

IMAGES OF KNOWLEDGE, QUALITATIVE INDICATORS AND
SCIENCE POLICY

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(i) The task force of the Presidential Advisory Group on Anticipated Advances in Science and Technology has been worrying that "in many cases technical experts disagree on scientific facts that are relevant to important public decisions."^{*} The cases they have in mind are not less than nuclear power, disturbances to the ozone layer, and food additives. They propose to overcome this difficulty by establishing a Science Court, to become the highest authority on scientific fact.

"It will leave social value questions - the ultimate policy decisions - to the normal decision-making apparatus of our society, namely, the executive, legislative, and judicial branches of government as well as popular referenda".

The problem in a nut-shell. Not, I emphasize, not in the trivial, over-analyzed and only very partially true sense in which all science and scientists are subjective and very much value-oriented specially on these sensitive and by now emotionally explosive issues. This is not my point. I am referring to the more complex epistemological issue of partially overlapping, partially contradictory conceptual frameworks, sometimes one imbedded in the other, each with its own criterion of factuality, its own images of knowledge determining the sources, aims and audiences of knowledge.

The scientific 'facts' on which technical experts disagree are not on the molecular weight of uranium used in the reactors and not on the photo-chemical process which occurs when ultra-violet light strikes an ozone molecule. The

* The Science Court Experiment: An Interim Report", Science, 193, (20 Aug. 1976), 653-656.

issues to be decided are what the Group calls factual statements defined as statements that "must be results or anticipated results of experiments or observations of "nature" and not statements of the kind "if X occurs, then Y may occur". Clearly what they want to eliminate is the value-laden extrapolation from uncertain or ambiguous results. This is good but far from sufficient.

There are no objective facts in vacuo, only facts accepted as objective and shared by all who belong to a given conceptual framework. Since we all share the conceptual framework of today's accepted physical and chemical theory, relative to that framework the molecular weight of the uranium isotope and the basic photochemical reactions are undisputed facts. * But these theories and facts do not determine unambiguously what conditions are relevant for an experiment and often it is on the interpretation of an experiment that disagreement erupts.

* Nobody has checked, or could possibly do so, what shifts would occur between fact and theory if, let us say, we accepted a hidden variable quantum-theory, or a different theory of the evolution of macromolecules than the one we tend to accept today. When very fundamental theories are questioned by an individual or a small group, there is not enough theoretical development as yet to be able to predict minor, let alone applied scientific implications. At that stage the debate ranges on major experimental tests and their interpretation and on the price for change that we might have to pay by giving up whole networks of theories or accepting previously discarded ones. This is only natural.

On such sensitive issues like the ones mentioned above, the scientific community may split because the division is on questions to which the body of knowledge does not speak loudly and clearly: which experiments to rely on in case of contrary evidence; or: if in two set-ups only one parameter was deliberately changed does the "everything else being equal" or caeteris paribus clause apply to it? How do we know that all other things were equal? We do not say that much. We only claim that all but one relevant parameter remained unchanged. And which are the relevant parameters? At this point we already load this question with the whole complex theoretical web of our metaphysics, theories and images of knowledge.

Though the discussion will be couched in terms of experimental evidence, actually it will be on the underlying assumptions either about the world or about the nature of science that the experts will disagree or perhaps reach a consensus. And on these assumptions, mostly about the nature and sources of knowledge, which I called images of knowledge, or about methodologies which too are images of knowledge, there must be consensus. In order to reach it the assumptions have to be articulated, analyzed, debated by those who belong to the "normal decision-making apparatus of our society" as well as by the practitioners. Neither can do it alone. The considerations which involve decisions must thus rely on images of knowledge. These deal with relevance of data, validity of explanation, interpretation of experiments according to some conceptual framework selected out of several possibilities, the hierarchy of sources of knowledge ordered

according to reliability and importance and the priority of some aims of knowledge as against others. Adherence to some images, or rejection of others are qualitative indicators of the state of scientific knowledge in a given discipline, its social acceptability, its appeal to laymen and professionals, its place on the risk-taking scale. All these qualitative indicators are an indispensable input to a well-informed and reasonable science policy way before, and independent of, the interests and values of the groups participating in the decisions process.

