an industry in its own right,[21] it does not follow that its discourse has acquired the character of scientific demonstration, or, above all, that it eliminates or even diminishes the ambiguous texture of decisions. The art of conjecture, however, strict the scientific apparatus on which it rests, remains an art.

This does not mean either, that science policy can do without a certain forethought. Research programmes do not conform to the annual pattern of national budgets and their results can never be accounted for in the short term. Since "government is foresight," research administration must inevitably project itself into the future. Even more, when we think of the consequences, direct and indirect, unexpected or undesirable, of technical progress, what field of political action has greater need of foresight than this? Since the applications of science and technology furnish the most powerful instruments of change, it seems all the more necessary to influence the course of events

[20] Gaston Berger, "La Prospective", 1957, in *Phénoménologie du Temps et Prospective*, Paris, P.U.F., 1967, p. 221.

[21] The annual investments of American enterprises in R. & D. institutes and firms are valued at more than $ 65 million for technological forecasts alone. See Eric Jantsch, *Technological Forecasting in Perspective*, OECD, Paris, 1967, pp. 251-253 and 272.

by orienting technical progress in the light of its implications rather than of its genesis. But while we can (up to a certain point) pre-determine the nature, number and even the timetable, of new technologies, we are in no position to foresee their effects on social evolution.

To take the very simple example cited by Bertrand de Jouvenel, the decline in domestic service in the industrialised countries has been the consequence not so much of technological progress as of a deliberate policy of full employment, and if, in the United States, in a period of under-employment, the shortage of domestic staff is not relieved by an increase in the numbers of unemployed, this is because domestic service has no "psychological" attraction, and all the more because there is "the alternative resource of unemployment benefits, resulting from political measures."[22] The technical revolution in household appliances did not do away with domestic service, any more than the introduction of the horse's collar in the West did away with slavery. There had to be the roundabout road of political initiatives which had nothing to do with technological progress (the multiplication of household appliances itself being a consequence of growth linked with full employment, rather than of their technical improvements). "If, in 1913" said Bertrand de Jouvenel, "you had 'fed' a social forecaster with the whole of the technological evolution of the next half-century, he would never have inferred from it the disappearance of domestic staff."[23]

Utopias, too, depicted the future countenance of a society, a future more or less plausible, depending on the links they preserved with the conditions of the present. Unfettered by time, they pictured a future at some imaginary date, in the form, sometimes of a return to the "Golden Age" of primaeval perfection and sometimes of a mythical image of a metamorphosed present.[24] The prospective approach, in contrast, does not evade time by restoring or founding the ideal city; it proposes dates, milestones, promises, which fall into a mathematical form, constitute a timetable and pinpoint the due dates of "events that shape the future." But if the prospective approach challenges

[22] Bertrand de Jouvenel, *The Art of Conjecture*, Weidenfeld, 1967.

[23] *Ibid.*, p. 358.

[24] See *Les Utopies de la Renaissance*, Paris, P.U.F., 1963.

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hazard on the same ground where the Utopias rejected time, the imagination of the future has preserved the same function of exorcism; and the spatial language of the probable does not give one a more rigorous vision of the long range than the intemporal language of preferences. Social forecasting is based on the projection of trends linked with variables whose number, though greater and more sophisticated than those which nourished the Utopias, nevertheless remains limited. Furthermore, as economists have long known, the limitations of all forecasting are set less by defects of method than by the absence of adequate data about the present, uniform, full, and above all, available at the moment when they are wanted: "Getting the data in time," recalls Donald A. Shon, "may be as critical as getting it at all, as when we attempted in 1965 to make forecasts to 1975 on the basis of 1958 data."[25]

It is no different when the prospective approach is applied to scientific research. In the first place, the process, the institutions and the performers of scientific research constitute such a complex system that it is not feasible to assemble and master all the information which seems necessary. Secondly, this system itself cannot be treated as if it had a life of its own, independent of all other systems. As Gilbert Simondon has emphasised, the technical object has a defined mode of existence because it has a genesis, but this genesis is not only a genesis of objects, it is the history in its relation to man and to the world.[26] Technological forecasting defies hazard as though it were dealing with a reality independent of man, peopled with pure objects whose probable existence is defined by the instrumental model of the finished object, disposable and manageable, the tool detached from its history, rather than integrated in the generic terms of its relation to man. And yet that relation is there from the start of play; the pinpointing of the Utopia does not make the genesis of the technical object a game without human partners. Technological forecasts are never more than conjectures as to trends, that is to say the result

[25] Donald A. Schon, "Forecasting and Technological Forecasting," in "Toward the Year 2000: Work in Progress," *Daedalus*, Boston, Summer 1967, p. 765.

