

CAN SCIENCE BE MADE RATIONAL ?

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1. Before this question is dismissed as paradoxical or even silly, it is useful to remember that a distinction can be made between cognitive and practical rationality, or k-rationality (concerning knowledge) and d-rationality (concerning doing or acts)¹. Since science provides us with the most reliable procedures to acquire knowledge, the scientific method is certainly k-rational. But in order to be d-rational as well, a human activity ought to be directed to clearly defined goals and the most adequate means should be used to attain them. Moreover, human action is not characterized by a mere succession of particular activities and equivalent goals, but rather by a goal or value hierarchy in which partial goals function as means to more general ends. Therefore true d-rationality with an individual or a society, presupposes the existence of a clear insight in the goal hierarchy, and the choosing of those particular (micro)goals which contribute to the attainment of the more general ones. (the beliefs concerning the means-ends relations should be based, of course, on k-rational enquiry).

From this point of view, it is clear that human activities can be called (d-)irrational when a value system of an individual or a society is jeopardized by a dysfunctional relationship of the micro goals to the basic ends, even when these micro goals themselves are reached in a d-rational way. Now, since science may be considered as a system of activities of individuals, or of human society in general, it cannot justly be called d-rational unless (a) the aims are clearly defined, (b) the best means to attain them are used, and (c) these aims themselves and the activities to arrive at them are not conflicting with the more all embracing value systems of individuals and of society.

2. But what are the aims of science ? With regard to this questions two kinds of answers seem to be widely accepted and are rarely

questioned.

In the first place it is said that science is an activity whose basic goal is to arrive at *knowledge for its own sake*. In this view scientists are human beings moved by a passion for insight and endowed with the creative capacity to attain their ideal to some extent. The pursuit of knowledge for knowledge' sake is considered to be a sublime human activity that, just as the creation of art, doesn't need any further justification. This conception can be traced back to Plato, Aristotle, Archimedes, etc., but has also been upheld by a great number of scientists, throughout the history of modern science. Some of them — like e.g. the mathematician Hardy — stick to this idea so tenaciously, that they would consider it almost a blemish if their theories would prove to be applicable.

The second conception sees science as an activity in the service of *human welfare*. Like Francis Bacon, Robert Boyle and many others after them have expressed it, knowledge is a means to command nature and through its development man will become more and more capable to master the world and to explicit it in the service of mankind.

These two conceptions are not mutually exclusive : most scientists and many others with them, seem to accept both with a greater or lesser bias for one of them according to their personal temperament.

From the point of view we are concerned with in this paper, it is important to call attention in the first place to a characteristic these two conceptions have in common and that I would call "scientific optimism". This is a firm belief in science and in the unquestionableness of its free development, which seems to be based on the following postulates. (a) Science is something *good* anyway as an end in itself, or because it contributes to human wellbeing. (b) Science is a search for the unknown; as nobody knows where the unknown is to be found, the development of science must be completely free. (c) When problems or dangers arise in the course of its evolution they will only be solved or eliminated by even more science. (d) Finally, as in classical economic theory, there is a sop to conscience at hand : whenever a particular scientist, has doubts and autocriticism on the worth of his own activities, he may take comfort in the reflection that through an "invisible hand" the efforts of individuals, in whatever field they are working, contribute ultimately to the progress of human knowledge and to the benefit of mankind in general.

If these postulates were indubitable, nothing could be wrong with an unbridled development of science and an unquestioning engagement in it could justly be called rational in the full sense of

the word.

But, such an unconditional engagement *has* been questioned. A few months before his death A. Einstein published the following statement : "If I were young man again and had to decide how to make a living, I would not try to become a scientist or scholar or teacher. I would rather choose to be a plumber or a peddler, in the hope of finding that modest degree of independence still available under present circumstances". In a letter to Max Born he made the following comment : "What I wanted to say was just this : In the present circumstances, the only profession I would choose would be one where earning a living had nothing to do with the search for knowledge 2.

It is a profound tragedy that a man who had known the greatest successes one can achieve in the clarification of our insight in the structure of the world and who, according to his own sayings had found great pleasure in physics ("Weil es uns Spass macht"), should end his life with such an opinion of science. How can this be explained ?