(ii) The theoretical background to this argument is a dialectical view of the growth of knowledge and a thesis of simultaneously held realism and relativism - a view I called two-tier- thinking - both of which focus on the central concept of images of knowledge. This is not the place to repeat these theoretical frameworks, I shall only give a bare outline in order to be intelligible and refer to the places where they are developed in somewhat greater detail. *

Knowledge grows by an ongoing critical dialogue between competing conceptual frameworks. These include the body of knowledge (be it the physical world, society of men, the human individual, or God). At the core of the body of knowledge is a scientific metaphysics - that is untestable beliefs about the world, or society, or man, or God which are held to be true in the given framework. Further to the body of knowledge, in every framework, there are

* See my "Rationality of Scientific Change" in M. Wartofsky (ed.), Proc. of the first Internat. Conf. of Hist. & Phil. of Science held at Yvaskylä, Finland in Summer 1973 (forthcoming); and my forthcoming book "Science as a Cultural System".

cognitive entities which have to do with knowledge as an object; these are the images of knowledge, and they are of several kinds: sources of knowledge which tell us what kinds of knowledge are to be trusted in that framework: knowledge drawn from the senses, from conscious reflection, from revelation, from intuition, from authority, from prestige, from political status, from ritual, from tradition, from aesthetic considerations; other images tell us which of the acceptable sources is more important and which is less - i. e., a hierarchical ordering; yet others are the aims of knowledge: knowledge for utility, for status, for ornament, for understanding, for serving God, for happiness; methodologies are images as well. The important common denominator about all images is that they are all meta-statements about knowledge and not about the subject of the discourse, and all are verbalized - i. e., cognitive entities. In addition to the body of knowledge and the consciously held images of each framework, the people who hold a conceptual framework live in a social world where many personal and social interests are at work which dictate to a man how to act, how to think and often what to believe. Some of these forces relate plainly to personal advantage, while others are dignified social-religious ideologies. In any case they are not simply part of the body of knowledge nor of the images of knowledge. When conceptual frameworks are in competition it is scientific metaphysics and contrary images which are the subject of debate, rarely the politico-socio-religious ideologies.

The body of knowledge does not ever include directives for the scientists about problem-choice, risk-taking, criteria of validity, of interest, of worthwhileness, of method, of aim. All these images are socially determined in the

cultural context of the investigator and the group he is part of. Yet it is one of the most pervasive images of knowledge held by scientists today that it is the body of knowledge which objectively gives all these criteria. Is that true?

Relative to my conceptual framework: no. Relative to the conceptual framework of many scientists: unhesitatingly, yes. Here we come to the relativism-realism issue and to two-tier thinking.

A realist believes that there exist some realities, truths, facts which are real and true and factual, independent of any conceptual framework. The relativist says that there are no such things and everything is relative to a given framework. In my view realism is essential in a given framework - not only a psychological but an epistemological necessity. At the same time there is no reason not to remember the framework relative to which those realities look so absolute. If these two attitudes are held simultaneously, this is two-tier thinking. The links between any two tiers are the images of knowledge which supply the criteria according to which reality is fixed in a given framework. This is as far as I can go here. *

(iii) My thesis is that in order to construct a reasonable bridge between the body of knowledge and science policy, images of knowledge viewed as qualitative indicators have to be studied. Differential support of experimental as against

* I have tried to clarify this issue in "Approaches to Science: Philosophical Realism - Historical Relativism", lecture held at Harvard in October 1976 and submitted for publication.

theoretical physics; of evolutionary biology as against molecular biology; of societies whose mores are quite different from ours; of anthropological, historical or psychological cross-cultural studies which are actually exercises in translation, will follow the qualitative indicators expressing the value, worthwhileness, advance, riskiness, beauty, utility of what have not of these areas of research; these are all images of knowledge. The study of images and two-tier-thinking support a pluralistic, open-ended, many-valued science-policy always spelling out the rationale for the articulated indicators and at the same time, searching for and attempting to spell out the tacit indicators. Moreover, a study of images of knowledge will help not only to discover indicators but also to invent new ones. In the examples below I shall try to illustrate some of these ideas.

At present both the language of science-policy is different from the language of historical sociology in which images are formulated, and the people whose task is to formulate a science-policy are neither the scientist nor the historical-sociologists.

The reasons for the divorce between those studying the historical sociology of scientific knowledge and those who have to make science-policy * decisions are social and philosophical. Usually there is a "holy" alliance between the decision-maker and the scientist. They share a presupposition - an image of knowledge: namely that the one party, the scientists, supply the facts - objective, value free, etc. The others, the decisions makers, are

the careers of society's values and aims only being checked and monitored by the scientists so as not to distort the facts and to anticipate other facts. Considering the framework in which they work, this division of labour is acceptable. (Let me remind you once more, lest I be misunderstood, that I firmly hold that relative to any agreed upon framework, some facts are basic, rough, irreducible, etc., and some other entities are theories built on the basis of these facts.)

However, there is another, to me less acceptable, "unholy" alliance between the scientific community and institutional sociology of science. The unwritten agreement is that the epistemological independence of science is acknowledged by the sociologists and in rare cases by psychologists, while the scientists admit that since they are also human, all the acceptable sociological theory on strain, interest, value-ladenness, institutional organization, career structure, professional advance, etc., apply to them. Where sociology should better stop is any attempt to explain in historical or sociological terms the criteria of problem-choice, the sources of knowledge, the definition of aims and priorities, and the theory of explanation. These, as already remarked above, according to the scientific community, are determined by the body of knowledge, and must be left to the scientists themselves. To this most sociologists agree. Add to it, that there is a tacit assumption of both communities, that in any case, without knowing science proper one could not penetrate deeper into the structure of scientific knowledge.

