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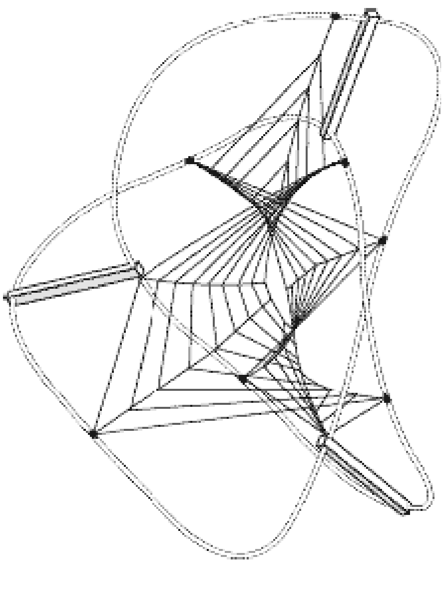

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By Nils Roll-Hansen

Editor's Note

This paper was presented at the 'Institutionalising Epistemic Standards for Science' workshop held on December 1st 2008. In this paper, Roll-Hansen presents an argument as to the fundamental importance of distinguishing basic and applied scientific research for the history of science, science studies and science policy. These are areas that Roll-Hansen argues have suffered for overlooking the distinction's importance since the 1960 and 70's. After a careful and critical review of the areas and ways in which the distinction is overlooked and under-valued, Roll-Hansen argues for a re-introduction of the distinction to greatly improve theory and practice in the politics of science.

Abstract

The term "basic research" is ubiquitous in present debates on policy in science and higher education. It is a term with positive connotations, important for example in legitimising traditional universities. The popular term "research" generally corresponds to "research and development" (R&D) in official statistics. This includes "applied research" and "technological development" as well as "basic research." In public discourse these distinctions are regularly overlooked. "Research" simply includes" the whole of R&D. The public neglect of the differences corresponds to a trend in science studies. Within present historical, philosophical, sociological, economic, etc. studies of science and technology there is a strong tendency to play down the difference between applied and basic research, or between science and technology, or explicitly to reject it as relevant to the politics or governance of science. This paper argues that more respect for these differences would improve the chances of developing a politics of science to serve society as a whole and not only the special interests of certain groups, be they private enterprises, political movements, particular religions, the scientific community, or others.

Introduction

This paper aims to recover and demonstrate the present relevance of an understanding of basic science that was still widely taken for granted half a century ago. It was then often called "pure science" or simply "science." On this view truthfulness is the root value and scientific research is perceived as a search for truth that ought to be as free as possible from the interference of particular economic, political, ideological and religious interests. This Weberian ideal type “science” was championed in the aftermath of World War II by defenders of a classical liberal and enlightenment political ideal, like Karl Popper and Michael Polanyi. It was most famously codified by the sociologist Robert Merton in his “ethos of science.” By the early 1960s this classical liberal ideal of science was being eroded by a theory primarily interested in its contribution to economic growth. The new wave of political and intellectual radicalism in the 1970s reacted against bourgeois matter-of-factness with inspiration from Marxist theory of knowledge. In spite of political confrontations government economic planners and intellectual radicals alike perceived the academic “ivory tower” as a main obstacle to a socially progressive science and technology: Political so-called democratic control of science appeared more important than its autonomy. I argue that present politics of science is still reluctant to squarely face this ambiguous ideological legacy.[1] The present paper will focus on the economic framing that has dominated science studies and science policy since after 1960.

My distinction between basic and applied research reflects the difference between science and politics as social institutions. Science is dedicated to managing and increasing knowledge of general validity, and basic research is its dynamic element. The role of politics is to produce agreement, decisions and collective action. Applied science can roughly be understood as the area of intersection between science and politics. It depends highly on advanced scientific knowledge and methods but is dedicated to the solution of practical economic, social and political problems rather than the further development of such knowledge and methods.

[1] This historical perspective is developed in my account of how misconceived theory and policy of science drove the most famous twentieth century case of state supported pseudoscience: Nils Roll-Hansen, “Wishful Science: The persistence of T.D. Lysenko’s Agrobiology in the Politics of Science”, OSIRIS, 23 (2008): 166-188.

The purpose of my distinctions between basic research, applied research, and technological development is not to separate and isolate or separate these scientific activities but to grasp the differences in order to understand and further the interactions. As indicated these are ideal types and not sharply defined categories for precise classification. In spite of all the overlap and intertwining they carry different expectations and have different social effects, which the politics of science has to address.

1. A traditional conception of applied and basic research[2]

This paper discusses three kinds of difference between applied and basic research:

1. Differences in criteria to judge the success or failure of the research.
2. Differences in effects on social processes.
3. Differences in organization, especially in degree of autonomy to political and economic interest and goals.

According to the traditional view it is the correlation between differences in all three respects that makes the distinction between applied and basic research important in the politics of science.