Up to the second world war, Einstein, like most scientists had been a believer in scientific optimism : there could be nothing wrong with knowledge and science. But after the explosion of the atomic bombs on Hiroshima and Nagasaki he came to the insight (as also Oppenheimer, Wiener and others) that this optimism was not tenable any more, that you could no longer engage in science for joy or pleasure without further ado : it had become a *serious* business. An unrestrained development of science would not necessarily lift up mankind to the heights, but could equally well destroy it.

It was no less appalling to realize the danger of the conscious or unconscious belief in an "invisible hand". Einstein was shocked that a great number of scientists, even after the explosions and after the end of the war, were willing to continue the weapons research as being an acceptable scientific aim using the pretext that if things would go amiss, the politicians would be to blame.

Since these first warnings, their legitimacy has been accepted by an increasing number of people and in the same time the insights have been broadened : the problem of science is not confined to the engagement in the weapons industry; also research in "peaceful" domains like agronomy, molecular biology, medicine, etc., can directly, or by the detour of applications, lead to dangers for man and his environment.

If this is the case, the paradox is inevitable : science, the *rational* procedure *par excellence*, has evolved to a system of activities the free *development* of which can no longer be called a rational process.

It seems to me that the paradoxical character of this situation is a continuous impediment to a clear analysis of the issues involved. It leads e.g. to the strange conceptions of the Frankfurter philosophers, who claim that reason itself contains its own negation. The development of the Spirit of the Enlightenment would inevitably result in an impoverishment : from wisdom to purely instrumental thinking and from humaneness to barbarousness. A less sophisticated but more popular reaction to this paradox finds expression through the prophets of a "counter culture" and the numerous adepts of pseudo-sciences and new religions. A solution cannot be found there of course. Irrational traits in the development of science and in the engagement of individual scientists, can hardly be eliminated by the indictment of rationality itself. On the other hand, the reaction of a great number of scientists is no less sterile. Warnings concerning the dangers of science are invariably waved away with two common places : either they point out that the critic fails to see the difference between science itself and its applications, or they refer to the excesses of the counter culture adepts to prove the irrational and reactionary character of all criticism of science. They continue to believe that science is rational in itself and so intrinsically good. "*Tout va pour le mieux dans le meilleur des mondes*".

If, however, Einstein and so many others are right, if the problem is not fictitious, we do not need commonplaces and appeasing talks, but rather more attempts to scrutinize what is really happening, and to find out where the origin of the misunderstandings lies.

To clarify the issue it is necessary in the first place to demonstrate that "scientific optimism" and the theory of the "invisible hand" are by no means rational but on the contrary, purely mythic conceptions.

Those who hold that science promotes human wellbeing assume apparently that through science we get control over nature and, in other words, that it provides man with more means to realize his *aims*. But this benefit can only be achieved when there *are* indeed generally accepted aims of mankind. However, it cannot be denied that up to now mankind has presented a rather gloomy spectacle of profound disagreement with regard to its aims. There has been incompatibility between the conflicting aims of enemy nations and, within the states themselves, opposition between the aims of different classes. In our time it is difficult not to be alarmed by the conflict of interests of the rich and the poor countries and the divergent views on the basic goals of society between capitalist and socialist countries. True, since the 19th century ever more powerful

means to control nature have been developed, but there is no universally accepted value hierarchy, and even if it would exist, no common organisation of mankind could direct the arsenal of means to the ends.

In consequence the means tend gradually to lead their own existence : they influence the choosing of the goals rather than to be chosen in function of preexisting ends; or they are made subservient to the interests of particular individuals, groups or states, which entails the danger of reducing much more than enhancing the general welfare. This problem of the proper relationship of means and goals forces itself upon us in a more acute way with regard to the *sciences of man*. When human beings become the *object* of science, as is the case in physiology, human genetics, psychology, neurology, then man himself is open to power, to control. He can be *changed* even in his personality, as has become possible e.g. with psychopharmaca, psychosurgery or electroshock therapy. But changed to what ? Who will prescribe the objectives ? When, e.g. with persuasion techniques based on psychological research, millions of people are induced to smoke or to drink more alcohol, smoking or drinking become part of their goal hierarchy; but who has decided that this should be a goal for them ? It is clear that the candid vision of "science in the service of human welfare" is no longer tenable when man's conception of welfare itself is open to control through techniques based on that very science.