Clearly my whole approach and emphasis on two-tier-thinking and on images of knowledge is contrary to the spirit of the above described alliance. My claim above was that precisely the issues which the scientific community guards most, like criteria of validity, problem choice, considerations of importance, relevance and interest are images of knowledge determined by cultural context and not by the body of knowledge alone. However, as I also made it clear, the images of knowledge depend on the cultural context as much as on the body of knowledge and thus a fairly good understanding of the body of knowledge is indispensable. With this knowledge at hand, historical case-studies can illuminate the mutual dependence between images of knowledge and the body of knowledge: this amounts to a social epistemology, or what some of us call a historical sociology of scientific knowledge.

Very few scientists would go as far as I do in advocating a real sociology of knowledge. John Ziman, the eminent solid-state theorist and FRS is one of the few. Recently, in a lecture to a lay audience he said:

"The scientific world picture is merely an extension of the everyday world encompassing the very small, the very large, the very rapid, and the very slow: If one turns to works on child psychology, especially the books of Jean Piaget, ... one finds a most interesting discussion of the way that a child acquires concepts of number, size, space, time, etc. Two factors are at work:

- (a) personal experience and manipulation of objects, and
- (b) social contact by means of language, with older people who have already learnt to give meaning to such experience". *

* John Ziman, "Is Science to be Believed?", Thursday Evening Discourse, May 3, 1973, p. 273.

(a) is the body of knowledge, (b) are the images of knowledge as determined by the culture. Ziman continues:

"The interpretation of evidence depends on one's training, on motivation, on the context, and on the point of view. "*"

If so, what follows for science-policy? I shall examine a few specific cases:

(iv) National Attitudes to Science as a Qualitative Indicator

One of the panels participating in a Delphi poll on the future of U.S. scientific effort on behalf of the N. S. F.'s National Science Board (in preparation for Science Indicators '72), had to discuss "Criteria for Use in Determining Total Funding Levels for Basic Research". It turns out that the top-rated criterion was potential for fundamental new insights. The lowest-rated criterion was international competition. ** Yet one of the most emphasized results of the generally neutral, low-key, SI-74 which is conceptually even less committal than the SI-72, is that the "bulk of the indicators that are used to compare the U.S. with other countries appears to be headed downward. "*** Now, are international comparisons and competition important or not? Is there a national science at all? Clearly a case

* Ibid., p. 274.

** Joseph P. Martino emphasized this in his "Science Indicators: Charting the Progress of Research", The Futurist, IX (February 1975), 44-48.

*** Philip M. Beffey: "Science Indicators! New Report Finds U.S. Performance Weakening," Science, 191 (# 4231, 1976), 1031-1033.

of conflicting images of knowledge between scientists whose consensus was being solicited by Delphi method and the NSF personnel and leadership. It seems that the scientists were primarily interested in new insights. The consensus of the granting-community was that advancing of national interests is of prime importance. But the split is not so simple: the panelists were surely not that naive as to be interested only in the future of 'Science' in vacuo, forgetting national interests and even the material welfare of their own laboratories. Rather, it was part of their ideology to expect an 'objective', 'value-free', 'internationalistic' view of science to be rewarded by ample funding. From their point of view, their part of the game is to achieve progress. According to the images of knowledge of many science administrators, basic as well as applied science should promote national interests. The differential support given will thus rely on the indicators whether a field or research project promotes national interest and this indicator will be rooted in its images of basic and applied science, of conception of progress, of what is national interest. Whether there is such a tension between competing images of knowledge and how much strain does it create is very important for science policy. In the U.S. today there is such a tension but it is not openly admitted. The language of science-policy as much as of the scientific community is internationalistic. It will not be easily admitted publicly that between two equally important research proposals the one which can potentially do more for national prestige will be preferred. This may be mentioned in the committee where the decision is made.

The result of the tension is thus an official language and an undercurrent, the two rooted in conflicting images, but no open debate ensues and the outcome is necessarily distorted.

