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Why do we need fundamental research?

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Research is considered as a major component of innovation and a key to the development of modern societies. However, fundamental research, which essentially aims at improving our understanding of Nature, is often questioned about its specific role. In this paper, arguments of 'general interests' in support of fundamental research are presented to contribute to the science-policy debate. Beyond a notable return on investment, now acknowledged by most economists, a number of positive societal impacts of fundamental research can be underlined. Fundamental research helps society as a whole, as well as individual firms, to keep options, possible scenarios and choices open (e.g. in relation with sustainable development), to maintain a good capability for top-level scientific expertise, to develop conditions favourable to scientific and technological breakthroughs, to ensure training at the highest possible level and also to guarantee access to, and free circulation of, the most valuable information. Finally, fundamental research may contribute to a better structural link between science and society.

Introduction

The strategic importance of research for innovation and, consequently, for economic and social progress, is universally acknowledged.[1] However, research is increasingly regarded as one resource amongst others (notably financial) which it is necessary to mobilize optimally in order to respond to market demands, in the face of keen worldwide competition, and to society's expectations. For instance, the ambition of the European Council, Parliament and Commission is to raise Europe to first place in the coming knowledge-based economy.[2,3] Towards this goal, the Sixth Framework research programme of the European Commission, for the period 2002–2006, is explicitly focused on major socio-economic issues. In this context, the place of applied research seems relatively clear and undisputed. On the other hand, fundamental research, when it is perceived as an activity

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without any direct link either with the market or, more broadly, with society's immediate 'demands', is often the subject of questioning as to its specific role. In fact, whereas it is clear that a significant part of national and European research programmes must be mobilized visibly and effectively in order to solve the concrete problems that are posed (sometimes even before a 'social demand' becomes apparent!), much experience also shows that applied research rapidly withers when it is not firmly rooted in the 'rich soil' of quality fundamental research.

The necessity to carry out fundamental research may not be openly contested but its funding is rarely satisfying. Some actors may even be tempted to consider that, since the results of fundamental research are freely available in the open literature, it is much cleverer to let others make the financial effort. Moreover, the pressure on scientists to shift their interest to applied research projects and to deal with short-term issues has been constant in most institutions since for least a decade. In brief, the importance of fundamental research for European society is not fully understood and thus needs to be discussed with both decision-makers and the broader public who often have no scientific background. In addition, an appropriate funding mechanism needs to be designed and implemented at the European level to support long-term fundamental research, especially in fields where the scope now exceeds the possibilities of individual nations. This analysis has justified, amongst other considerations, the proposition to create a European Research Council, a new institution advocated by a number of actors in the European research area (individual scientists, research institutions, science policy bodies and governments).[4,5] Although created in a quite different context and with distinctive regulating principles, institutions such as CERN and ESO have been very successful attempts to coordinate international efforts in particle physics and astronomy.

In this paper, I shall examine the 'general interest' arguments that can be advanced in order better to determine the place of fundamental research in the research effort of Europe and Member States. I shall not stick to one particular discipline, or activities specific to one body or another, but will deal with the question from a very general point of view, which is also the public policy standpoint. I do not, however, claim to give an exhaustive account of the question but only to outline a few areas for consideration and present some ideas that, it seems to me, are not yet being adequately discussed. In particular, I shall not develop the purely cognitive function of fundamental research, a task that is nevertheless essential and whose long-term value to society is sometimes underestimated. This is because, on the one hand, this argument is theoretically well known, even if it needs to be continually restated at a time when our societies' horizons seem to be becoming limited to (short-term) utilitarianism, and, on the other hand, other considerations, less obvious to 'lay people', need to be

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understood and developed in the present context. I shall not develop either another positive aspect of fundamental research even though it is important to realize that, by its very nature, fundamental research progressively builds a truly international community of scientists and scholars who basically share the same values, free as much as possible from ideological bias, exchange information in the same language, coordinate efforts towards common goals, develop a solidarity and, in many cases, even friendship between co-workers of diverse nationalities. Science is one of the very few forums where a true dialogue can be maintained between people who otherwise could be pushed to oppose each other, in particular in periods of intense international crisis, as has been demonstrated during the Cold War.