[26] Gilbert Simondon, *Du Mode d'existence des objets techniques*, Paris, Aubier-Montaigne, 1969, in particular pp. 154-158.

of a series of questions put to experts, whose views, however they may be mathematically processed, do not thereby cease to be a matter of opinion.[27]

Erich Jantsch's book is the one which has done most to propagate among national administrations the idea of technological forecasting as an instrument of science policy. It is a fascinating book in many respects, and not least by the panorama it affords of the many methods and institutions devoted to "what is defined as the probabilistic assessment, on a relatively high confidence level, of future technology transfers."[28] The concept of "technology transfers" endows forecasting with a new dimension, in that it proposes a "space" within which the ambition is to forecast discovery, invention and innovation, not only in themselves, but also in so far as they have a repercussion on the social environment. The first type or level of forecasting is content to determine the time needed to perfect an invention, the efforts needed to obtain it and its functional possibilities. This is what Jantsch calls "exploratory technological forecasting," as distinguished from the higher level of "normative technological forecasting," in which the very consequences of the invention must be the subject of forecasting. The first level, in short, is concerned with the history of the invention itself, the second with the way in which the invention should affect history in general.

In a certain sense, all the literature stimulated by the prospective approach seems to smack of the *Matin des Magiciens*,

[27] The "Delphi method" is the best known example of procedures which do not conceal the fact that they are primarily based on intuition. Carefully programmed questions on the technological trend in a given field are put to a group of experts, and their replies collated with information obtained from outside; these "brainstorming" sessions are then computerised. The result is a table of forecasted technological breakthroughs, following a timetable composed of intervals of probabilities arrived at by reasonable agreement among the experts. As for technological forecasting methods which try to reduce the role of intuition by the use of different mathematical tools (matrices, similation, scenarios, input-output tables, etc.,) it is obvious that they cannot do without preliminary enquiry from experts. On the "Delphi method," see T. J. Gordon and Olaf Helmer, *Report on a Long-Range Forecasting Study*, Report p. 2982, Rand Corporation, Santa Monica, September 1964; on the other methods see Erich Jantsch, *Technological Forecasting in Perspective*, OECD, Paris, 1967, second part, Chapters 3 and. 4.

[28] Erich Jantsch, *Technological Forecasting in Perspective* cit., p. 15.

and Erich Jantsch's book no less than the others.[29] But the sense of responsibility of the institutions whose activities he lists and describes, and indeed, of the international organisation which sponsored his book, are enough to ensure that he is not to be taken lightly. The optimism which he displays towards the possibilities of technological forecasting is a revelation of the climate in which science policy is steeped; many are the administrators who share this optimism and who are ready to believe that, provided techniques are perfected and applied systematically to government decisions affecting science, rationality will soon make a mock of history by mastering all hazards.

From this point of view, technological forecasting seems more akin to the Roman technique of interpreting omens than to the mythical image of the ideal city in the Utopias of the Renaissance. For these Utopias, in so far as they recked nothing of time, had no concern with immediate action; the augurs were determined to make themselves masters of the signs which determined action.[30] Just as it was the function of the augurs to "valorise," in the primitive meaning of the term, "the omen in itself become, by virtue of the rite, decisive,"[31] so the technological forecasters valorise the lines of force of science and technology, destined by virtue of mathematical calculation to become, in their turn, decisive. The rational and methodical approach transfers the interpretation of signs from the unfanthomable terrain of nature to the formalisable area of culture. The message of the future is no longer to be deciphered from natural manifestations in which the Gods may, perhaps, intervene between man and his *fatum*, but from the products of culture in which the future of technology matches itself with destiny. But the issue has not changed, it is still immediate action, the choice to be made at

[29] For example, when it places on the same plane methods whose degree of assurance is very different, or treats facts as established, which are not. Thus one reads that "only two countries have so far established a framework in which technological forecasting can be used systematically to aid national planning: France and the United States." It is not the "*only*" which shocks, but the assertion that such a framework has ever existed in France or the United States. E. Jantsch, *Technological Forecasting in Perspective* cit., p. 279.