Unlike in the U.S., in countries under economic strain, like Great Britain, or under military strain, like Israel today, like France after 1871, or even in Napoleonic times, the images of 'science for the nation' is or was accepted by scientists and science-politicians alike. Alternatively, "international science" first and foremost for the sake of knowledge might have been the dominant images in the U.S. of Vannever Bush, in Germany at around 1900, or in England after the Glorious Revolution. It becomes thus very important to find out what is the connection between the body of knowledge and the prevailing images: whether the images mentioned above are identical for all disciplines, or do they depend on the degree of advance in the discipline and on its current utility. Inside a discipline we shall have to ask what is believed to promote new insights; and what features are thought to serve the national interests. For that, the few, well-known, historical debates about the "health of a nation's science" like in England around Babbage's "Decline of Science" in 1831, or like in France after the defeat of 1871, have to be studied. * Let us compare the images of science in France, Germany, and the U.S. in the second half of

* See also Arnold Thackray's "Reflection on the Decline of Science in America and on Some of its Causes", Science, 173 (# 3991, 1976) 27-31.

the nineteenth century. *

In France, "Science" meant both theoretical and applied science - but not technology - technology was almost never mentioned- science was thought to develop in 'schools' and 'laboratories' rather than due to individual contributions; especially contributions from lonely investigators in faraway places (which in France always meant simply outside Paris) were considered negligible. Along with these images went a belief in the importance and role of institutional organization. In the 1860s, French science was thought to be declining only due to lack of institutional support and lack of equipment. Since the French believed in a national character of institutions - this was an invented indicator - explorations of national characteristics were many. As a result, a school of historical research into the social setting of ideas developed, unlike in England where straight classical inductivist history-writing was at its height. In any case, science proper was taken very seriously by the French Government and its organization was and remained very centralistic.

In spite of such individuals, whose contributions were undisputed, such as Duhem, Pioncaré, Lippmann, Langevin, Perrin, Curie, Le Chatelier, the institutional impact remained central and dirigisme was the hallmark of French

* I do not presume to present here any original historical work. I am merely recounting the work of others and reformulating them into my terms for the sake of my argument.

science.* In France a research project submitted inside the system, fitting the rules of dirigisme has much higher chances to be supported than a proposal by a non-official individual. This became thus for French science-policy an invented, albeit tacit indicator. It is not an accident that J. J. Salomon ran a conference in 1975 posing the question "peut-on re-diriger la science?"** Nobody but a French scholar would have formulated such a question.

When in 1871 the French defeat was simplistically ascribed to the supremacy of German science, the French scientific establishment received unprecedented support and naturally a new era started. Yet, not surprisingly, the new expansion was so guided that it fitted the images of science of the French science-policy leadership. They presupposed that German supremacy was caused by quantitative superiority and high social status. When the famous Dean of the Paris Medical Society, Adolphe Wurtz, reported to his Minister of Education on his visit to flourishing German universities, he even remarked on the luxurious furnishings in their studies, which lent dignity to German scientific establishments. This became a tacit indicator of success. Under his influence the Ecole Pratique des Hautes Etudes was set up in 1868, very

* On French science after 1871 Harry W. Paul's "The Sorcerer's Apprentice: The French Scientist's Image of German Science 1840-1919", University of Florida, Soc. Sci. Mono, 44, 1972.

** The Conservatoire National des Arts et Metiers, of the French Ministry of Education, sponsored under the guidance of Professor Jean-Jacques Salomon an international colloquium in December 1975 with the above title. Shortly described in "Le Progres Scientifique", 181, March-April 1976, 39-46.

luxuriously, on the German model as far as externalities go. * The dignity of German science was anchored in the heavy, self-important and individualistic personality of the German scientist; the absence of the kind of centralistic influence which could interfere with his work was not noted. This could serve as an invented indicator resulting from the study of images of knowledge. **

It is a typical case where science policy is so influenced by the prevailing images of knowledge that much of the important information is overlooked. If epistemological comparisons were made at all, then it turned on the German 'mystical obscurity' as against French 'clarity'; it was not asked how vague thinking and academic freedom related to fruitfulness of research. My point is that there is no way to separate sharply the epistemological from the

* All this is told well by Paul - I am only recasting the story into terms of science policy rooted in the images of knowledge held by professional scientists, administrators and laymen.

** In Germany the independence of the scholar and the image of science as a goal in itself emerged with the new universities in the early nineteenth century. Earlier, when Meupertuis and Euler were active in the Berlin Academy, 'Frederic II (the Great) image of knowledge was purely national science and science for power.' On that, see R.S. Calinger: Frederick the Great and the Berlin Academy of Sciences (1740-1765). Ann. Sci. 24 (1968), 239-149.

social-institutional part. *

The image of knowledge in nineteenth century America is of special interest because in certain crucial elements it is quite different from the situation in Europe. ** After the Civil War the image of science as a status symbol among the mercantile and professional groups became widespread (fifty years later than in England) and the public jammed popular science lectures. Science was often

* The typically German image of science prevalent in the second half of the nineteenth century (in spite of opposite trends coming up from time to time, like in the 1830s and 40s when in reaction to the Naturphilosophie, only experimental science was called science) was that the aim of science was to formulate all-pervasive deep truths of nature. This encouraged vague and mystical thinking, which, if left alone, alongside to great failures also resulted in some spectacular successes like the discovery of conservation of energy which was rooted in vague concepts like "Kraft" and in metaphysical principles like the conservation and the correlation of forces. See my "Discovery of the Conservation of Energy", Harvard Univ. Press, second corrected printing 1975, and my "Scientific and Metaphysical Problems: Euler and Kant", in Vol XIV of the Boston Studies in the Philosophy of Science, edited by R.S. Cohen and M.S. Wartofsky, 1974, pp.277-305.