A working definition of fundamental and applied research is indeed necessary, not with the intention to be normative but simply because readers of this essay need to share a common understanding. Both types of research may have, in certain circumstances and more often than in the past, common features. Some authors, particularly science policy specialists and sociologists of science (much more rarely scientists themselves), even argue that there is a continuum between fundamental and applied research and that such a distinction is outmoded.[6] This may be (increasingly) true for particular aspects of research and/or in some disciplines, e.g. in biosciences, information and communication sciences, where the distance between research, technology and market seems to constantly decrease. And they are becoming more and more inextricably linked in the so-called 'innovation process', even in some cases at the laboratory scale. Other authors may also underline the complex, and rapidly evolving, nature of research[7,8] to discard such a relatively simple distinction. Some scientists may be involved in all these aspects of research at the same time.

The fact that the interaction of fundamental and applied research can be extremely fruitful, notably because it may draw to the attention of scientists entirely new and exciting problems (as instanced by Pierre-Gilles de Gennes and Badoz[9] for soft-condensed matter) should be fully acknowledged. Finally, the typical transfer time for knowledge and know-how, from research to industrial development, is currently considered to be getting shorter and shorter. I will not comment further on that. From this point of view, each discipline and research body is subject to specific factors linked to its own objects, methodologies and agenda and also to its history and 'culture'. Therefore, it is always extremely difficult to extrapolate from a particular (sub)discipline or personal experience to the whole scientific enterprise and to make definitive ex cathedra statements about what research is, is not or should be. Indeed, a too restrictive understanding of the scientific enterprise may easily be at risk of delegitimizing entire fields of research, not directly linked to any innovation process but justified by the

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existentialist quest profoundly anchored in all human beings (e.g. paleontology, cosmology, etc).

Strangely enough, the scientific fields that are able to produce or contribute to 'grand narratives' about the cosmos, the origin of life, the emergence of Homo sapiens, etc, all topics with no direct application, seem particularly attractive to the public and are popular to science students.

A number of other terms have been suggested to describe the different aspects of scientific activities: basic research, pure research, curiosity-driven research, knowledge production orientated research and 'blue-sky' research, as well as targeted-research, problem-solving orientated research or strategic research, etc. In fact, all these terms are in common use and may be useful, because they indeed cover specific aspects of the scientific activities. None of these terms, however, could impose themselves on the whole community. The key point here is to be aware of the notions on which these terms, and their definitions, are built as well as their history in relationship to the changing socio-political context during the 20th century, notably since the end of the Second World War and the Cold War (cf. Stokes[10], Calvert[11] and the proceedings of an OECD workshop[6]).

There is a double impasse which, in my opinion, must be avoided, either insisting too much on a supposedly radical division between fundamental and applied research, which may reflect a refusal to recognize the major developments in science and technology over the last few decades, or denying a nevertheless real difference in thought, style, ideals and objectives, motivation as well as practices, which often correspond to a conception of science essentially orientated towards practical ends.

Many fundamental scientists, if not most, believe that the desire of human beings to unveil the 'laws of Nature' remains a noble ideal, able to attract some of the most brilliant minds, and which does not need to be grounded on practical purposes and mundane interests. Scientists dealing with basic cognitive issues primarily identified by the scientific community itself with no specific technological, industrial and/or social application in view (other than the understanding of Nature), are doing fundamental research. Their curiosity-driven approach, which mainly follows a disciplinary or interdisciplinary logic, usually with a long-term horizon, aims at producing new knowledge. Scientists dealing with problems primarily identified by some kind of client, customer or 'society' as a whole related to specific, and usually short term, technological, industrial and/or social applications are doing applied research. Their target-oriented approach, which essentially follows a problem-solving logic, is aimed at finding practical solutions. The results of fundamental research are essentially evaluated by the scientific community with respect to notions of excellence and originality, according to its self-regulated procedures, whereas those of applied research need to be appreciated in reference to utilitarian values and criteria. These definitions

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are compatible with those of the classical Frascati Manual[13] although articulated differently, do not take specifically into account any institutional criterion ('where the research is done') or disclosure norms ('published or patented work') (see for instance Calvert and Martin[12]) but underline a relative autonomy of the scientific community with respect to other social forces (economical, political, industrial, etc).

The importance of intentional criteria, and the practical consequences on the way research is carried out on a day-to-day basis, was underlined in the 1960s by Abragam.[14] This pioneer of nuclear magnetism argued that a major difference between fundamental and applied scientists is the choice of problems investigated and thought styles, even if 'people with similar backgrounds use similar equipment in similar surroundings to do very similar work'. I should add that who defines which problems are worth investigating or not and who evaluates the results or production of research is obviously a central issue of science policy.