[30] Jean Bayet, *Histoire politique et psychologique de la religion romaine*, Paris, Payot, 1957, pp. 51-60 and Raymond Bloch, *Les prodiges dans l'Antiquité Classique*, Paris, P.U.F., 1963, 3rd part.

[31] Jean Bayet, *op. cit.*, p. 102.

the moment, the decisions to be taken *hic et nunc*, which is to be inspired.

And yet, it is easy to question the assurance of the technological forecasters, since even the first level of their conjectures, that of "exploratory forecasting," is still far from having proved itself—except here again as a tool for programming the transition from the stage of completed discovery or invention to the stage of innovation. The efficient working of discoveries which is ensured by modern management techniques induces the belief that, by the same mechanistic process, discovery itself can be reduced to the execution of a programme forecasting methods allowing the transformation of the potentialities of discovery into possibilities or, in other words, allowing the image of the discovery conceived in time to be treated mathematically as a state already achieved in space. But control of the process by which the product of scientific research can be obtained is still not the realised product. However clearly it may be visualised in its technical affiliations and its economic constraints, the uncertainties of its genesis are not thereby swept away.

Unless of course it is maintained that the discovery to come, like all the future work of which Bergson spoke, is locked away "in some box or other, full of possibles," to which the experts "by virtue of their already long-standing relationship" with science and technology will wrest the key from them.[32] The illusion is no different in speculations on technical change from what it is in metaphysical speculation, even if the event seems more easily calculable as a future state of a closed system of material points; this is to be blind to the fact that it is abstracted from a totality which does not include merely space and matter. "If I knew what would be the great dramatic work of tomorrow, I would create it." One of the best specialists in technical innovation has no different answer from that of Bergson: "There is a special problem about any theory that presumes to permit prediction of invention," writes Donald A. Schon. "In one sense, a prediction of invention *is* invention, and the prediction

[32] Henri Bergson, "Le Possible et le Réel," 1930, in *La pensée et le mouvant*, Paris, P.U.F., 1950, p. 110.

fulfils itself. To claim that a theory permits prediction of invention is to claim that a theory permits invention."[33]

But it is when we move on to the higher level of "normative technological forecasting," that the technological forecasters reveal with less ambiguity the role of augurs which they play, or want to play, in modern societies. "Normative technological forecasting" postulates in effect that it is possible by analysing the parameters of the discovery to come, to anticipate its applications, the needs which they will satisfy and the effects they will involve on the social environment. The "space of technology transfer" is presumed to be explorable in the totality of its future relations, like a feedback system where the knowledge of the result, at once the cause and the effect of the decisions of which it is the product, in all probability allows it to be achieved.

The art of forecasting here tends to substitute itself for the art of decision; it transfers the promises induced from technological tendencies into the realm of social objectives. It is "normative" precisely in that it founds its conjecture on the technical object, on the values by reference to which the realisation of that object should be decided upon. One might almost believe that Norbert Wiener's hopes had been fulfilled, for technical knowledge grown conscious of its own ends; does not "know how" extend itself into "know what?"[34] But the profile of the future, drawn merely with those technical lines which intuition, even associated with mathematical models, can forecast with maximum plausibility, is thoroughly informed by today's values: the options inspired by tomorrow's technological imagination are inseparable from today's ideological realities.

No doubt, like the Roman augurs, the technological forecasters stress that they confine themselves to designating alternatives and that their vaticinations can always be rejected—*omen exsecrari.* The Romans became so skilful in this art of

[33] Donald A. Schon, "Forecasting and Technological Forecasting," in *Daedalus*, already quoted, p. 767. See his *Technology and Change*, New York, Delta Book, 1967.