The second naivity of scientific optimism reveals itself in the answer its adherents are always prone to make to criticism of the foregoing kind; we should make a clear distinction between pure and applied science or between science and technology; applications of science may be dangerous, but pure science, the pursuit of knowledge for its own sake, can still proceed without being affected by these drawbacks. Those who hold this opinion ignore that since the 20th century science and technology are so interrelated that they could no longer lead a separate existence. Technology cannot fully expand without the theoretical basis provided by science, but science itself cannot go on in its most advanced areas without an enormous technological arsenal needed e.g. for the detection and measurement of new phenomena. Moreover, purely theoretical science itself has obtained an intrinsically technological characteristic. Scientific enquiry is no longer restricted to the study of objects or phenomena existing in nature; modern research is constantly creating part of its own subject matter. In physics, reactors, accelerators, lasers, produce new elements, new particles, new kinds of radiations; in chemistry

new compounds are constantly made with previously unknown properties; in molecular biology one tries to change macro-molecules and to create new ones and further research will inevitably lead to the development of new viruses and perhaps of new cells. The reason of this is obvious : scientific theories have an intrinsic tendency to generality : they do not apply only to the objects we know, but to all entities that *could* exist in a given field. A test of the generality of laws implies necessarily the creation of new situations and new objects. This is not applied science but an inevitable characteristic of theoretical science itself.

Once this is ascertained, one should realize that these products of science too can begin *to lead their own existence*; the bare fact that they are there, changes the conditions of man's interaction with his environment and with his fellow beings. In a world like ours, without central organisation or common aims, the invention of fission energy, fusion energy, lasers, viruses, antibiotics, psychopharmaca, etc. causes irreversible developments, the direction of which gets totally out of the control of the inventors. It is naive when scientists say they only make scientific discoveries which, to their regret, are afterward misused. They *should* know that in human society as it is organized now, there cannot be any guarantee that they will be used in the right way : this is a matter of fact, independent of the question whether the politicians are good or bad. The existence of this *necessary connection* between the progress of science and the uncontrollable impact of its discoveries makes clear that we face a problem of the development of science itself, and not only of the terms of its application. In these circumstances the "bad uses" argument amounts to the same as if Pandora, after opening her box and causing all evils to spread over the world, could have rightly excused herself by saying that she only wanted to have a look; or, as if one could distribute matches in a kindergarten and then complain afterward that the children didn't use them in the right way.

But even in a *centrally governed* world with well defined aims for mankind, science would continue to raise problems. In the search for knowledge and explanation new objects would still be generated (e.g. viruses) some of which man might prove unable to master. And the conscious application of science in the service of the accepted goals could turn out to be a complicated matter. A thoughtless reliance on new inventions in the field of agriculture, medicine, production of energy, pedagogy or behaviour control, could, in the long run, give rise to problems more difficult to overcome than those they were introduced for.

We conclude that an *unbridled*, undirected development of science, pure or applied, can never more rightly be considered as a rational, justifiable process. If this would be true even in an ideally organized world, it is the more urgent in our situation to challenge scientific optimism as being *nothing* but an ideology, that procures a false feeling of security and progress, while (d-)irrational and dangerous developments may be taking place.

3. As already has been pointed out, the confrontation with this questionability of the evolution of science gives rise to unbelief or denial on one side and to the flight in irrationality and pseudo-science on the other side.

But a reasonable response to the issues at stake will not be found by pushing them aside or by falling back on wild fantasy. It is impossible to stop the development of science, and this is not desirable either; in order to solve the problems of an overpopulated world, we are badly in need of more knowledge and insight. On the other hand, an uncontrolled development is equally dangerous. So the only possible way out is to find directives for planning scientific research. *Like any other activity* of individuals and societies, the pursuit of knowledge should be organized in a conscious, rational way.

About this issue too widely divergent opinions are held. A considerable number of scientists contend that the future of science cannot be planned, since by definition, science is concerned with the unknown : and the unknown is unpredictable. Accordingly they advocate complete freedom of research for all scientists. In this view, organisation and management of science can only be a hindrance for the really creative scientist.

As is the case with many easy generalisations, this point of view is naive and, when it is taken literally, it is even silly. Nobody could seriously advance that any kind of research has an equal probability of resulting in success in any kind of domain. It is far more probable that physicists will obtain results in physics and biologists in biology; and even if one has to agree that a particular development in physics may lead to progress in biology, reasonable estimations of the probability that such a thing could happen in one area of physics rather than in another can be put forward. Likewise, one could argue that e.g. studies in the field of artificial intelligence are more likely to advance our knowledge of the human mind than would results concerning astrophysics. Some members of the scientific community are fascinated by the fact that a number of discoveries have been made by scientists while they were occupied with a completely

different problem. They seem to forget, however, that the great majority of discoveries were made by the very people who were searching for them, and in the other cases the results were obtained by men who at least were prepared for them. Stories of unexpected discoveries are interesting as a warning against rigid and dogmatic opinions concerning methodology, but they cannot be used as an argument against serious organisation of research, e.i. against a rational analysis of the relative fertility chance of particular research directions with respect to specific objectives and even to the development of science in general.