Identifying reasons of societal interest to support fundamental research

In his report of 1945, Vannevar Bush[15], scientific adviser to the US President, was urging scientists to change their objectives massively and to use their skills and means, developed for military applications during the Second World War, to contribute to public welfare. His vision, which fully acknowledged the essential role of fundamental research, justified science policies in the US but also overseas. Since then, much attention was paid, in OECD Member States, to the evaluation of the output of research, notably fundamental research, from an economic perspective. The economics of science is now developed as a distinct sub-discipline.[16,17] There is now no doubt that the return on investment of research, both from a global viewpoint and for individual firms, is indeed quite high. Most studies evaluate it to about 20 to 30% annually, i.e. twice the average return of stock market investments over a long period of time.[18] Although difficult to estimate on the basis of simple empirical data, the specific contribution of fundamental research to economic performance is also now firmly established by specialists (see for instance the review by Martin and Salter[19]).

One difficulty in fully appreciating this contribution, underlined elsewhere by the same authors,[20] is that it can take a variety of forms which may be hard to quantify (increasing the stock of useful knowledge, training skilled graduates, creating new firms, new scientific instruments and methodologies, etc). The causal link between fundamental research and positive economic outputs is highly diffused within the scientific and engineering communities. In addition, it may take a long time before basic scientific knowledge is transformed, sometimes through tortuous ways, into a concrete innovation. The way from applied research

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to innovation is, in general, easier to trace back and to make visible for the public and decision-makers.

A number of official reports,[21] notably in the United States, regularly insist on the central importance of fundamental research in any responsible national science policy. Considering the opportunities and challenges of the future, the Committee for Economic Development, an influential US Think Tank financed by business and industry, considers that 'one way to think of basic research is as a low-cost insurance', the support of fundamental research which is less than 0.5% of GDP goes 'to create very significant long-term economic and social gains'.[22] World-class scientists have published papers to illustrate the value of fundamental research for society, including from an economic viewpoint. I shall only mention the paper by Llewellyn Smith[23] who was Director General of the CERN particle physics international laboratory from 1994–98, who provides numerous historical and more recent instances, showing that discoveries of enormous economic and practical importance as well as spin-offs and stimulation of industry can be expected from fundamental research, even in disciplines such as particle physics, which seems *a priori* quite distant from applications.

A more general pending question in the economics of science is to decide whether or not fundamental research (or *science* as many authors continue to call it) is a 'public good' and, as such, requires governmental support. This still hot topic in science policy is discussed in detail by Callon.[24] In standard economic theory, scientific knowledge is considered as a non-appropriable, non-rival and durable good, which classically defines a 'public good'. Due to the very nature of research, the production of scientific knowledge is accompanied by a large uncertainty. Therefore, classical economists conclude that the private sector (notably, industrial companies) tend to under-invest in this field, especially in fundamental research. In this theory of the 'market failure', public authorities have a specific role, and responsibility, to substitute for the private sector and to support laboratories doing fundamental research, in particular in academic institutions. This model is contested by a number of authors, usually sociologists or socio-economists, as being contrary to the results of empirical studies. In fact, big international companies (e.g. IBM, Unilever, Philips, Saint-Gobain, Motorola, L'Oreal, etc) do invest massively, in research and their scientists significantly contribute to publications at the highest-level. Callon argues that their scientific activity is not restricted to the production of codified knowledge as classical economists postulate, which would justify the assimilation of scientific knowledge to a pure public good. In fact, scientists also generate 'incorporated' knowledge and know-how that cannot be appropriated simply by learning, reading articles or searching information in databases but essentially by doing research oneself. The consequence of this observation, supported by all the empirical results of the sociology of science is extremely important. It demonstrates that, to understand

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fully the results of scientific research, to use them in the innovation process and for expert appraisements to maintain the highest level of education for their scientific and engineering *elites* or even to efficiently 'buy research', companies, and nations have a vital need to do research themselves to keep a decisive competitive advantage on the long run. This argument is particularly true for fundamental research, which is at the forefront of the advancement of knowledge and of the development of high technologies. Here, I will not develop further these important economic issues and will rather present additional arguments.