[34] "There is one quality more important than '*know-how,*' and we cannot accuse the United States of any undue amount of it. This is '*know-what*' by which we determine not only how to accomplish our purposes but what our purposes are to be." Norbert Wiener, *The Human Use of the Human Beings*, New York, Avon Books, 1967, pp. 250-251.

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reassurance as to the scope which the vision of the future leaves for freedom of action that, by mastering the techniques to which they subjected fate, they gave the impression of mastering fate itself. Thus "these technicians themselves, faithful to the tendency of the Latin spirit, became more and more the masters of the signs to which they were deemed to be subject"—to the extent of controlling the appetite of the sacred chickens in their cages.[35] Similarly, technological forecasts must have propaganda value if they are to be fulfilled; the chances which they leave to hazard are all the less the greater the weight of the discourse about the future in the decisions of the present. The lines of force, the trends and directions of scientific research are designated as probable on the basis of the present values which make them desirable and, at the same time, invest them with a higher probability coefficient.

"The important point to remember," says one of the champions of "normative technological forecasting," "is that a planning system thus expanded permits the introduction of objectives and policy-goals as part of the anticipation, and these become operational elements in defining the changes that are needed in the present—in suggesting the sets of policies that need to be applied, the inter-related and inter-active policies that need to be pursued—if the anticipated preferred future is to be translated into current reality."[36] The heuristic function of the modern augurs is no less political than the function of divination among the Romans. Just as that divination became a tool in the hands of authority or candidates for authority, foredooming their enterprises to success or failure, so "normative technological forecasting" nurses the ambition to be at the centre of State decisions, determining their orientations and their ends. "How is an *a priori* history possible?" asked Kant ironically. Answer: "If the seer himself *creates* and arranges the events which he foretells."[37]

[35] Jean Bayet, *op. cit.*, p. 55.

[36] Hasan Ozbekhan, *The Idea of a "Look-Out" Institution*, System Development Corporation, Santa Monica, California, March 1965, cited by Jantsch, *Technological Forecasting in Perspective*, p. 244.

[37] E. Kant, "The Conflict of the Faculties," in the pamphlets on *The Philosophy of History*, Paris, Aubier-Montaigne, 1947.

FORECASTING AND FREE RESEARCH

In holding itself out as the instrument of a social technology,[38] technological forecasting is not satisfied with interpreting the signs which indicate the possible directions of scientific research. It commits itself to a conception of research and of society in which the search for new knowledge is concerned only with the applications which that knowledge makes possible. The "normative" horizon of technological forecasting is utility, whereby the discovery and the invention are translated into innovation and affect not the relation of the research to the object of his research, but the relation of the research product to the social whole. Thus, forecasting techniques are not only techniques, but also ideologies whose postulates set the frame within which the research system is assigned not only its subject but its ends.

Free research, the results of which remain uncertain for the researchers who undertake it and the institutions which support it, appears to be an extreme case—a deviation from the postulates of utility and economic return—only so far as it is admitted that the research system includes an obscure and irreducible zone, doomed to hazard and surprise. But suffice it to postulate the total integration of the research system in the social whole, free research included, even with a lesser degree of assurance than in the other forms of research, for the whole difficulty to be overcome. Predetermining what it must aim at and above all, what it must serve for, the forecasting technician will be in a position to orient free research in line with the social aims of which he is the augur. "Normative technological forecasting," writes Erich Jantsch, "starting from social requirements, is capable of applying spur and guidance to fundamental research in areas of social relevance, in the same way as they are applied by industry in the economic area."[39]

Jantsch cannot be charged with failing to put the question

[38] Olaf Helmer, *Social Technology*, New York, Basic Books, 1966 and Hasan Ozbekhan, *Technology and Man's Future* report SP-2494, System Development Corporation, Santa Monica, California, 27th May 1966.

[39] Erich Jantsch, *Technological Forecasting in Perspective* cit. p. 60. See also of the same: "Technological Forecasting—A Tool for a Dynamic Science Policy," in *Problems of Science Policy*, Paris, OECD, 1964, pp. 113-123.