The primary criterion of success in applied research is contribution to the solution of specific practical problems. Practical technical success is the superior yardstick for evaluating applied research both in advance as projects and retrospectively in terms of results. Scientific competence of researchers is an essential condition for success, but it determines neither the choice of problem/theme nor the satisfaction of the funding patron. Applied research is funded by government agencies, private firms, non governmental interest organizations, etc.

[2] The rapidly increasing organizational differentiation of scientific activities from the middle of the nineteenth century onwards regularly refers to this kind of distinction. Applied research in universities was growing to a volume that demanded clearer rules of responsibility and public accountability and led to the establishment of new special institutions for task in applied research. For instance new institutions for practical research regarding fisheries, agriculture, geological surveying, social statistics, etc. were in a relatively short time established in most European and Western countries.

to further their respective purposes in terms of social and medical improvements, economic profitability, ideological and political acclaim, etc.

Basic research, on the other hand, is successful when it discovers new phenomena or new ideas of general interest. The general scientific interest is judged in the first instance by the discipline in question. But in the long run the promotion of other scientific disciplines is essential, and in the last instance the improvement of our general world picture is decisive. The aim of basic research is theoretical, to improve general understanding. It has no specific aim outside of this. But it is, of course, not accidental that improved understanding of the world increases our ability to act rationally and efficiently. It improves our grasp of what the world is like and is thus also a basis for developing efficient technologies. Some degree of realism with respect to scientific theories is inherent in basic research in this sense.

The social effect of applied research, when successful, is solutions to practical problems as recognized by politicians, government bureaucrats, commercial entrepreneurs, etc. It is an instrument in the service of its patron. Applied research helps interpret and refine the patron’s problems to make them researchable, and then investigates possible solutions. The practical problems of the patron set the frame for the activity. Applied research is in this sense subordinate to social, economic and political aims. Rewards are primarily for results that help the patron realize his purposes.

The result of basic research, when successful, is discovery of new phenomena and new ideas of general interest. By shaping our understanding of the world the discoveries of basic science become preconditions for any precise formulation of political and other practical problems. Sometimes basic research has a direct and dramatic effect by discovering new threatening problems and thus immediately setting a new political agenda. The present grave concern over climate change is a striking example of how politics is completely dependent on science to assess the problem, i.e. make educated guesses about its future magnitude and development, and think of possible countermeasures.

The differences between applied and basic research in content, in social effects, and in criteria for success imply a different relationship to politics. Science does not only provide means (instruments) for solving tasks or problems set by politics, it also shapes social and political values and goals. Applied research is generally well adapted to serve the first task while basic research is best suited for the second. From the point of view of liberal democratic decision-making there is an important distinction between solving recognized problems and introducing and formulating new problems. In the first case science has an *instrumental* role subordinate to politics. In the second case the role is politically *enlightening* and depends on independence from politics to work well.[3] When science is asked for advice on a fearful threat like climate change, which has not yet materialized but is only a prediction about future events, the importance of autonomy becomes particularly acute and correspondingly hard to maintain.

2. Statistical classification of scientific research

In the early 1960s the OECD set up an international comparative system for research statistics covering scientific research in a broad sense. This statistics was motivated by the OECD's aim of furthering economic growth and development and therefore included not only "basic research" but also "applied research" and "experimental development." The inclusive OECD

[3] This liberal democratic argument for autonomy of science was important in science policy debates in the period around World War II in middle of the 20th century. It was supported not least by critics of totalitarian regimes, in Soviet Russia and Nazi Germany, like Michael Polanyi and Karl Popper, both emigrants to Britain from Central Europe.

classification followed a growing tendency to identify scientific research with "research and development" ("R&D"). Within a few decades this new inclusive concept of "research" had to a large extent taken over the role of the traditional concept of "science" in public discourse.

However, the OECD definitions which were introduced in the 1960s, still serve as the basis for international research statistics:

> R&D is a term covering three activities: basic research, applied research and experimental development. … <u>Basic research</u> is experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundation of phenomena and observable facts, without any particular application or use in view. <u>Applied research</u> is also original investigation undertaken in order to acquire new knowledge. It is, however, directed primarily towards a practical aim or objective. <u>Experimental development</u> is systematic work, drawing on existing knowledge gained from research and/or practical experience that is directed to producing new materials, products or devices, to installing new processes, systems and services, or to improving substantially those already produced or installed (OECD 1981: 25).

These OECD categories and definitions have been much criticized for not being sufficiently clear and objective for a dependable and stable statistics. For instance the central distinction between basic and applied appears highly dependent on the subjective attitude of the researcher. And individual projects will often include so much both of basic and applied aspects that they do not comfortable fit either category. The balance may also shift as a project develops. Over time a statistics built on such flexible categories seems liable to register changes in fashions and attitudes more than in substance of research.

