Basic and applied research in the university − have they changed?

OECD WORKSHOP ON BASIC RESEARCH:
POLICY RELEVANT DEFINITONS AND MEASUREMENT
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Abstract

My hypothesis is that there is a gap between the understanding of science among funding institutions, and the understanding of science among scientists. Why is the gap there, and was it always the same? I argue that the scientist work the same way as earlier. Science policy on the governmental level has, however, changed. The main reason for this is that the motivation for investing in science has changed. Arguments can be given that the gap should be narrowed, and it is possible to do so by addressing the scientists in better ways. Most important is to address the research group in the university. The definition of basic research should not be changed, however.

1. Introduction

What motivates a scientist?
How does a scientist work?
Concepts in science have not changed!
Conceptions have changed
Can we bridge the gap?

The purpose of this workshop is "to analyse the concept of basic research in the context of science policy and of research statistics", but the overall aim is to create a better policy for science. My view, as a scientist, is that this cannot be done without discussing basic research, as perceived by the scientist. In order to create a good policy for science, one has to understand the psychology and mechanisms of the work. A good way of addressing science, is a way that scientists feel well about. Science is like art and can only be promoted by good feelings.

I shall therefore not speak so much about the university as an institution, but rather about the scientist in the university. I shall discuss what motivates a scientist, and how a scientist works. I shall argue that *basic and applied research* have not changed during the course of time. *Concepts of basic and applied* research have not changed for the scientist. *The conception of basic science* has changed, however, and my hypothesis is that this is mostly related to the funding situation.

One argument to fund science is "for the sake of knowledge". This *is* an argument that scientists feel well about, because it refers to certain ideals about knowledge. It fits well with the definition in the Frascati manual:

"Basic research is experimental and theoretical work undertaken to primarily acquire new knowledge of the underlying foundations of phenomena and observable facts."

In the background documents for this workshop[1], it was concluded that "arguments using knowledge for knowledge's sake will almost certainly lead to greatly reduced funds for basic research". This describes in a nutshell what kind of controversy we are in. We are not recommended to use an argument for funding science, which scientists feel well about and that agrees with the OECD definition of basic research.

While I appreciate efforts that can increase the funding of basic research, I think honesty pays better, also here. There have been changes in the funding of science during the last five decades. The changes have lead to a gap between funding institutions and scientists. Shall we attempt to bridge the gap? I hope that we can for the better of both, and I point at some possibilities in the end.

2. What motivates a scientist?

The great scientists in the history have often appeared as idealistic, almost egoistic individuals willing to work long hours for little or no money to pursue their idea. We all know that Marie and Pierre Curie did this, without institutional support. The biologist Peter Mitchell had to publish his Nobel prize work by his own means, nobody else wanted to print it. The mathematician Nils Henrik Abel died from pneumonia, because he did not have money to heat his house. There was no position available in Norway for the most famous physical chemist in the last century, Lars Onsager. Those were top scientists. There are many medium level scientists, but their motivation and working habits are pretty much the same.

In science, we are all working with the questions of why and how. Basic research has a higher prestige than applied research. The reason may be that rewards in basic research take more time than in applied research. Applied research give rewards on a shorter time scale. This has always been so. Let us take the discussion between Sokrates and Glaukon as an example.

Sokrates: What about taking up astronomy in our next studies?
Glaukon: That would be wonderful, Sokrates!
Think about the importance for warfare, agriculture and travels at sea!
Sokrates: You disappoint me, Glaukon, by your shortsighted views on knowledge.

Both Sokrates and Glaukon are clearly interested in knowledge. But while Glaukon wants his reward soon, Sokrates has a longer time perspective. Glaukon's ambition is to be acknowledged by the men of his time. But also Sokrates has an ambition. How could he otherwise be disappointed in Glaukon? These are the equally important different ambitions behind work in science.

[1] Martin and Calvert, Changing conceptions in science, 2001

Fig. 1. Sokrates and his students painted by Greuter.