One major role of fundamental research, notably for governmental authorities, is to keep options, possible scenarios and choices open. Indeed, whatever conception one has of the chains of events leading from research to development and to industry (see Ref. 25), this 'course' is always accompanied by a high degree of irreversibility, linked to decisions of a scientific, technical, financial and organizational nature. This movement towards irreversibility, which is also the price to be paid for technological and industrial efficacy, has the consequence of restricting appreciably the possible scenarios and policies that can be considered. Research in general, and fundamental research in particular, allows this increasing irreversibility to be minimized by keeping options open or even creating new, sometimes totally unexpected, ones. In a world of growing complexity and consequent steady decline in any real capacity for anticipation and control, reactivity (or 'plasticity') becomes a major asset. This is on the condition that it comes within the framework of a clear, stated strategy and draws on a whole range of responses, implying knowledge and know-how of a diverse nature and which are as complete as possible.

Fundamental research enables us to set up, maintain and develop continuously (upstream, as one habitually says) this range of possibilities or margins of room for manoeuvre that are so essential for a genuine policy/political decision. Indeed, the latter requires the ability to make choices based on values (or 'visions' of the world) and must not simply result from constraints alone. This 'political' role of scientific knowledge justifies, in itself, a vigorous public support of fundamental research.

Numerous historical cases can be given to exemplify the contribution of fundamental research to such a major policy issue. For instance, the scientific discoveries in the 1930s of artificial radioactivity (Curie and Joliot), nuclear fission (Strassmann and Hahn) and chain reactions (Meitner and Hahn) led, within a few years, to nuclear technology, with its highly unexpected military and civilian applications. The capability to produce weapons of mass destruction and large quantities of energy at relatively low cost changed drastically the geostrategic equation. The introduction of this form of energy production, which is not a simple innovation but rather a kind of revolution, has helped developed countries reduce their energy dependence after the first oil crisis in 1973.

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A major political and economical issue for the future will probably be the promotion of sustainable development. This drastic reorientation of all our activities would not occur spontaneously if the public, decision-makers at all appropriate levels and the scientific community itself do not specifically focus all efforts towards this survival goal. The concept of sustainable development introduced by the Brundtland report[26] in 1987 defined 'development which meets the needs of the present without compromising the ability of future generations to meet their own needs'.

The notion has been reinforced by the Rio Earth Summit of 1992 and, since then, considered with increasing attention in the political agenda of a number of nations, in particular in Europe. This concept is interesting because it gives meaning, direction and coherence to a number of policies that otherwise, would remain sectorial. Sustainable development is basically a process that encourages the reconciliation of economical and social interests with environmental protection. It stretches the political and economical action to both long-term (the time span of future generations) and global considerations (the Earth as a whole). Finally, it introduces ethical criteria for evaluating the action of actors who are expected to enter into a virtuous circle and contribute to the equitable sharing of economic outputs and to the well-being of all Earth's inhabitants.

There is no doubt that implementing such a (utopian?) programme would profoundly change the lifestyle of numerous citizens, notably in the richer countries. Sustainable development is far from being a mere status quo, contrary to a current opinion. This revolution implies the solution of extremely difficult issues concerning, in particular, the preservation of natural resources and the protection of the environment, the production, transport and consumption of energy, the development of clean and sober industrial processes, the management of all sorts of wastes (including very long-lived and toxic ones) and so on. A vigorous research effort in a wide range of disciplines (including social sciences and humanities) and international coordination will be necessary to make, at least, some progress towards this goal. The problems that we will have to face are so complex and, in many cases, so unusual that a strong contribution of fundamental research will be called for. For instance, many scientists consider that nuclear fusion could be an environmentally acceptable answer to the increasing need of energy of both industrialized and developing countries in the future, as its basic 'fuel' (hydrogen) is an almost unlimited and equally distributed natural resource. However, beyond the very difficult technological problems, the development of nuclear fusion will be viable only if advances in very fundamental problems, notably in plasma physics, are made.