In spite of such weaknesses the three categories of basic, applied and experimental development are still being used for OECD's research statistics. The terms "basic" and "applied" still appear to be indispensable in public debate on the politics of science. It is widely assumed that "basic research" designates a valuable social activity in need of defence against commercial and bureaucratic inroads. And, however imperfect, the statistics based on these terms do in a useful way monitor changes over time as well as contemporary differences between countries. What causes trouble is not so much the uncertainty of the statistical numbers but their sloppy use due to terminological equivocation.

There is a tendency both in scholarly studies and in the practical politics of science to concentrate on the inclusive category of R&D and neglect the subcategories. This obscures critical problems concerning the distribution of resources between basic science, applied science, and experimental development. Such decisions always depend on what, in a certain country, are the specific pressing practical problems and what are the opportunities and general social needs for basic research. What is the best balance will vary between countries because of differences in existing economy and political goals. Keeping up an independent and strong military capability, for instance, demands large investments in development of defence technology. Similarly, countries with a large high tech industry, producing pharmaceuticals, advanced electronics, etc. will have high investment in applied research and technological development. In a country mostly dependent on exporting semi-fabricated or raw materials there is simpler and much smaller market for such research.

In public debates on science policy the words “science” and “research” mostly vacillate between a broad sense corresponding to “research and development” (R&D) and a narrow sense corresponding to “basic research”. Such ambiguity produces confused debates for instance concerning proper levels of funding or social usefulness of investment in scientific research, and eventually produces poor decision-making.

3. An example of confusion

The recent Norwegian debate on the funding of scientific research provides an instructive example of how superficial understanding of the categories in research statistics can generate a confused and unproductive science policy debate.

Norway likes to compare itself to its neighbours, Sweden, Denmark and Finland, and especially to the big brother Sweden. Since World War II Sweden has had a large defence industry due to its policy of neutrality between the communist and the capitalist blocks of the Cold War. The country also has an economy highly dependent on making and exporting technologically advanced pharmaceutical, electronic and other industrial products. The Norwegian economy is dominated by industries producing raw materials and semi-manufactured products. Such industries generally have low research intensity, and Norway is thus untypical of most with a high general educational and technological level. These differences explain why Sweden’s expenditure on total R&D is among the highest in the world while Norway’s is well below the OECD average, 4.3 % of GNP for Sweden (2001) and 1.8 % for Norway (2003). However, with respect to government support for basic science Norway is well above the OECD average with 0.76 % of GNP (2003) not much lower than Sweden. It is support from industry that is dramatically different, 0.82% in Norway (2003) and 3.07% in Sweden (Vilje til forskning 2005: 18, 40). Denmark and Finland also depend on exports of high tech products and have investments in total R&D a little lower than Sweden.

A realistic science policy should squarely face the reasons for the actual level of total R&D expenditure. And future goals should be clearly related to economic goals, what kind of industry the country wants to develop in coming decades. In 2005 a Norwegian government report on scientific research recommended that investment in research (R&D) was raised from the current 1.8 % of GNP to 3 % by 2010 (Vilje til forskning 2005). The figure 3% was inspired by an European Union (EU) recommendation that average investment in R&D for EU countries should be 3%. The Norwegian government intuitively felt that Norway was a technologically highly developed country and could not stay below the EU average. Thus it mechanically copied the EU recommendation without considering in any detail that the Norwegian economy was very different from the EU average. During the preparation of this report economists and others did caution that a rapid growth can be difficult to achieve because it takes a long time to train good scientists and talents are limited. They also mentioned that industrial initiative and will to contribute and make good use of such a research boost might be lacking. However, these warnings were quite restrained, including little or no differentiated analysis of the real opportunities for different sectors of research (Cappelen et al. 2004, Klette and Møen 2002).

In fact the official goal of the Norwegian government since the 1980s has been to raise investment in R&D to the average OECD level. And the effect has been small, for good reasons. The contribution of the private sector has remained at the same low level, and this has also restrained public investment in applied research and experimental development, due to lack of promising industrial projects to support. With the basic structure of Norwegian economy in mind it makes little sense to try to raise the total R&D investment by more than 50% in a 5 year period, as would be the consequence of the adopted policy (Vilje til forskning, 2005). It takes time to build high quality research whether basic or applied. And rapid expansion of applied research and industrial development without strong and active industrial partners is of little use. In a way it is reassuring to see that Norwegian investment in research has not grown at a pace corresponding to official goals. The percentage of GNP for R&D decreased rather than increased from 2003 to 2008.[4]

In the early spring of 2009 an increase to 3% of GNP for support of R&D is still the official aim of the Norwegian government, though it is clearly acknowledged that this level cannot be reached by 2010. Leading academics are also constantly reminding the government about its failure to even move in the right direction toward the 3% goal in their public pleas for more resources for university research. There is discouraging a lack of more precise analysis of the situation for Norwegian R&D. Some parts, like social science and the humanities, are well funded in comparison to the neighbouring countries. The big discrepancy is in industrial applied research and development. Existing industries mostly have a limited need for R&D and in the absence of active industrial partners government investments in research for a future high tech industry has to be limited. It is surprising that the universities, which could be expected to possess some of the best theoretical knowledge on such policy issues, have not been able to promote a more sophisticated public debate.