3. The research group − the way of working.

Sokrates and his students formed a research group. The way they did their work is basically the same as the way we work in research today. Also today, the research group consists of one or two professors, their students and guests. The difference is only that today the students have names: They are doctoral students, post docs and master students. Group members have always had different background. In my group now there are chemists, physicists, and engineers. They come from countries all over the world, but can immediately speak to each other, *because the concepts of science are the same.* The method of working is to ask questions and discuss them in the group.

The research group is the nucleus that has always been active in research, and on which everything is based. The group may be small or big. It may have grown in average size, since there are more scientists today than 50 or 2000 years ago. But the group interaction is the same. There is a limit, however, as to how many co−workers the group leader can have, and still be active in science.

4. Basic or applied research?

How can we define the nature of research done in a group? What may be confusing to the outside is that the group normally does *both* basic and applied work. Projects will naturally reflect the research aims of the leader, but the individual *directs his own work*. Some projects are often generated outside the group. They are commissioned. Patent applications originate from basic or applied projects. Whether a person does basic or applied research changes with the position in the group, the age, funding possibilities and what is currently the best idea worth pursuing.

One person has at a given time a defined research goal, an idea to investigate, and a question to answer. It is *the type of question* which determines the nature of the research. Basic questions often start with "why". Applied questions more often start with "how".

A typical basic question is: Why do we see this behaviour?
A typical applied question is: How do we (use this understanding to) achieve the results (for agriculture, warfare or travels at sea.)

Basic research has understanding as only goal. [*]Basic research *does only promise a contribution to question*, not to anything else, but it can give the most unexpected applications. The GPS−system, for instance, is an application of the theory of relativity. We can draw a borderline between basic and applied research by sorting questions into those with or without knowledge as aim.

Each individual may cross this borderline many times. Einstein is known for his basic research, but he had patents! The same person normally does both basic and applied research over a life in the university.

Oriented basic research has been defined in the Frascati manual as " research carried out with the expectation that it will produce a broad base of knowledge likely to form the background to the solution of ?. future problems". Oriented basic research *does not exist* in my view. If we would have known where to look for a solution, the basic research had already been done. It had already produced the broad base that forms the background for finding the solution. Basic research may know the problem, but *does not know how* to orient itself towards a solution. From the scientist's point of view, the two definitions of basic and applied research are sufficient. My question is therefore whether we need the concept "oriented basic research". We can simplify.

5. The importance of the research group

The worst thing one can present to a creative mind, be it an artist or a scientist, is negative attitudes, fear and neglect. To create something new is difficult. You bare your self and your innermost thinking in doing it. Fundamental progress in science depends more on the individual, than on a group. But if you happen to belong to an inspiring group that functions socially, you are not afraid to bare yourself. It is telling

[*] The expression "Blue sky−research" is misleading because it does not indicate a goal. The same applies to the expression "Curiosity driven research".

that the most ambitious scientists seek to have such a group around them. The group holds the clue to a good research policy!

The importance of the research group can be illustrated by the Copenhagen school with Niels Bohr at its centre. This group had an enormous impact on the development of quantum mechanics and physics at large in the previous century. It is still giving Denmark a reputation for good science. There is *statistically* nothing so important for getting the Nobel prize, as being a student of a person that has one, or coming from a scientific group, where ideas can be exchanged without fear and negative attitudes.

The group *does not need* to keep basic research from applied research. On the contrary, progress in basic research has often been promoted by difficult problems posed by industry. The groups I know welcome a multitude of questions.

Up to this point, I have argued that scientific work means to the scientist now, very much the same as it did before. In our world, *the concepts of basic and applied research are absolutes.*

The objective of this meeting is to analyse the concept of basic research *in the context of science policy* and research statistics. The suffix "in the context of science policy" becomes very important, because this is where changes can occur. Without the suffix, the statement is meaningless to me. We are asked "to suggest changes in the characterisation of basic research which correspond better to the use of this concept in the present science policy environment"[2]. This cannot be done if basic research is still the same. I do agree that there have been changes in the perception of science by the non−scientists, however. I would therefore like to turn the sentence around:

The present science policy must be changed so that it corresponds better to the way science is done.