** Daniel Kevles has shown great sensitivity of the images of knowledge of groups varying in ethnic, racial and religious identity as well as in social and economic status. He documents for the U.S. in the late nineteenth century that when "less than 4% of the nation had graduated from high school, a considerable fraction of the population was in fact indifferent to basic research": "On the Flaws of American Physics: A Social and Institutional Analysis", in G.H. Daniels (ed.): Nineteenth Century American Science: A Reappraisal, Evanston, 1972.

written about in the popular journals: Harpers, the Atlantic, Popular Science Monthly, and even more importantly, it was preached from the pulpit. This was not science for utility but science as culture. The images held by these science-enthusiasts were idealistic. They believed that "technology feeds on science", while "science does not need technology"; "science is mainly theoretical", "impractical laboratory work is not really part of science", "science is culture - to be read and diffused, not hard training, rigorous method, or experimental method". Not surprisingly, the actual science alone was rather mediocre. Since they believed in the beneficial ethical influences of science, since science was good for general culture, colleges continued to teach science. The image of "science for progress" was not yet widespread and thus we have the seemingly paradoxical situation of widespread popularity of a very theoretical science on a low level. What finally turned the tide was an alliance between these university theoreticians and the non-academic, mechanical-minded inventors and patenters. In order to promote their institutionalization and professionalization of science they now, paradoxically began fighting the trend for popularization and advanced a new image of professional, separatist, empirical science to replace popular, watered-down, non-empirical science at the universities. One interesting result, typical for American, was the promotion of theoretical research into empirical-useful fields like agriculture, hydrology, geology, mining, machine-tools, etc., in all of which America soon became the world leader. Now science was indeed progress!

(v) The Aims of Knowledge, For Use, For Power, For Status, For Ornament, For God.

Science can serve for subduing nature, or be a value in itself, or for worshipping God, or for personal ornament; the aim of knowledge is an image and it is an important qualitative indicator influencing science policy.

Knowledge was viewed in very utilitarian terms in the England of Henry VII and Henry VIII. Much has been written on how this image has influenced university curriculum and the private academies when your noblemen received their education. During the Elizabethan Renaissance, with a flourishing economy and political stability, knowledge for ornament started to gain in predominance. A partial reversal occurred around the times of the approaching war with Spain - sudden demand for applied mathematics and related fields brought support for some kinds of knowledge: a yearly mathematical lecture was established in the City of London, Gresham College was founded; hundreds of mathematical practitioners found employment in the houses of gentry and merchants alike. At the turn of the century, Francis Bacon's emphasis on the double aim of science: for "light" and for "fruit" was actually a shift from the predominantly utilitarian towards a more balanced view between knowledge "for delight, for ornament, and for ability". As Bacon so lucidly explained:

"To spend too much time in studies is sloth; to use them too much for ornament is affectation; to make judgement wholly by their rules, is humour of a scholar ... [Studies] perfect nature, and are perfected

by experience". *

In the first decades of the seventeenth century, higher education became the vehicle for attaining social status. With the growing influence of Puritanism, and during the Interregnum, knowledge became the symbol of opposition to the great Puritan vices: Ignorance, Prophaneness and Idleness. Such images and the opposition to the Aristotelian philosophy of the Catholic Church, created a favourable climate for support of the various utopian reform movements. Later, after the Restoration, the virtuosi and knowledge for ornament were ridiculed, while knowledge for use was treated sceptically. Demand for higher education declined somewhat. In the last quarter of the seventeenth century there is practically no support for science in spite of the brilliance of the Royal Society at the very time when Newton's great works were published. Latitudinarian socio-religious ideology supported knowledge as a way to understand God's creation better and worship God deeper. The scientific world-view became a hand-maiden of this emerging ideology. For that reason, in the first half of the eighteenth century, when the Royal Society sinks to a low-point, science at the universities and in society in general, is better supported than in the seventeenth century. This, again, correlates science policy to images of knowledge.