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fairly and squarely: "Normative technological forecasting" challenges the whole idea of a "pure" science with characteristics of such a kind that its evolution cannot in any way be anticipated. This is the theory of what he calls the "encapsulation" of science—its withdrawal into an ivory tower, immune from the pressures of the profane world—and of which he finds, not without reason, one of the best examples in the book by Thomas S. Kuhn, *The Structure of Scientific Revolutions*. According to Kuhn, scientific progress is made up of two sorts of movement, that of "normal science," which develops within the limits of established "paradigms," and that of science in a period of crisis, when the revolution set off by the "anomalies" of the concepts in use takes the form of strife between the old and new "paradigms," until the victory of the new concepts, recognised and adopted, gives rise to a new "normal science."[40]

The paradigms provide a criterion which is enough in itself for the selection of the problems to be solved. If the scientific enterprise proves useful, it is because of the solutions afforded in the context of "normal science." But it is impossible to influence this process from outside, and *a fortiori*, to foresee the "anomalies" which become the source of new paradigms. "We are deeply accustomed" says Kuhn, "to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance,"[41] but it does not help to imagine that there is some one full, objective, true account of nature, and we should "account for both science's existence and its success in terms of evolution from the community's state of knowledge at any given time. (...) If we can learn to substitute evolution from what-we-do-know, for evolution toward-what-we-wish-to-know, a number of vexing problems may vanish in the process."[42]

This assuredly "purist" conception, which rejects all influence

[40] Thomas S. Kuhn, *The Structure of Scientific Revolution*, Chicago Press, 1962. In many respects Kuhn's "anomalies" recall the "epistemological obstacles" of Bachelard in *La formation de l'esprit scientifique* (Paris, Vrin, 1938, p. 91). But just as the idea of "paradigm" is vague, so that "of the epistemological obstacle" is precise, and rich to the point of being the principle which explains the "anomalies" themselves.

[41] Thomas S. Kuhn, *op. cit.*, p. 170.

[42] *Ibid.*

over the course of science other than that of its own problematics, contrasts, at the other extreme, with the conception of the "integration" of science in the social system. The empirical course of history takes precedence here over theoretical discourse of knowledge, as though knowledge had no significance except in so far as it is conditioned by history. *If these conceptions are diametrically opposed, it is not so much because they both alike find illustrations founded on the facts, but rather because they each refer back to irreconcilable ideologies.* A dialogue of the deaf, because each camp refers to an object which could be defined independently of the values which it attaches to it: "Both positions," said Georges Canguilhem, "come down to treating the subject of the history of sciences as the subject of a science."[43]

Hindsight or foresight, the misapprehension is the same: if one finds here and there, formulated in terms as absolute as those of the "idealistic" interpretation, the conception of the total integration of science in society, it is because the subject of reflection is in both cases presumed to be determinable *as the subject of a science.* The idea of a "pure" science, sheltered in its ivory tower to the point of being heedless of the noise of the modern world, is challenged by the evolution of the relations between knowledge and power, contradicted by the intervention of power and the dependence of knowledge, transformed by the absence of strict frontiers between the different stages of the research system. But, just as the social demands and responses of which science is the subject do not mechanically explain the roads which it follows, so the prediction of its possible results does not mechanically determine the process needed to achieve them. The ambition displayed by the technological forecasters to orient science—and society—on the basis of a scientific fore-

[43] See Georges Canguilhem, *Etudes d'histoire et de philosophie des sciences,* Paris, Vrin, 1968, Introduction, p. 15, referring to the dialogue between Alexandre Koyré and Henry Guerlac at the Conference at Oxford, July 1961, where Guerlac accused Koyré of being an "idealist" that is to say of regarding scientific activity purely as a theoretical activity and giving to the facts of the history of sciences a reality independent of the social context. (Henry Guerlac, *Some Historical Assumptions of the History of Science,* reproduced in *Scientific Change,* A.C. Crombie, ed., London, 1963, pp. 797-812, and the reply of Alexandre Koyré in *Etudes d'histoire de la pensée scientifique,* Paris, P.U.F., 1966, pp. 352-361..

casting of its possible directions, does not do away with the coefficient of uncertainty attached to discovery and invention. And, indeed, the utilitarian postulate, on the basis of which the future of science brought into relation with social needs, is deemed to be predeterminable, does not lead to the application of techniques which are, or even which can be, mastered.[44] It is impermissible to pass off conjectures on the future of the scientific discourse as being fully rational and to treat them as the scientific foundation of possible political decisions; this scientific foundation is non-existent and conjectures do not cease to be conjectures by relying on mathematical instruments rather than on intuition.