If we consider the management of some industrial wastes that will remain dangerous for extremely long periods of time, such as heavy metals (cadmium, lead, mercury, etc), these compounds can be confined in very stable solid matrices

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and disposed in multi-barriers (near-)surface or underground repositories: technological and industrial solutions already exist. However, many citizens (not only environmentalists) ask for increasing assurances as to the long-term behaviour of these matrices in such geological environments, far beyond the periods that can be tested directly in our laboratories. Again, fundamental problems concerning the basic mechanisms involved in solid/liquid interactions, the modelling of heterogeneous systems as well as the use of natural analogues of materials, alteration and transport processes in geological media need to be tackled. A final example will be that of climate change, an issue for which intensive research efforts in a range of fundamental problems are needed, including palaeo-climatological variations at different time and spatial scales, CO_2 and other greenhouse gas cycles, etc. In all these cases and many others, coordinated international efforts in fundamental research will be needed, perhaps for decades.

Making appropriate public-policy decisions between alternative options and scenarios implies the availability of top-level scientific expertise in a wide range of issues. In effect, Member States and other institutions (including NGOs) have to deal with complex and controversial topics, such as the protection of the environment, health-products safety, electromagnetic fields, biotechnologies, GMOs, etc. The defence of the general interest may not result from the actions and interests of increasingly powerful private companies developing their activities worldwide. Therefore, a majority of citizens would probably agree that public decision-makers need competent and independent scientific and technological advice, a good capability of expertise becoming an essential asset for governments in a time of globalization. Expertise, be it individual, collective or collegial, mobilizes a diversity of skills, know-how and access to the various networks of the scientific community (see below) which only active research, including fundamental research aiming at international excellence, can provide. Research laboratories of private companies can obviously nurture excellent scientists; however, it is also clear that public decisions should include the advice of independent experts, specifically when huge interests, notably financial, are in debate.

Consolidating competitive advantages in the world economy, compatible with sustainable development, for European companies and Member States is obviously a major strategic goal for all decision-makers. This implies, amongst other measures, to maintain or even develop conditions favourable to scientific and technological breakthroughs. Here, there may be a misunderstanding about the word ‘breakthrough’, which is often misleadingly used to describe a simple incremental scientific advance translated into a significant innovation. This kind of improvement of knowledge and know-how usually relies, at least in first analysis, on the output of applied research and development. But actual

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breakthroughs, true discoveries, unpredictable and radical changes of 'world views' (moving from one paradigm[27] to another) can only emerge from fundamental research.

As de Gennes used to say, electricity was not discovered because scientists were urged to improve candles (nor was atomic physics developed because one was trying to produce electricity). In fact, electricity has been made possible because of the research of Faraday and the theory of electromagnetism developed by Maxwell around 1873. Who would deny that electricity is an innovation that profoundly modified modern societies? Similarly, X-ray radiography was not made possible because medical doctors, or anyone else, wanted to see through human bodies, but was an unexpected discovery in 1895 due to Röntgen who was studying the passage of an electric current through a gas at very low pressure, a purely fundamental research programme with no specific application in mind. Who would regret *a posteriori* the investments which were certainly made to obtain this major advance for public health? Another good example is that of lasers, with their wide range of applications from ophthalmic surgery to entertainment devices such as CDs and DVDs. None of these applications would exist without the work of Kastler in fundamental optical spectroscopy, who discovered in 1952 the method of optical pumping which led to the discovery of lasers by Townes, Shawlow, Prokhorov and Basov.

A further instance would be the discovery of DNA structure in 1953 by Crick, Watson and Wilkins. This major discovery, was for Crick and coworkers, no more than a fascinating scientific game but it is the foundation of molecular biology and modern biomedical sciences. There is no doubt that a hypothetical committee of experts evaluating all these research proposals according to present-day procedures would have rejected them because they could see no likely socio-economic impact!

Beyond these historical instances, Comroe and Dripps[28] tried to measure the actual role of fundamental research in their own field (biomedical sciences) on more quantitative grounds. Ten major advances in cardiovascular and pulmonary medicine and surgery, developed from 1945 to 1975, were selected by a complex process involving tens of physicians, in an effort to avoid as much bias as possible. The so-called 'essential bodies of knowledge' necessary for these advances to occur and the key articles that contributed to them were then identified with the help of 140 consultants. Having browsed thousands of articles, Comroe and Dripps concluded that nearly 62% of the crucial articles dealt with fundamental research, considerably more than results of applied or clinical research. In addition, about 41% (variable according to the speciality), judged to be essential for further clinical advance, were even not clinically oriented fundamental research. According to Comroe and Dripps, 'the scientists responsible for these key articles sought knowledge for the sake of knowledge'. It is fair to mention

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that the conclusions of this seminal study were criticized by Smith[29] in 1987 and very recently revisited by Grant *et al*.[30] in the case of neonatal intensive care. Using another methodology based on a bibliometric protocol, they suggest that, after a time-lag of 17 years, up to 21% of clinical advances are underpinned by fundamental research, a much less substantial score than that of Comroe and Dripps, but still significant in some cases. However, Grant *et al*. caution against the over-interpretation of their results and advocate more research to understand better the link between fundamental research and healthcare.