The really "vulgar Baconianism", i. e. , science for use or for power, is the creation of Victorian England, not of the seventeenth century. On the other hand, with the creation of the majestic edifice of classical physics it was also realized that much basic knowledge is necessary also for applied science, even

* Francis Bacon: "Of Studies", being # 50 of the Essays in: Spedding, Ellis and Heath (eds.), The Work of Francis Bacon, VI, 497.

though a direct connection was never clearly established. It was then that the present image was formulated, namely that "there is no basic research which will not finally lead to useful application". This approach was ably presented in all its common-sensical triviality by Joseph Henry in America in the second half of the nineteenth century, just as in our time Vannever Bush became its foremost spokesman. Henry, spreading the English ideas of self-improvement - an image of knowledge rooted in religious dissent and the democratization of the learning process accompanying the Industrial Revolution, lectured in 1876:

"The work of internal improvement, at this time zealously prosecuted in all parts of the Union, emphatically teach us how mathematical and mechanical knowledge may be applied not only to the necessities of ordinary life, but to increase the power and promote the wealth of a nation", (p. 326).

In the same vein, "mixed mathematics" (as applied mathematics used to be called) was important for "the measurement of land, in optics to the properties of light, in astronomy to the motion of the heavenly bodies, and in political economy to the doctrine of annuities, the amount of interest, etc."* (p. 327). Although there was a strong counter-trend of ridiculing basic research among artisans and practical people, Henry's influence was not negligible in securing support for basic research in the early days of American science.

The influence of Vannever Bush needs no documentation. His passionate plea for support of basic research, based on his confidence that it is basic research

* Both quotations are cited by Arthur P. Molella & Nathan Reingold: "Theorists and Ingenious Mechanics: Joseph Henry Defines Science", Sci. Studies, 3 (1973), 323-351.

which chiefly contributes to the needs of pure and applied science alike, is well-known:

"If the colleges, universities, and research institutions are to meet the rapidly increasing demands of industry and Government for new scientific knowledge, their basic research should be strengthened by use of public funds."*

At the same time, let us listen for a discordant image, to the eminent physicist, Arthur Compton speaking in 1944 on the American war-effort:

"A hundred physicists in this war are worth a million soldiers", or "We have learned that knowledge is strength and the intensive scientific research is the only way of supplying certain types of knowledge that are essential to waging modern war."**

It would be interesting to find out how this conflict of image was resolved in American science-policy in the year following the war.

(vi) We have looked cursorily at the image of science as international or national and at the aims of knowledge. A related but not identical issues is the whole complex of science-technology interaction. Any science-policy in the modern world will deliberate how much basic research to support when it actually aims at technological advance either for international prestige, or for economic success, or for military superiority. The issue is so complex that not even from history can we learn much.

* "Science, The Endless Frontier", U.S. Government Office, July 1945, p. 15.

** Quoted by A. Hunter Dupree's book review of R. H. Kargon (ed.): The Maturing of American Science: A Portrait of Science in Public Life drawn from the Presidential Addressee of the AAAS 1920-1970", Washington 1974. The review is in Minerva, XIV (1976), 143-150.

The medieval image of manual labor as a contributing source of knowledge as against the purely theoretical, rationalistic, Greco-Roman classical image, had great influence on the knowledge policy of medieval culture. Since the monasteries inherited the tradition of manual labor and a combination of worshipping God by prayer, study and work, they were incapable of assimilating the twelfth and thirteenth century renewal of classical Greco-Roman cultural heritage. Lynn White makes the interesting point that this inability to assimilate the new-old knowledge may have been one of the reasons why a novel institution, the university, had to be established "to cope with the intellectual needs of the time". *

There is an ongoing debate among historians and sociologists of science whether the contribution of science to the Industrial Revolution was nil, very small, or, at best, uneven. Rupert Hall and others even tried to show that there was practically no role for technology in the emergence of the Scientific Revolution. Charles Gillispie documented carefully that science contributed almost nothing to French technology. Even the influence of Joseph Black's theory of latent heat on the thinking of James Watt, his student, is hotly disputed. David Landes, in his magisterial, "The Unbound Prometheus" describes mainly the contributions of non-professional amateurs and improvers to technology, but

* "Medieval Engineering and the Sociology of Knowledge", in: Pacific Hist. Rev., 44 (1975), 1-21. He also points out that not accidentally the Benedictines who remained manual laborers avoided the universities, while the Franciscans and the Dominicans who "substituted mendicancy for the older monastic tradition of ascetic labor" surged into the universities.

scientists are not mentioned. On more balanced views, like that of Thackray or Mathias, the relations between science and technology are very complex and never straightforward. * Thus, truth is not at all manifest. We do not know what the influence of science on technology is, and we do not know whether or how many, or which basic research leads to useful applications. What we do learn is that in the political struggle between various groups competing for influence and power, the image of knowledge held by them influence their science policies. The interesting epistemological question is what is the connection between the image of science, of technology, and of the interaction of the two held by a group of people and the actual kind and depth of their knowledge on science and technology, how that complex