The gap between policy and real work can then be bridged.

6. The funding of science

Science policy is all about the funding situation.

6.1. The funding situation from the scientist perspective

Let us look at the way Newton was funded for his work on the law of gravitation (see Weber, A random walk in science, 1973).

The young Newton was a professor of mathematics in Cambridge, teaching all his time. He got his idea about the planets falling constantly in their orbit, while he watched an apple fall in the garden. This is known, because he mentioned it in his letter to the King when he asked for funding. The funding committee had a demanding member. He needed to know if Newton had means of improving the conditions for apple growth. Newton began to explain that the apple was not an essential part of the hypothesis, but in vain, because now the whole committee was speaking in favour of a project to improve conditions for apple growth. And with a

[2] Possible results of the workshop, Issues paper p.5, and Objective, Final Program

promise to assist the fruit growers of England, Newton was funded. The world obtained the theory of gravitation, however. In our terminology, can we say that his project was funded in a strategic program for optimum fruit growth?

The story is old and yet new. A scientist today needs funding, and is facing a series of national and international research programs. You look for a program, which gives room to develop your idea. You are willing to give some concessions, that is, "to discuss some conditions for fruit growing", so that you also can work on the topic that you feel is important. If you think that you can contribute to basic electrochemistry, you can seek funding from a fuel cell program, and try to make room to do basic studies within the program. Basic electrochemistry is hardly funded today. Fuel cell research is.

6.2. The funding situation on the policy level

The difference between today and yesterday is that the number of national as well as international programmes that fund science has grown substantially. They greatly outnumber the free projects. Knowledge for knowledge's sake is not a visible argument in science policy. The economic arguments for science dominate completely.

Let us return to the university research group, with its multitude of questions to be answered. Some projects are funded by the Norwegian Research Council, one by the industry (Statoil) and one by EU. Guest from abroad work on topics of their choice with their own money (from Japan), or are paid to solve particular problems (Hydro aluminium). Only I have my salary paid by the university. The multiple origins of funding is fine − this is exactly what is needed to have a fruitful mix of different perspectives, differently trained people, and different challenges. It is a problem that it is most difficult to obtain funding for long term questions. These questions are solved, "in between", together with students that pay themselves or with guests.

I explain that as follows. The main change over the last decade has been an *increased economic motivation* for the public funding of science. Scientific research is recognised as a major driving force behind the knowledge−based economy. This is also already stated in "the issues paper" given to us before the meeting. Present days public funding is motivated mainly by its economic importance. It is measured as a percentage of the states national budget, and correlated to economic growth etc. Cultural and educational motives for funding are barely visible.

In the early 1980'ies, when I entered the board of The Norwegian Research Council for Science and Technology (NTNF), the model for knowledge transfer between academia and industry was *the linear model.* The documents and the discussion reflected an opinion that knowledge was handed over in a step−wise manner from basic groups to applied groups, and further to development. The linear model was created in order to obtain a better return of the research investments; it was used to argue for more funds. When we compare this model to the activity and motivation of