4. The "economic" approach to science – a historical sketch

In the early years of the cold war intellectual freedom was the main topic in political debates about science. The European scientific tradition was seen as a crucial support for liberal democracy against totalitarian regimes of the left and the right. By the early 1960s the focus of science policy had shifted to economic growth and development. Science was recognized as a fundamental motor in economic growth and supported by public money on a scale unknown before the war. How to distribute resources between different kinds and areas of

[4] Recent statistics shows that Norwegian investment in research dropped further to between 1,6 and 1,5 % of GNP in 2005. The national budget for 2008 indicates approximate status quo. Because of rapid increase in national income, however, there has been substantial increase in total investment in R&D in recent years.

research was a main issue. Starting from economic considerations the justification of basic research became a problem.

The nuclear physicist and science administrator Alvin M. Weinberg saw two possible principal justifications of basic research: Either as a “Branch of High Culture” or as “An Overhead Charge on applied Science and Technology” (Weinberg 1964/1968: 84, 89). The philosopher and historian of science Stephen Toulmin did not find the high culture doctrine reassuring and opted instead for basic science as a “tertiary industry” that would soak up the work force otherwise doomed to unemployment by the rapid progress of technological efficiency in production of all goods needed for a comfortable life. Liberating people from trivial production tasks and giving them the opportunity to pursue science for its own sake would be an important contribution to a superior quality of life in the new society that was emerging (Toulmin 1964/1968: 126-133). Down to earth economists were hesitant to acclaim Toulmin’s utopian dream and insisted that basic science had to be considered either as investment or consumption. Resources for basic research could be justified either because of “positive output consequences” or “because it is a pleasurable consumption activity” (Rottenberg 1966/1968: 134-142).

In other words: On closer economic scrutiny Toulmin’s argument did not go beyond science as “high culture.” This implied that public spending on basic research was to be justified politically in competition with other cultural activities art and literature, or even sport - to the extent that it was not justified as a necessary overhead on applied research and technological development, which clearly contributed to economic growth. It is striking how the science policy debate in the 1960s had narrowed down to an economic perspective. The idea of (basic) science as a pillar of liberal democracy had receded to the background.

Recent analysis and discussion in the "economics of science" follow the 1960s tradition in its framing of basic research in economic categories. *Science Bought and Sold* (Mirowski and Sent 2002) is a collection of papers ranging from classic contributions by Charles Sanders Peirce, Richard R. Nelson, Kenneth Arrow, and Michael Polanyi, to recent contributions by historians, philosophers, economist and sociologists of science like Paul Foreman, Philip Kitcher, Michel Callon, Paul David and Steve Fuller.

Kenneth Arrow's paper on "Economics of Welfare and the Allocation of Resources for Innovation" from 1962 starts with the assumption that "the production of knowledge" is more or less identical to "invention" (Arrow 1962/ 2002: 165). This economic and technological perspective on knowledge is more explicitly presented in Richard Nelson's discussion of "The Simple Economics of Basic Science", arguing that the US should spend more on "basic science" to be internationally competitive economically and technologically. The paper was first published in 1959 and starts by referring to the sputnik-shock, still fresh in the public mind. Nelson's epistemology has an instrumentalist flavour which makes good sense in economics but is more problematic for theories of natural science. Theories are described generally as "relationships between facts (usually, but not always, equations)" (Nelson 1959/ 2002:154). Nelson stresses that basic research is not a "homogeneous commodity". He argues that a free-enterprise economy will under-invest in common welfare commodities like education and public health, which are important economic factors, because such investment is not profitable for private firms. Nelson's worry is that the same problem of "external economies" also applies to basic science (Nelson 1959/2002: 162-263).

This economic perspective on science and technology policy and the difference between basic and applied science has more recently been developed by Partha Dasgupta and Paul David in a paper from 1994, "Toward a New Economics of Science". They want to synthesize the classical economic approach of Arrow and Nelson with Mertonian norm-based sociology. Dasgupta and David find that the institution of publicly funded "open science", governed by Merton's ethos of science, is "functionally quite well suited to maximize the long-run growth of the stock of scientific knowledge" but "most ill suited" for applied research, i.e., "to securing a maximal flow of economic rents from the existing stock of scientific knowledge". The latter is best served by a distinctly different system subordinate to principles of private commerce. For the sake of balance and long term economic development society needs public patronage of the open science system (Dasgupta and David 1994/2002: 241-242).