* See, Arnold Thackray, "Science & Technology in the Industrial Revolution", Hist. Sci., 9 (1970), 76-89, and Peter Mathias, "Who Unbound Prometheus? Science & Technical Change, 1600-1800" in: P. Mathias (ed.) Science & Society, 1600-1900, Cambridge University Press, 1972, 54-80.

interaction expresses itself in a science-policy. *

In addition to the thorny problem of the policy-maker of how much basic science to support if he aims at technological advance, he also has to make up his mind on what kind of basic science to support. Steven Weinberg gave the rationale for continuing support of relativity theory and quantum

* An interesting analysis of how the images of the purity of science from the seventeenth century on developed is given by Molella and Reingold in the above-cited paper:

"Beyond the desire to maintain the purity of the Scientific Revolution of the seventeenth century is a clear element of Whiggish history. The argument implicitly takes the following form. If western civilization was drastically changed from the seventeenth century to the present, science had to play a major role in this transformation as the Scientific Revolution was the major discontinuity in the western world. Technological change is a principle component in the transformation. Since science did not influence technology significantly in the seventeenth and eighteenth centuries, when did its influence become manifest? The obvious answer is during the so-called second industrial revolution of the late nineteenth century which roughly coincides with the rise of industrial research in the electrical and chemical industries. If one probes further to find out why this impact of science occurred then, the implicit answer is that science had obviously reached a stage where it could significantly influence industry. Theory was not applied earlier because past theory was not as good as present theory. Here, in the absence of the restraint, imposed by the desire to keep the Scientific Revolution free of technological taint, the historian of science can assume that theoretical progress necessarily implies technological progress", p. 342.

mechanics as foundation of physics. He expressed his optimism that "we are much closer [to the aim of science] than is generally realized." He explained:

"The reason I take such an optimistic view of where we are now is that relativity and quantum mechanics, taken together, but without any additional assumptions, are extraordinarily restrictive principles. ... When you put quantum mechanics together with relativity, you find that it is nearly impossible to conceive of any possible physical system at all. Nature somehow manages to be both relativistic and quantum mechanical, but these two requirements restrict it so much that it has only a limited choice of how to be - hopefully a very limited choice."*

Thus it becomes a science-policy directive to support, promote, encourage training in only those fields or problems which are rooted in these two physical theories and which, on the one hand, do not proliferate possible physical worlds, nor do they take the conflict between relativity theory and quantum-mechanics so seriously that no world becomes possible. This guide applies to grants, appointments, choice of thesis topics and to refereeing of scientific papers by respectable journals. Accepting these guidelines we can consider the number of additional assumptions we need to add to relativity and quantum mechanics to be able to deduce these observer properties of the elementary particles, as an indicator of the state of physics: the less we need the healthier the field and thus the better support it deserves. Behind this very operational criterion are the images of knowledge which can be explained only as cultural criteria which do not follow from physics: "[one of] the aim[s]"

* "Where We are Now", Science, 180 (1973), 276-278.

of physics is to find the ultimate laws of nature"; these laws of nature must be few and general; "a theory is better the more restrictive it is"; and so on. Weinberg now continues to explain what the essence of the conflict between relativity theory and quantum mechanics is and how one can explain it away. The assumption in relativity theory that there is no velocity greater than that of light and quantum-mechanical uncertainty principle serve each as cornerstone of the basic theory as part of the body of knowledge. But we could have constructed an epistemology in which the uncertainty principle would have been a fundamental image of knowledge, i. e., statement about what kinds of knowledge are permissible and to have started then to construct a physical theory which would have led to 'many possible physical systems'. That such worlds: particles in rigid boxes or particles on springs are not permissible, follows from Weinberg's basic realistic image of knowledge, according to which these cases "which don't exist in the real world but are perfectly consistent with the principles of quantum mechanics" are not allowed in physics. Being a realist, and not an instrumentalist, Weinberg now develops and accepts the general ideas of quantum field theory and the conclusions that follow from these. What criterion is there for checking the acceptability of these conclusions? The criterion, the source of knowledge, for Weinberg is not empirical and not even straight deductionistic:

"These conclusions are reasonably non-controversial, by which I mean not that they have been proved rigorously, but that most theorists would expect that they would be proved in any relativistic quantum theory".

His source of knowledge "that they would be proved in any realistic quantum theory" is a complicated amalgamation of prediction, deduction and consensus!

For a different source of knowledge, let us recall a remark at the end of the Einstein-Podolsky-Rosen paper:

"One could object to this conclusion on the grounds that our criterion of reality is not sufficiently restrictive. Indeed one would not arrive at our conclusion if one insisted that two or more physical quantities can be regarded as simultaneous elements of reality only when they can be simultaneously predicted"... *

For Einstein, restrictiveness, is not a source of knowledge! On the contrary!