The claim to complete freedom of research is not only naive, it is also fictitious. Development of science is impossible without considerable financial support. The investments are made either by the state or by private enterprises and their money is never sown at random over the heads of scientists and scholars, but is distributed according to certain criteria based on the conviction that some kinds of research are to be furthered more than others. Those who stress the benefits a free development of science has procured us up to now and express their fears of organisation, simply ignore the fact that since the second world war only a small amount of the "Research & Development" funds has been spent to completely free, undirected research. In fact, most of the money went to weapons research and to space programs closely related with it. Instead of objecting to the rightfulness of an organisation of science, it would be more realistic to ask whether the type of priorities that are assigned *now* are really the best ones for the furthering of knowledge and the welfare of mankind.

With regard to the issue of organisation of knowledge there is a growing interest for a position completely opposite to that of the advocates of free research. In view of the great number of urgent problems in our world, like hunger, malnutrition, illness, ignorance and poverty, the thesis is put forward that absolute priority should be given to applied science, to research programs that are likely to result in a fast relief from acute distress; this point of view is, at a first sight, comprehensible: when science is a means to realize a more humane world why should'nt it be directed preferably to those area's where the need is the greatest? However, a complex instrument, as science certainly is, cannot be adequately used by simply stating some objectives; it has its own laws and limitations. We could certainly try to gather large amounts of empirical evidence concerning a problem area and then hope to arrive quickly at a solution by using some simple methodological rules; but history of science is there to teach us that this would'nt necessarily work. In

many cases, progress — even in applied science — has only been made possible by a roundabout way of decisive advances in pure theory. The great strength of science is its tendency towards generality and as a consequence of this, the remarkable quality that every advance towards the realisation of more general and more encompassing theories, carries in its track a great number of new insights and practical consequences; much more than would ever be possible if they were searched for each separately. Therefore, although it be necessary to remain alert towards the problem of quick application of science to the urgent needs, it could be harmful to limit ourselves primarily to applied science and technology.

4. If we conclude that unrestrained freedom of research is not desirable — even if it were possible — and that an exclusive bias for applied research projects cannot be a reasonable alternative, we are again left with the question on what kind of criteria an organisation of scientific research could be based.

To solve the problem in a satisfactory way would of course be a formidable task and I cannot pretend to give in this paper even the outline of a solution. I shall only point out some distinctions which can be made to avoid simplifications of the issue. First, in the discussion concerning the organisation of science, a preliminary distinction between two types of directives obtrudes itself.

As the development of science is an endeavor made by human society, it follows that society has a right and a duty to fix the ultimate objectives of it and to make sure that its basic goal hierarchy will not be endangered. Accordingly one is entitled to ask what are the aims of society concerning science, or at least through what kind of channels the opinions may be shaped and the decisions may be formulated. In other words how and to what extent is a democratic control of science possible ?

The second type of questions takes into account that in order to guarantee an optimal development of science — to make research as efficient as possible — one has seriously to reckon with the internal laws and necessities of scientific development itself and with the relative risks and benefits that may be expected from particular lines of research.

Questions of the first type are very difficult to tackle in a general way as not only the opinions concerning the value hierarchy but also the channels of decision-making vary greatly from one society to another. Moreover even when one finds it desirable that the general aims are set by democratic consent, one has to admit that democratic decisions cannot dictate the laws inherent to optimal research.

Therefore I would limit myself in the rest of this paper to looking for some criteria which have to be followed in order to avoid that human decisions would harm scientific enquiry in its most valuable core.

As a first step in this direction it seems to me that we can distinguish a particular domain of research which may continue to deserve considerable research priority and, of course, important investments.