Dasgupta and David's use of Merton's norms hardly goes beyond the instrumental economic perspective that justifies basic science as an overhead on applied science and technology. They argue that the differences with respect to "methods of inquiry" and "nature of the knowledge obtained" are just "epiphenomena". The essential differences lie at the "deeper level" of socio-economic rule structures that govern the behaviour of individual scientists. Researchers conduct "essentially the same inquiries" in technology as in basic science (Dasgupta and David, 1994/2002: 228). Thus technological and economic innovation remains the channel by which science affects society. In the discussion of David and Dasgupta there is no consideration of the enlightening role of basic science, of its direct effect on the goals and values of politics.

I have shown how the economic externalist approach has dominated science policy debates since the 1960s. Investment in basic research is important because it fuels technological and applied research. The high culture argument was not convincing for esoteric natural science or for the rapidly expanding and increasingly detailed and specialized social science. Some participants, like Stephen Toulmin, felt that this justification was to a narrowly economic and oblivious to the fundamental role of the scientific tradition in European civilization, but in tune with the spirit of the times nevertheless fell back on basically economic categories. They did not stress that a vigorous basic science was important in support of political democracy as people like Karl Popper, Robert Merton, and Michael Polanyi had so forcefully done two decades earlier.

Recent science policy literature tends to overlook that Merton's ethos of science was an ideal for basic science and not for applied science or the broad category of R&D. David Guston, for instance, presents Merton's norms through the eyes of the sociology of scientific knowledge with little sense for differences between basic and applied science when he analyzes the problems of preserving quality in scientific research or how to assure "Integrity and Productivity of Research" (Guston 2003: 9, 27).[5] Descriptions of the interaction between science and society are dominated by money one way and innovations the other. Guston discusses Vannevar Bush's 1945 report *Science: The Endless Frontier* without making clear that it was primarily a plea for strengthening basic science (Guston 2003: 56-59), and there is no entry for "basic" or "applied" science in his index. It is primarily a book about applied science and technology as instruments for politics. The enlightening role of basic science is left in the dark. This neglect of differences within the broad statistical category of R&D seems

[5] Guston here refers to work by Sheila Jasanoff and Thomas Gieryn where science is discussed in a wide sense including applied research.

typical of most recent literature in the field characterized as “science, technology and society” (STS).[6]

The externalist economic and sociological approach to science policy also dominates the most influential input to science policy debate during the last two decades: *The New Production of Knowledge* (1994) distinguishes two different ways of doing scientific research, Mode 1 and Mode 2, corresponding to two different kinds of knowledge. Mode 1 stands for the traditional academic and discipline-oriented research and knowledge. Mode 2 is “different in nearly every respect.” It “operates within a context of application” and is “transdisciplinary rather than mono- or multidisciplinary” (Gibbons et al. 1994: vii). Organizationally it is transient without a stable hierarchy, as well as “more socially accountable and reflexive” than Mode 1. Thus Mode 2 has many similarities to applied research in the OECD definition but is nevertheless quite different (Gibbons et al.: 2-3). Mode 2, “the new production of scientific knowledge” represents the inclusive OECD category of R&D “research and development”, and thus erases the distinction between basic and applied research.

The thesis of the book is that this new mode of scientific research is expanding with respect to economic as well as epistemic importance and threatens to marginalize or swallow up the traditional academic and disciplinary way of doing science. Mode 2 is a new way of producing scientific knowledge which represents a general framework of political and economic steering of science, which the institutions of Mode 1 will have to adapt to (Gibbons 1994: 11-16). It represents a unification of theoretical and practical science under the governance of politics and economics. This conceals the traditional liberal justification of an autonomous institution of science responsible for true knowledge rather than economic

[6] For instance it is hard to find any discussion of differences between basic science, applied science, or technological development in the third edition of *The Handbook of Science and Technology Studies* (Hacket et al., 2007). The index has no entries for “basic” or “applied”, either as “research” or “science.”

efficiency or social usefulness. The difference between arguments relevant for judging scientific claims and arguments for and against conducting a certain kind of scientific research is obscured.

Although the Mode 2 economic approach to the governing of science has been dominant in recent decades there are also opposing voices emphasizing the epistemic and ultimately political difference between basic and applied research. Illka Niiniluoto in his characterization of applied research starts from a distinction between two fundamentally different goals, epistemic and practical. The primary task of basic research is "cognitive" to help us "*explain* and *understand* reality" and develop a "*world view*". Applied research and technological development on the other hand is also subject to practical technical goals. It is governed by "technological utility" and should be assessed according to this (Niiniluoto 1993: 3-6). Aant Elzing has persistently pointed to the danger that "epistemic drift" under such regimes of science policy will undermine the reliability and authority of science and in the end its value as a source of common good (Elzinga 1985, 2004).