Another possible way to evaluate quantitatively (as much as possible) the contribution of fundamental research to innovations was proposed by Narin *et al*.[31]. These authors analysed the scientific references in about 400,000 US patents, citing the relevant literature, and showed that about 50% of the cited papers were authored at academic institutions. They concluded that 'public science plays an essential role in supporting US industry' and that, in addition, this link is increasing. When analysing the universities involved in such works (often top US research universities, e.g. Harvard, Stanford, Johns Hopkins, Yale, Cornell, etc) and the scientific journals where the studies are published, there is no doubt that fundamental research contributes at a notable level. A key feature of true scientific and technological breakthroughs is their unpredictability, even for the best scientists themselves. One famous erroneous prediction is due to Ernest Rutherford, one of the most brilliant physicists of his time, who declared in 1933, only a few years before the discovery of nuclear fission, that 'Anyone who expects a source of power from the transformation of these atoms is talking moonshine'.

Fundamental research, precisely because of its relative independence from society's demand and short-term needs, can tackle basic issues and long-term problems from which scientific revolutions and paradigmatic shifts, in Kuhn's terms, can proceed. Supporting fundamental research now is just making possible major innovations in the long-term future that may change our entire lives or those of the next generations.

One major contribution of fundamental research in the world competition economically dominated by the rule of ceaseless innovation is to ensure training at the highest possible level. The importance of training through research for the education of scientific and engineering elites, has been stressed for many years. Their finely honed scientific and technical skills can also be exploited in many other fields. These include working methods combining the greatest possible rigour with the ability to adapt to a 'stubborn reality', tenacity in the face of lack of success, the ability to go beyond or even transgress known concepts, methods and solutions etc.

Training through research is also a process of 'socialization' of the young researcher within the scientific community (which, in fundamental research, can only be international). This apprenticeship leads him/her gradually to adopt a

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system of basic values of science:[32] universalism, the idea of community, disinterestedness, integrity and organized or methodological scepticism. Even if this 'ethos' (to use the expression of Merton, one of the founders of the sociology of science) is mainly an ideal, sometimes mishandled, these qualities are still very meaningful to scientists. Curiously enough, these values of the scientific community are perfectly compatible with very fierce, even 'savage,' international competition in certain fields.

It seems to me extremely useful for future elites to have early formative experience of this balanced mixture of values. The education of world-class scientists guarantees access to, and free circulation of, the most valuable information. In this respect, the usual discourse on information seems to miss a basic dimension when it focuses exclusively on access to databases and other explicit sources of information. These are, of course very important, and management of access to these now very rich and diverse resources must obviously be developed. In this connection, all experienced observers note a tendency towards privatization of (even basic) knowledge, which in the long term may considerably limit access to it, or at least freedom to use it. This is true, for example, in the case of some basic knowledge in the field of biotechnology (e.g. specific DNA sequences, etc). This trend is strongly criticized, not only by fundamental scientists and their institutions (e.g. the US National Research Council) advocating free access to databases and free exchange of information,[33] but also by scholars warning about its counter-productive economic effects. For instance, Reichmann[34] argues that reinforcing the legislation is needless, as the present copyright laws ensure sufficient protection of original and creative data. Moreover, in his opinion, a historic rupture in the common practice of giving access to abundant data to the scientific community at the marginal cost of dissemination would 'cause permanent, incalculable damage to scientific and technical progress'. Reichmann considers that free access and exchange of information is largely responsible for the dynamism and competitiveness of US technology.

However, as already discussed previously, the most valuable information, which is not available from any database, is 'incorporated' in the implicit knowledge and know-how of scientists themselves. To have access to it, one must be able to circulate physically within the scientific community (exchanges of personnel, post-doctorates, long-term visits by senior scientists, collaborations of various kinds, etc).