The demarcation of this research area can be facilitated by referring to the concept of "micro-reduction" introduced by Oppenheim and Putnam in an influential article on the topic of unity of science³. In order to clarify the way in which this unity can be attained, they state that it is possible in present day science to distinguish a number of different universes of discourse which can be ordered as a series of "reductive levels". To say that a branch of science B₂ (e.g. chemistry) is *reduced* to a branch of science B₁ (e.g. physics), means that the theories of B₂ are explainable by (deducible from) the theory of B₁⁴. The reduction is called *micro-reduction* when the objects studied in branch B₁ are *parts* of the objects dealt with in B₂. In other words chemistry is micro-reduced to physics, when the theories of chemistry can be explained from physical theory and when the objects of physics (e.g. atoms) are parts of the objects of chemistry (molecules) (or inversely when molecules are *composed* of atoms.) The authors advance that the object levels of the sciences may be ordered in the following succession, according to the part-whole relationship suggested by micro-reduction: elementary particles, atoms, molecules, cells, multicellular organisms (including human individuals), social groups of organisms (including human societies). The ideal of unity of science would be attained, when all science could be reduced to a general theory of elementary particles, not directly, however, but in this sense, that from the theory of elementary particles, the theory of atoms could be deduced, the theory of molecules explained by that of atoms, etc.. Although the actually existing branches of science do not coincide exactly with the proposed object levels, the idea is, of course, that somehow, when this hypothesis will appear to be true, the theories of sociology and ethology will be reducible to those of organisms (animal, plant and human physiology, including neurophysiology), the latter to the theory of cells (cell biology and part of molecular biology), cell biology to chemistry, chemistry to atomic physics and ultimately, all actual physical theories (including astrophysics) to a theory of elementary particles. If you add — as I would suggest — a level concerning the *products* of individuals and societies (culture) and you loosen somewhat the strong requirement of micro-reduction

(which only permits reduction to the immediately foregoing level), it is possible to consider this reduction chain as a reasonable approach to the future ordering of the most fundamental scientific theories.

Now, I submit that we call **basic research** these enquiries which tend to build up the theories concerning these object levels in such a way that the *reduction links be more and more realized*. E.g. when a type of research in quantum physics is contributing to a perfection of the theory that leads to a better explanation of the basic laws of chemistry, or when research in molecular biology tends to realize the link between the theory of cells and that of molecules, I would call this basic research. The advantage of this definition lies amongst other things herein that it enables us to delimitate a type of research in which the tendency to generality and deductive strength is maximized. If there is any practical benefit for man in the knowledge of human physiology (e.g. to improve medicine) or of chemistry (e.g. to make better plastics or alloys), it will certainly be *enhanced* by the improvement of the reduction lines to the preceding levels. A clarification of the links between organisms and the composing cells will lead to new insights in the structure and function of organisms, and the reduction from the molecular to the atomic level will widen our grasp of the limitations and possibilities of new molecules. Moreover, the demand to improve the reduction link is a constant incentive to the perfecting of the reducing theories themselves. Thus, in "basic research" the search for knowledge for its own sake, which still remains a drive for many scientists, merges happily with the goal-directedness wanted by society. It could be argued that this program corresponds exactly with what most of the scientists have been doing all the time; reference to these reduction links would then be trivial and offer no criteria for research priorities.

This consideration fails to take into account a remarkable trait of our ladder of object levels. When we climb it up (.. organisms, cells, molecules..) we arrive at ever more general, all-embracing theories, and when we descend it, the theories become less general, but most of all, we notice a *diminishing necessity of existence* of the objects concerned. We could not easily conceive of a universe without elementary particles, but we could perhaps imagine it consisting only of hydrogen atoms; if this would be the case, most atoms would not exist but the elementary particles would already be implied. Stars and planets have gone through stages at which most of our elements were there, but none or only a few of our chemical compounds. Similarly, on the majority of the planets we would find lots of molecules, and, of course, atoms and elementary particles, but no cells and perhaps on Mars there are unicellular beings but no

multicellular organisms. It is clear, that the human beings we are, and our societies and cultural creations, present an even more contingent character.