5. "Pasteur's quadrant"

In spite of the theories that see basic and applied research as a seamless whole practical science policy perceives a persistent dilemma in dividing resources between basic and applied research. Much attention has been given to an attempt to overcome this dilemma made by Donald Stokes, political scientist with extensive practical experience in science policy. In *Pasteur's Quadrant. Basic Science and Technological Innovation* (1997) Stokes argues that the problems disappear if one focuses on the most outstanding scientific research because here the theoretical and practical come together. The contributions of Pasteur, for instance, belong in the top category with respect to theoretical as well as practical contributions, whether you consider it as applied or basic.

Stokes (1997: 72) illustrates his idea of “Pasteur’s quadrant” with a four-box diagram:

***Research is inspired by*:**

		Considerations of use?	
		Yes	No
Quest for fundamental understanding	Yes	Pure basic research (Bohr)	Use-inspired basic research (Pasteur)
	No		Pure applied research (Edison)

Table 1. Pasteur’s quadrant

Stokes holds that since World War II it has been widely believed that “the categories of basic and applied research are radically separate” and that their “goals are inevitably in tension” (Stokes 1997: 24). The solution, he argues, lies in noting that the annals of science are “rich with cases of research that is guided both by understanding and by use, confounding the view of basic and applied science as inherently separate realms” (Stokes 1997: 25). This means a research policy that gives priority to the upper left hand corner of his diagram, ‘Pasteur’s quadrant’. Here belongs the “major work of John Maynard Keynes, the fundamental research of the Manhattan project, and Irving Langmuir’s surface physics” as well as Pasteur’s biomedical contributions (Stokes1997: 74).

But how well does this solution stand up to closer scrutiny using the concepts of basic and applied research developed in this paper? First: The Manhattan project appears misplaced. It was primarily applied research and experimental development, aiming to produce a fission bomb. Second: The purported dilemmas are not between the lifelong contributions of prominent scientists. They are between individual projects or programmes with specified goals that differ in their emphasis on theoretical versus practical achievements, and have to be evaluated accordingly. Projects can be small, involving just one or a few researchers, as is quite common in basic research. Or they can be large involving hundreds of scientists. There are large projects and programmes in applied as well as in basic research. The Apollo project of the 1960s and the experiments presently conducted in the Large Hadron Collider at CERN are impressive modern examples of large scale applied and basic research respectively. These two examples also illustrate the difference in criteria for success. In the first case research was primarily the necessary instrument for putting a man on the Moon. In the second new understanding of the nature of the smallest components of matter is the goal. Especially in large projects interesting spin-off in terms of unexpected theoretical discoveries or new technology is likely. But this was not the reason for funding them.

A research policy that aims for Pasteur’s quadrant implies judgements that combine the criteria of applied and basic research. Much will depend on how this is done. Considering the tough competition high scores are needed to succeed. A procedure which simply adds up the scores on both dimensions seems likely to favour projects that have an above average score on both dimensions rather than the more specialized ones with a top score in one and a low score in the other. The result will easily be elimination of the most promising projects and thus favour mediocre research, in either dimension. It is hardly a good idea to stop support for

Edison and Bohr because giving it to Pasteur would solve the troublesome problem of dividing between the two of them.

The important and valid point of “Pasteur’s quadrant” is that interaction between the most challenging theoretical and practical problems can be highly productive. Theory provides practice with new concepts and theories, and practice presents theory with unexpected facts. Some of the most important achievements, both in basic and applied research, have their origin in settings which include both. This indicates that interaction between basic and applied research is most effectively secured by institutions and individuals that are in some way concerned with both.

Pasteur, for instance, started his career in crystallography investigating the difference of organic from inorganic chemistry. He found asymmetry in crystals of organic compounds to be explained by asymmetry in molecular structure, and speculated that the asymmetry of organic molecules was the secret of life. This “organismic” view inspired his refutation of spontaneous generation in micro-organisms and development of a germ theory. Together these theories remained a continuing hard core in his later researches in applied microbiology, on brewing, diseases of animals and plants, vaccination, etc. (Dagognet 1967, Roll-Hansen 2008). This conceptual and theoretical framework was not a result of his involvement in practical technological problems, as has been assumed by economists referring to Stokes’ ideas about ‘Pasteur’s quadrant’ (Klette and Møen 1997: 159). Rather it was a precondition for his ability to formulate soluble and fruitful questions in applied research.

6. The normative foundation of basic research

It is often objected to the distinction between theoretical ("basic") and practical ("applied") science that it is based on an untenable dichotomy between facts and values. However, the abandonment of dichotomy is quite compatible with upholding a distinction. In an essay on "The collapse of the fact/value dichotomy" Hilary Putnam has argued that "an absolute dichotomy" between "facts" and "values" is a misunderstanding derived from logical empiricist illusions that a strict separation of fact and value claims is both possible and desirable. Since Quine's 1951 critique of the analytic/synthetic distinction it has become clear that such dichotomies cannot be upheld. Putnam underlines that what must be discarded is the *dichotomy*, the categorical separation between fact and value. Like John Dewey he is fully aware of the importance of a *distinction* between facts and values (Putnam 2002: 2-9). As an alternative to the dichotomy he describes the "entanglement" between facts and values (Putnam 2002: 28 ff.).