It is true that there are some "strategic" decisions which take the form of a more rigorous determination of the fields in which, in view of the results expected, efforts should be concentrated. The country which wishes to equip itself with nuclear power must necessarily have researchers capable of solving the problems of fission and the perfecting of reactors; or again, since molecular biology is manifestly a science "of the future," the same spectacular results may be expected from DNA in the next decade as those which followed in physics, around 1930, from the discovery of the neutron. But this determination is in no way an objective and rigorous discourse on the technical possibilities of the future and their repercussions on the social whole. From the outset, or masked by the mathematical apparatus, it is invested by the values in which its authors are steeped. "One may even venture to predict," writes Jantsch, "that technological forecasting will be largely instrumental in determining fundamental research in the near future."[45] One may no doubt venture, but if fundamental research is likely to be increasingly oriented, it is not because the forecasts in the

[44] Jantsch, moreover, contrasts Kunhn's views with those of R.G.H. Siu who proposes, in *The Tao of Science*, the addition to Western knowledge founded on reason of certain elements of piercing the mysteries of nature (*op. cit.*, pp. 59-61). Scientific creation would be "the fluorescence of non-knowledge," neither rational nor intuitive, but communion. There is no better illustration than this reference of the para-scientific-divinatory-terrain of technological forecasting; one could ask oneself what practical advantage the technological forecasters would derive from it. See R.G.H. Siu, *The Tao of Science*, M.I.T. Press, 1964, in particular Chapter 9.

[45] *Op. cit.* p. 57.

name of which it is oriented will have any greater force of probability tomorrow than they have today, it is because what they will determine as probable will be increasingly confused with what they deem, implicitly or explicitly, to be desirable.[46]

Just as in the history of sciences, there is no forerunner, in the strict sense of the word—because, a forerunner is a man of whom it can be said that it is not until afterwards that he is known to have gone before—so technical forecasting does not predict, in the strict mathematical and deterministic sense of the word, the scientific and technical breakthroughs of tomorrow, or *a fortiori* their effects on the social environment. It can serve as a guide only "all other things *becoming* equal," that is to say, at the risk of the very factor which it professes to eliminate—the gamble on the future. "The subject matter of the historian of the sciences," says Georges Canguilhem, "can be delimited only by a decision which assigns it its interest and its importance."[47] In the same way the subject matter of technological forecasting is circumscribed by the decision of the technological forecasters who apply to it their own frame of reference. If technological forecasting is "normative," it is not because it finds ready-made values stored away in science's "box of possibles," implied in advance in some formalisable relationships between applications and social needs; it is because it colours the scientific future, of which it professes to hold the key, with its own proper values. The normative space of the forecast is not the objective, logical, rational space of scientific discourse. In spite of the mathematical apparatus with which it surrounds itself, it is never more than one of the elements of the equivocal space, in which decisions are reached and applied. If the prophets advise the Prince, they can only be sure of their vision of the future if they themselves make it happen.

[46] Relying on the conclusions of "Project Hindsight," Jantsch writes: "*The absence of normative thinking* has been found to render fundamental research quite useless for the purpose of American defense developments" (Authors italics; *Technological Forecasting* cit. p. 54).

In the first place it is not true, and secondly and above all it is clear that the definition of the useful becomes the pretext here for the retrospective definition of the probable. On the "Project Hindsight," see C.W. Sherwin and alii, *First Interim Report*, Office of the Director of Defense Research and Engineering, Clearinghouse for Federal Scientific and Technical Information, Washington, 1966.