In consequence, science on the different objects levels cannot be considered *general* in the same sense of the word. The science of elementary particles has to provide us with the general laws of the universe as necessary conditions of all that can happen in it⁵. But — for somebody primarily interested in the *reduction links* — the theory of atoms doesn't have to be worked out in full generality : we do'nt need a theory of all *possible* atoms (elements) : perhaps a theory excluding the transuranium elements (and many isotopes) could suffice to explain the characteristics of all naturally existing molecules and obviously a theory describing *all possible compounds* is not asked for to explain the properties of our macromolecules and cells. Again, to deduce the laws of the organisms of our earth, we do'nt want a theory embracing all *possible living cells*, but only one describing those that have really come into existence on our planet. The same remark can be made, with regard to the theories of society and culture. The point is that, although we can certainly plan research programs to penetrate the secrets of possible, *non-existent domains* of objects, and as a consequence create them, (as we are doing to some extent in atomic physics, chemistry and biology) these enquiries may be, but *are not necessarily* related to the objective of constructing the above mentioned *reduction* chain. In order to arrive at full knowledge of human beings and their culture we should be able to climb up the ladder of objects and the succeeding parts they are composed of, but while descending it we need not know all the other ways the universe could have run (and did run perhaps on other galaxies).

Thus we are able to define a type of scientific research which can be distinguished from basic research as well as from applied research. As I called **basic research** all enquiries related to the building up of the reduction links, I suggest to call **quasi basic research** all types of scientific work concerning the characteristics and, eventually, the creation of *possible objects* (and phenomena) not existing before human intervention. Since both, basic and quasi basic research, are of the theoretical kind, they cannot be confounded with **applied research** (technology) where a direct answer to practical problems is searched for. The rationale of the basic-quasi basic distinction is the following. *Basic research* is concerned with the search for knowledge of what we are, where we come from and what are the general laws governing our existence and that of all things around us. It is very likely that an elucidation of these problems not only gives us an answer on the old

craving for insight, for its own sake, but, simultaneously, through the construction of increasingly general theories, an overview is attained of possible deductions and applications that can be made for human welfare. While making these deductions, however, we are gradually engaging in research that bears on possible, non existing objects and phenomena, and at the same time we are creating them. But the creation of these *artificial entities* cannot be left to free choice : some of them may help us in the pursuit of our goals, but others can be destructive : you can do research that leads to the making of new medicines but also of poisons; you can make plutonium for energy production or for bombs. In general, every bringing forth of new phenomena and objects may cause a change in the delicate balance of human beings and their biological, chemical and physical environment.

Moreover the possibilities of creating new domains of entities seem to be unlimited. As scientific methodology itself provides us with no norms to impose restrictions on the production of new objects, and the creation of new research domains, and as some of them are clearly dangerous, it is imperative that on the one hand *quasi basic research* should be subjected to clear *limitations* and that, on the other hand *priorities* should be imposed as far as the engagement in accepted areas is concerned. This demand for restrictions and priorities is not trivial, as it would be if quasi basic research and applied research were confounded. Most people agree — in principle at least — that it would not be a reasonable scientific aim to try to invent useless or dangerous technological objects such as airplanes with bird wings or poison gases; but when *theoretical* science is at stake, many scientists are still inclined to think that intellectually interesting or fascinating problems have a right in themselves to be solved. My thesis is that, with regard to the field of completely new (man made) entities and phenomena (new radiations, fission or fusion chain reactions, isotopes, polymers, new or genetically modified viruses and cells, changes of personality...) this is no longer the case. It has been said that, from a purely intellectual point of view, the bringing about of the first thermonuclear chain reaction was an exciting task; if we consider this a sufficient reason to engage in that kind of research, what are we supposed to object when some Mr. Strangelove would find it a fascinating problem to make a virus that could kill all human beings ?

Apart from the dangers that may be linked to certain types of these enquiries, the mere fact that there seem to be no clear limits to the number of research directions that could be taken into the realm of the "possible", makes it imperative to propose priorities even after

some directions have been excluded⁶.

It is impossible of course to present by now a detailed set of criteria, but three basic ones seem to arise naturally in this context.

Quasi basic research may be admitted (a) when it is suggested directly by basic research as an important means in the test of its theories, or (b) when there are reasonable chances that the research may lead to the relief of human needs and the realisation of generally accepted goals. Of course, in order to consider this condition as realized, it will not suffice to say that *any* kind of research can lead to useful applications, because we are looking precisely for arguments to give priority to one type of research rather than to another. (c) In any case, a serious investigation should be made about the possible hazards connected with the program and the risks should be weighed carefully against the extent of possible benefit.