Putnam's notion of "entanglement" does not imply a gradual transition from claims that are purely factual to pure value claims. His point is that many assertions, in everyday life and politics as well as in science, simultaneously make claims both with respect to facts and values, because they rely on "thick" concepts like "cruel" that can refer simultaneously to moral value as well as fact. The claim that "He is very cruel" will usually both describe behaviour and judge the person as bad in character (Putnam 2002:34). But in a specific context, where certain decisions or actions are at stake it is both possible and desirable to distinguish between the actual behaviour or factual claims on the one hand and the motivations and attitudes of the person on the other. Examples are criminal processes, appointments and job hiring, political decision-making, and public debates.

I would argue that thick concepts are important also in natural science discourse, both internally in the specialist community and in public. In biology, for instance, terms like “fitness” and “function” have a value aspect. To be “fit” is better than “unfit”. A trait that functions to uphold the life of an organism can be said to be more valuable, both for the organism and its species, than one which does not. Thus it can be claimed that natural science as a means to human self-understanding involves values other than the purely epistemic. Some reductionist approaches to biology allow only causal explanation based on purely physico-chemical description as true biological science. In my view this can be seen as an attempt to purge biology of “thick” concepts which are essential for its function in human self-understanding. Putnam has suggested that *physicalism* has been an important source of the misleading fact value dichotomy in philosophy (Putnam 2002: 40).

That the choice of problems in applied research depends on external social values and not only on epistemic values internal to science is uncontroversial. But there is also broad agreement that even basic research must in the last instance serve generally accepted social values. Otherwise there would be no justification for special public support. This was the crux of the 1960s debates over “criteria of scientific development”. But even Stephen Toulmin was unable to make a clear break with the economic frame of thought.

The view that all of science, including basic research, must in the last instance serve general human welfare was indeed fundamental to science policy debate through the twentieth century. The first example of a grand government supported and directed policy for science and technology, in the Soviet Union, was based on Marxist theory of science and knowledge stressing the “unity” of theory and practice: Practice was exhorted as the driving force and theory chastised for its inability to catch up. Political guidance of theory was needed to keep science straight. The moral drawn from the Lysenko affair and other miscarriages in Soviet science was that political interference was the source of the corruption and must be avoided. One may ask, however, whether there is not a common root to the excessive socialist interventions and the extreme liberalist reaction of the West, namely a lack of differentiation between basic and applied research (Roll-Hansen 2007). Without an understanding of the differences it is difficult to see where political choices are approprioate and where scientific freedom and autonomy is a fundamental liberal democratic value.

In recent philosophy of science there is a growing recognition of the importance and complexity of this problem of the ultimate normative justification of basic science. For instance it has been analyzed from a feminist perspective by Helen Longino (1990, 2002a), and from a perspective of general social justice by Philip Kitcher (2001). The crucial problem is how to conceive and introduce the superior criterion of the common good without undermining the epistemic virtues of science, i.e. its ability to give us as truthful answers as possible to the questions we pose. Longino has argued that pluralism in the recruitment of scientists, securing a broad representation of social values among the specialist, is in fact a stimulus to scientific creativity and debate, increasing the ability of science to reach significant new insight and knowledge. Kitcher is unconvinced that such a "hidden hand" trust in liberality is enough to keep science on the right track and has sketched a more formal system for science policy.

In Philip Kitcher's model of "well-ordered science" the superior criterion of "human flourishing" is implemented through a democratic political system governing the politics of science (Kitcher 2001). In contrast to the argument of the present paper Kitcher holds that the distinction between basic and applied science lacks a sound empirical basis, and that it should be rejected as politically harmful because it can be an obstacle to relevant external criticism. His analysis starts from the assumption that the distinction between basic and applied research is based on the “myth” of pure science - the idea that “basic” science should be isolated, independent of social context, and not responsible to external moral and social values. He finds on closer scrutiny that such a basic science does not exist in reality, and also that it represent a pernicious ideal. There is therefore no significant distinction to be made between basic and applied research either in the descriptive or the normative dimension, argues Kitcher (2001: 65-66). But how adequate is his description of basic research when compared to the practices and discussions of science policy since the 1960s - or the1930s? Perhaps a proper distinction between basic and applied research represents just the kind of theoretical understanding that could help develop a workable version of his model for “well-ordered-science”.

The problem with Kitcher’s argument against “dichotomy” and “purity” in science is that his examples come mainly from the applied side. He presents Werner von Braun’s blatant moral disengagement from the effects his rockets[7] as typical of the “pure” science (Kitcher 2001: 89). For one thing it is much more difficult to foresee the practical effect of basic science than of weapons engineering. And there is also tradition of social responsibility of science running from the defence against totalitarian distortions in the 1930s, through the concerns of atomic scientists in the Second World War to the political engagement of the environmental scientists of today. This tradition was fronted by scientists renowned for their theoretical rather than their practical technological achievements.