(vii) Risk-taking in problem choice

As is documented in the various surveys on the images of science among secondary school children and university students, the concept of science is connected with concepts of competence, certainty, formalism and hard work. On the other hand, science, least of all other human endeavours, is thought of as imaginative, unpredictable, uncertain or risky. These prevailing images leave their mark on problem- and discipline-choice from the very early age and up to the doctoral dissertation at the university.

At the school level when a teacher discovers ability for formal thinking, logic, mathematics, or non-verbal intelligence, he will, according to his own images, decide, for the pupil that his talents are in the scientific

* A. Einstein, B. Podolsky & N. Rosen: "Can Quantum-Mechanical Description of Physical Reality be Considered Complete"? Phys. Rev., 47 (1935), 777-780.

direction. On the other hand, the imaginative, non-mathematical, artistic or verbalizing mind is called a humanist, artist or literary type who should be directed away from the sciences. The result is probably a great loss to both the humanities and the sciences and what is much more, millions of unhappy and intellectual maladjusted individuals.

The kind of research which is necessary in order to find out the images of science among science students at the university (or in high-school for that matter) and to find out the relationship between these images and the body of knowledge, the style of instruction, the social setting of the students, etc., is exemplified by Dorothy Zinsberg's five-year-long follow-up on the attitudes of chemistry students in a British university from 1968-1973.* This study was not meant to be quantitative. According to her findings: first year's students attitude to science in general and to chemistry in particular were very critical; later personal inter-group-relationship became increasingly important, actually more than the subject-matter or the method of instruction; for third year students the problem of job availability came into focus. These indeed are important indicators for the policy-considerations of future civil servants and administrators. But for understanding the complex web of images of knowledge it is not enough to know what their basic attitudes during their student days were. A much more refined analysis is needed: what was the connection between the discipline, its cognitive structure, the method of instruction, the level of sophistication and

* "Education Through Science: The Early Stages of Career Development in Chemistry", Soc. Studies of Science, 6 (1976), 215-46.

the attitudes developed. This is difficult, but not impossible - yet indispensable. Though by now outdated, but closer in its approach to the desired, is Margaret Mead and Rhoda Métraux's 1956 paper on the "Image of the Scientist Among High-School Students"* as is a more recent paper by the staff of the Harvard Project Physics.** The Mead-Métraux identified that an image of intellectual excitement in science was altogether missing. In fact, that this was missing became an early warning signal which together with the appearance of the Sputnik and the concomitant impression of the relative backwardness of U.S. science, helped to bring about a real revolution in science-teaching. Unfortunately, it had very little influence on college teaching, and even less on "the style of training" in good university departments.

At the advanced university level the situation is both complex and much more serious. A career in science proceeds in such narrow, well-defined channels that students are encouraged to pursue in their doctoral dissertation what they started when working towards an M.Sc. The young scientist is then encouraged to go into post-doctoral training in the same area, thus becoming a 'marketable' competent expert at the early age of 25. In order to secure a career, no allowance is made for groping or for failures. If he fails at any stage, his career is ruined. A conscientious doctoral supervisor

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* Science 126 (1956), 384-390, reprinted in B. Barber & W. Hirsch (eds.) Sociol. Science, N.Y., 1962.

could not allow him to take a risky topic or a problem where he, the supervisor, does not envisage a solution and a length of work limited to two, or at most, three years. This is tantamount to no-risk-taking in problem-choice. With the present career structure a scientist can afford to choose a risky, under-defined, or plain too big a problem not before the age of 35 if by then he has achieved tenure. How often is it brought up as an argument against a thesis prospectus that it is too big, too ambitious, too risky! At the same time it is also admitted that in the formal sciences the student is at the height of his creative potential at around the age of 20! The resulting science-policy follows the logic of the problem-choice: low-risk topic, high probability of success, good reputation, tenured investigator - these are the usual preconditions for receiving support! The number of Ph.D.'s in an area of science is a quantitative explicit indicator. In view of what was said above, low-risk-taking, small-problem-choice, remaining in the well-defined reward-system of a discipline is a positive qualitative indicator, partially tacit. With my view of the growth of knowledge and the implicit image of progress the same qualitative indicator becomes at least partially negative. By partially I mean the following: if the research-policy supports only low-risk projects the chances for progress are low. If it supported only high-risk projects the chances would be similarly bad. What I would consider a positive qualitative indicator for a healthy science-policy would be, let us say, a 20% of the total support going to high-risk problems.

Possibly I am right, or maybe I am wrong. That is not the point. The point of this paper is that the desirable science-policy should emerge out of a critical dialogue on competing images of knowledge. The correct science-policy is neither independent of the body of knowledge, nor does it follow unambiguously from the body of knowledge. It depends on images of knowledge serving as indicators.