The criteria (b) and (c) are certainly very difficult to apply in the present situation. As we have said already, there are no generally accepted goals of human society and so the danger will remain, that the benefit of a nation may be taken for the welfare of mankind as a whole, whereas in fact it may lead to disaster. In the second place the deeply rooted optimism of most scientists and their enthusiasm for their work may cause them to underestimate the dangers inherent in certain types of research. Most of all, however, they have to be convinced that the distinction between basic and quasi basic research is really possible. Indeed, I am willing to accept that there is a boundary area where the discrimination is difficult to make and some of my examples may not be well chosen; that would not necessarily remove the rightfulness of the distinction between research of possible and research of existing entities; the problem is that only specialists of the various fields can show us where the boundaries lie.

Once the distinction is admitted, the sense of the exclusion and priority criteria becomes clear. Whereas in basic research the quest of knowledge for its own sake is accepted, and in applied research the direct aim should be the benefit for mankind, one could not engage in *quasi basic research* without proving beforehand either that it is elicited by the necessities of basic research, or that there are serious reasons to hope for a substantial benefit for mankind. In other words, the priority order in basic research is an *intrinsically scientific* problem; in applied research it is totally dependent on probabilities for *human welfare*, whereas in quasi basic research one should ask to what extent the programs are conducive to a positive development in the basic or applied areas: the argument that the problem is

interesting in itself will not suffice.

It seems to me that already now, when this approach would be accepted, a fairly broad consensus could be reached with regard to some types of priorities. To give one example; with such a look on the criteria for science it would become clear very soon that the enormous investments in space research and especially the Apollo program, could by no means be considered as part of a rational development of science⁷.

5. A final analysis has to be made concerning the *sciences of man and society*. When we reflect on the way in which we tried in this paper to integrate the human activity that is called science, in a goal hierarchy, it appears that *man* as an individual or social being has been taken as the starting point with regard to the value hierarchy. In *basic research* his culture and society are at the beginning of the reduction chain and the aim of science at each level is to know how the corresponding entities are structured and to find out what are the necessary conditions on the foregoing level that can explain the laws and properties of the more complex one. We pointed out that the more elementary are the levels we study, the more necessary or inevitable is the existence of the objects concerned. a higher level of complexity goes along with a higher degree of contingency. Thus we have to conclude that the point of departure of science, the most original problem is man, the most contingent of all beings. As far as *quasi basic* and *applied* science are concerned, man is at the beginning too : the questions are asked in function of his values and goals, which are even more contingent than man himself.

To say that something is contingent, means that it has no necessary existence and that there is a possibility of *changing* it as soon as the required knowledge is obtained. When a situation is experienced as undesirable, the realisation of the power to change it will lead inevitably to a new situation that is more compatible with the prevailing value hierarchy.

Now, while the sciences concerning human individuals and societies are developed, the insight in their contingency will be growing too and consequently the possibility of *changing* their laws and characteristics will become apparent. And spontaneously it could be taken for granted that also on this level changes will be introduced when a situation is incompatible with the accepted values. But the value hierarchies that would direct these changes are themselves part of human society and culture and thus belong to the domain of the most *contingent, non-necessary* things.

The consequences are important. The *precedence* we gave in basic

research to the study of what there *is* over what *could be* (e.g. study of existing cells and organisms, instead of the possible ones) was based on the fact that the existing objects on each level are necessary conditions for the more complex ones, and ultimately for the existence and characteristics of human individuals and societies. But once it is recognized that many properties of man and society are liable to change, it can be asked whether on this level there are still reasons to prefer the study of what is, over that what could be. To answer this question we have to examine two hypotheses.

(a) We can suppose for a moment that there is an immovable goal hierarchy. In this case the preference for the study of the existing things would be justified if it is indispensable to bring about changes in a direction compatible with the goal hierarchy. But is this always the case : Is it necessary to know all existing types of education and all their undesirable results (poverty, criminality, neuroses, etc.), before one is able to develop a system in which these drawbacks could be avoided ? I submit that it is easier to imagine a type of education in which children will not become criminals than to make a complete study of all the factors that generate criminality in our societies. Likewise it may be easier to develop a strategy to avoid economic crises or to reduce their negative consequences, than to construct an exact theory to explain and forecast all crises in our economic system. Although a more thoroughgoing analysis of this issue is needed, it seems not evident that, in our first hypothesis the study of facts should be preferable to the study of the possible; or, in other words, it is not sure that basic research should still retain the type of priority it was assigned to on the other object levels.