Because of the considerable stratification of this community, this movement between the best laboratories is only possible for scientists with sufficient professional credibility. For the most competitive sectors, recognition by the community represents an 'admission ticket', with a very high price, only accessible to the best scientists or teams. This excellence in fundamental research,

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which is very fragile, can only be attained by sustained long-term effort and a recruitment policy capable of attracting the best people and, above all, creating favourable conditions for keeping them and maintaining their motivation (excellence, openness to knowledge and constant creativity, freedom etc).

Empirical studies have moreover shown that the most outstanding industrial laboratories are themselves actively seeking this integration into the international scientific community network (thus implying that scientific and technical intelligence-gathering from databases is manifestly inadequate). For instance, the study of Narin *et al.*[31] on the scientific papers cited in US patents clearly demonstrates that top industrial laboratories (IBM, General Electric, Motorola, Xerox, Hewlett Packard) do indeed actively contribute to international journals in a range of disciplines. This illustrates their strategy of publication of high-level scientific results – which they might normally tend to keep for the benefit of their company alone – to obtain recognition of their researchers by the scientific community.

Finally, I consider that fundamental research could (and should) contribute to a better structural link between science and society. This controversial question, which of course constitutes a major issue in the debate, is becoming a strategic goal for both sides. No one contests the idea that science must, in some way and at more or less long term, respond to society's expectations, just as does the justice system (in terms of fairness, speed, proximity etc) or other institutions that are part of the modern world. However, in a society where the crisis of mediation is increasingly visible (the crisis of representation and of intermediary bodies in particular), the real question is to identify the places, processes, human resources and other means to be utilized for the purpose of redefining the objectives of the research effort at the European and national levels (not only in the short term, but also in the medium and long terms) collectively and, ideally, by consensus.

In particular, the place reserved for fundamental research, which must remain characterized by a very large degree of freedom and by aims that are not immediately utilitarian, must not be sacrificed, since science, like justice, cannot be reduced to that which society expects (which is, moreover, changeable) at any given time, even supposing that one could identify it. Thus, the stress so frequently placed on response to 'public demands' as the only objective (according to a model of the 'customer/supplier' type operating, implicitly, in the short term), assumes that the very question that needs to be discussed has already been resolved. This cannot therefore simply be an obvious matter, to which every other consideration should be subordinated, unless one fails to appreciate that the idea is, in itself, problematic. Does the public really formulate demands that are, in addition, exploitable in terms of research programmes? Through what media? What public? Who are their legitimate and uncontested spokespersons? Does the historical record show us that scientific and technical progress, from which the population

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benefits and which is acknowledged as such, really results from a response to 'demands' or even to explicit needs? What do we do about demands that cannot be met financially (including at international level, for example diseases, endemic to the Third World)?

In fact, the major problem concerning this structural link between science and society seems to be posed in another area, just as all societal questions are in general, i.e. that of the final goals pursued. The necessary discussion of this question cannot ignore an authentically philosophical approach for which the world of research is not prepared and to which it is not, *a priori*, very favourable, since its standard position is to affirm the neutrality of science. In addition, modern societies themselves do not seem to desire questions about their final goals and rather behave like a somnambulist walking around a bottomless hole. Consequently, it seems to me that what is most urgently required are measures to favour the emergence of places and procedures that will enable all those concerned (decision-makers, the public, scientists, market operators, the media and many others) to work out together the necessary intermediation between science and society or, to use the classical formulation, between 'facts', 'values' and 'utilities'. Experiments have been tried along these lines (citizens' conferences, work carried out within a parliamentary framework, actions by certain associations in the medical field, etc), but the framework, means and procedures have not yet been firmly established. The role of the media, which should contribute to more widespread awareness of the scientific culture and its natural integration into the general culture, is certainly a fundamental one. Furthermore, it is imperative for scientists and engineers, who by their very nature play central roles in research, to be more active in this redefinition of the structural link between science and society that is currently in progress. In concrete terms, this undoubtedly presupposes greater mobility on the part of researchers, in the course of their careers, between different functions (research, teaching, administration, expertise, business etc), both enriching their personal experience and helping them to have a better grasp of the various facets of science and technology and its impact on our societies. This mobility must not only be encouraged but, in order to be effective, should also enhance the status of those who make this effort. This presupposes an in-depth review of criteria and procedures for assessment and promotion of researchers in a large number of institutions.

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