[7] In Tom Lehrer’s famous lyrics: “Once the rockets are up who cares where they come down, says Werner von Braun”.

Neglect of the difference between practical and theoretical science also weakens Kitcher's argument for "Constraints on free Inquiry", i.e. how socio-political criteria should in certain cases overrule the purely epistemic. He uses the example of research on hereditary differences in intelligence between men and women or between human races. But again he draws his evidence from applied rather than basic science. Kitcher argues that the history of biology shows that the effects of such research is likely to be biased in favour of racism and sexual discrimination, "a wealth of historical studies hammers home the same moral" (Kitcher 2001: 99). However, the historical literature he refers to is mostly concerned with research of applied nature, closely linked to specific social policies and ideologies, partly from an era before the advent of genetics as a special sub-discipline. A different picture emerges when more attention is paid to leading geneticists' criticism of eugenics from the 1920s on. Proposals for eugenic policies were effectively revealed by left and liberal scientists as built on untenable assumptions about human heredity. This was a crucial factor in the demise of eugenics at mid-century (Roll-Hansen 1980, 2009). Thus there is a good case for claiming, contrary to Kitcher and much of the recent literature on the history of eugenics, that basic research in genetics has been a major force in reducing discrimination due to race or sex.[8]

Helen Longino in *The fate of Knowledge* aims to "integrate the conceptual and normative concerns of the philosophers with the descriptive work of the sociologists and historians". She argues that a dichotomy between the "rational" and the "social" has been a common mistake on both sides of the recent "science wars". The parties have been talking through each other because they have taken either the one or the other as fundamental. Even Kitcher's attempt to integrate sensitive historical and sociological analysis into a normative philosophy of science remains too scientistic in her opinion (Longino 2002: 8). While Kitcher relies on scientific

[8] For a brief survey of recent literature on the history of eugenics and sterilization see the preface to Broberg and Roll-Hansen 2005.

specialists "tutoring" the public on its way to rational decisions in the politics of science Longino believes in an open and critical public debate of scientific issues where pluralism in method and involved external values will be the guarantee for scientific objectivity rather than Kitcher's somewhat authoritarian method of tutoring. [9] It appears that Longino argues in the fallibilist and pluralist Popperian tradition of Feyerabend while Kitcher follows Kuhn's more limited empiricist perspective on the social nature of science.[10]

Longino (2002a) does not engage the question of how to organize science policy decision-making like Kitcher (2001), and she does not explicitly comment on the distinction between basic and applied research. But in a later debate issuing from mutual reviews of their two books she agrees with Kitcher that there is no "'morally significant' distinction between science and technology" (Longino 2002b: 561). Maybe she too easily accepting Kitcher's dichotomy between basic and applied research. If she had been thinking more in terms of Putnam's distinction between facts and values, the conclusion might have been different, and in better harmony with her own rejection of a dichotomy between the rational and the social in theorizing about science. Both for Longino and Kitcher it can be argued that a theoretical framework for science policy that integrates epistemic and social values is more easily reached by taking into account the differences between basic and applied research. It seems hard to deny that there was a "morally significant" difference between the Hahn-Meitner investigation of the chain reaction in uranium 236 and the Manhattan project of building an atomic bomb. And it appears likely that this difference says something about the moral and political predicament of modern science, as it emerges for instance in the areas of biomedical and environmental research.

[9] He states for instance: "I believe that a sober review of the history of research into racial and sexual differences supports the view recorded in the argument, and thus any attempts to read that history differently embody just that epistemic bias the argument diagnoses" (Kitcher 2001: 106).

[10] This contrast between Kuhn and the Popperian tradition has been displayed in instructive manner by Steve Fuller, 2004.

Conclusion

This paper aims to delineate a distinction between basic and applied research which is at the core of present science policy debates, even if it is often, or even mostly, poorly understood and neglected, or even explicitly rejected. I argue that this distinction has been central in science policy debates since the early twentieth century. It was central to debates over autonomy in the ideologically charged debates over democracy and freedom in society and science around the Second World War. It was recognized as a fundamental premise in the 1960s seminal debates on criteria of scientific choice. But it became marginalized and sometimes explicitly rejected as meaningless and harmful by the new wave of science studies starting around 1970.

I have argued that when the distinction between basic and applied research makes good sense philosophically when not interpreted in terms of rigid and exclusive metaphysical categories, and that historical events in science as well as present science policy questions are more adequately understood with than without this distinction. In conclusion I suggest that the revival of a distinction between applied and basic research in the sense sketched in this paper would be helpful in improving historical accounts and philosophical analysis as well as present practice in the politics of science.

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