(b) For the second hypothesis, we have to take into account that the goal hierarchy of individuals and societies is itself contingent and liable to examination and change. If there were a universal agreement among men about their aims and values, the acknowledgment of their contingency wouldn't cause much trouble : one could decide that the goal hierarchy cannot be changed without general consent. Now, however, some of the values of individuals and societies are not only different, but even contradictory. As everybody tries to attain his own ends, it is very likely that this opposition of goals must lead to struggle and involves a continuing danger for mankind in general. In order to arrive at a rational world situation (a situation in which the attainment of goals is maximized for everybody), it seems advisable that we arrive as soon as possible to a universal consensus concerning value hierarchies. Consequently it is far more rational to find out how we can bring about a common value system, than to examine the actually existing goals and values.

I am not advancing, of course, that research bearing on existing situations and processes cannot be valuable in social science; the point is that, on this level, research concerning the facts is not *necessarily* preferable to an exploration in the domain of the possible. In the sciences of man and society facts are perhaps not always "more honorable than a lord mayor". Whereas on the other levels a kind of autonomous value was assigned to *basic research* — because it procures insight into the necessary conditions of what exists *and* of what is possible on the human levels — the distinction between basic and quasi basic research begins to fade in the study of man and society. In other words, with this kind of research, it becomes difficult to stick to the ideal of knowledge for its own sake : *every* research program has to be subjected to criteria of priority. These criteria themselves are based provisionally on the existing goal hierarchies, but could be made more rational when the tendency to unification of all value systems is built in as a central goal in all of them.

Our conclusions may be summarized as follows. Science and its development have become a worry. Those who defend the rightfulness of completely free research have to adduce their arguments; when this is done, it appears that they are at most applicable to basic research, but fail to justify quasi basic research : there we need the assignment of priorities. But when this distinction is applied to the sciences of man, new difficulties arise : on this level, basic research can hardly be retained as an ideal; here the *values* should be at the core of every research decision and the facts are only worth studying when their relevance to the values can be ascertained. The essential characteristic of the sciences of man in contrast to the 'natural sciences' has sometimes been sought in so called 'emergent' qualities, which escape the possibility of reduction to other levels. It seems to me that their original trait shows up only when the *goals* of the scientific enterprise are analyzed and when one examines how science can become a completely rational human activity.

NOTES

¹ Cfr. e.g. Vermeersch, E., Rationality, some preliminary remarks, in *Philosophica*, 14, 1974, pp. 73-82.

² Cfr. Easly, Brian, *Liberation and the aims of science*, London, Sussex U.P., 1973, p. 347.

³ Oppenheim, P., & Putnam, H., Unity of science as a working hypothesis, in *Minnesota Studies in the Philosophy of Science*, II,

Minneapolis, Un. of Minn. Press, 1958, pp. 3-36.

⁴As we are only interested here in the general idea, I simplify somewhat the precise definitions of the authors.

⁵For the convenience of the exposition I suppose here that there is no more elementary level than that of elementary particles; but it is clear that, if there were one, it would have the characteristics of generality and necessity attributed here to elementary particles. This doesn't change the core of the argument.

⁶(a) It would be difficult to give clear-cut examples of what has to be considered as "quasi basic research", as only the specialists in the field are able to distinguish work that is concerned with the reduction links, from studies that are mere excursions into the domain of the possible. We may suppose, however, that the definition is applicable to parts of plasma physics and solid state physics, of research concerning particle acceleration, thermonuclear energy, astrophysics, space, superconductivity, quantum electronics, electron optics, parts of anorganic and organic chemistry, of molecular biology and genetics, of behavior control and artificial intelligence.

(b) The boundary between 'quasi basic research' and applied research is not always easy to trace either. But the rationale of the distinction may be illustrated with the following example. When Fermi and Hahn tried in the mid 1930s to bombard uranium with neutrons, to find out what would happen, this could — in that stage of the development of science — be considered basic research. When afterwards Szilard, Fermi and Oppenheimer worked on the project of developing nuclear chain reaction either in an explosion or in a controlled way, this was quasi basic research. The problem of using this uncontrolled and controlled chain reactions to make a bomb or for energy production, is typical of applied research. Likewise, the objective of realizing a fusion chain reaction — controlled or uncontrolled — is a problem of quasi basic research; the application of the invention to produce clean energy and 'clean' bombs is, of course, applied research. The fact that quasi basic research may be part of a manifestly technological project (e.g. the Manhattan Project) doesn't change its intrinsically 'quasi basic' character.

⁷Of course, space research and the Apollo program were developed in the first place for military and political reasons, but my argument is directed against those who have tried to present it as a scientifically sound enterprise.