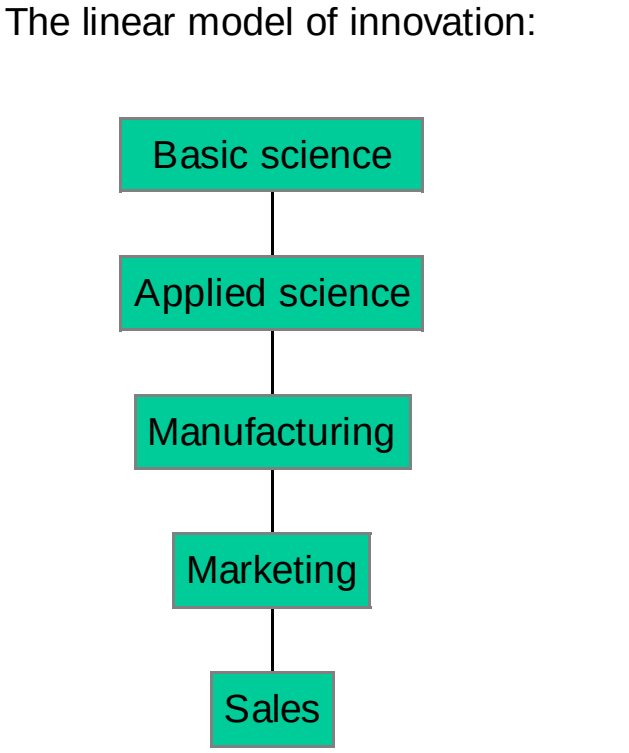

Fig.2. The linear model of innovation used by science politicians in the 1980'ies

the typical research group, there is clearly a mismatch. The research group is not an isolated link in the chain, out of contact with the others. Each group works on both basic and applied subjects.

The funding institutions in Norway were reorganised in 1993. The political motivation was to help speed up the process of innovation. The new Research Council of Norway (NFR) abandoned the linear model. Different actors in basic and applied research were put together in one group. The logo of NFR (I was on the board) was the symbol of a *meeting place.* It was easy for me to favour the new organisation. It seemed to promise that all types of activities that I knew from my research group were encouraged. It bore a promise of a fruitful common environment for basic and applied research together. The coupling model for innovation model seem right!

Fig. 3. The logo of the Research Council of Norway, funded 1993

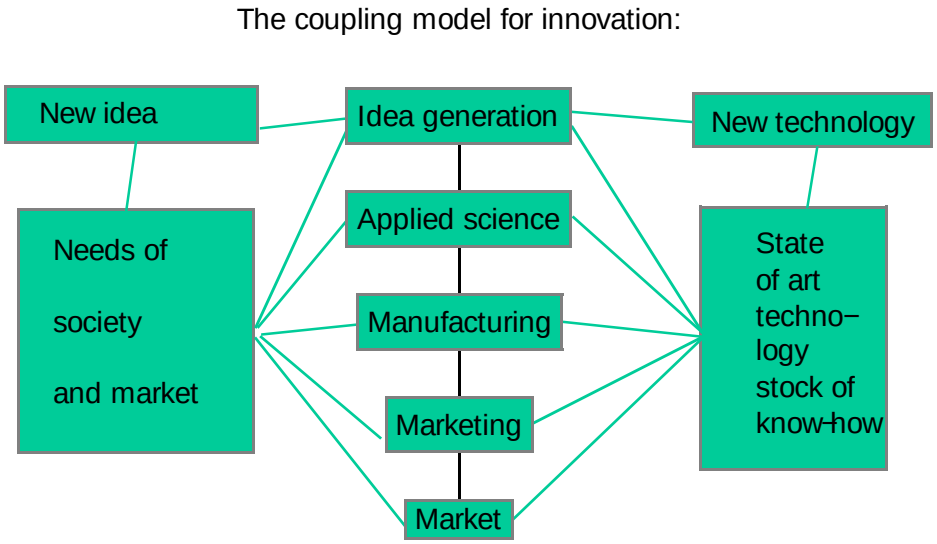

Fig.4. The coupling model for innovation

The organisational level is, however, not the same as the research group level. There was an increased *verbal* emphasis on the applied parts, which coincided in time with a lack of funding of basic parts. The possible outcome of science was used as *the* main argument for funding science. This happened not only in Norway. The European Union Science Framework programs strengthened national trends: To an increasing degree, the link between academia and industry was forced *by a third party, the government*.

The pressure on research groups to produce applicable results increased gradually. The percentage of applied projects in one group rose. The pressure to perform filtered long−term questions away. There were expectations to contribute "to society" by something useful. The taxpayers should indeed expect something useful from science, but they should also understand that by giving some freedom, more results can be

expected. Everybody understands that this applies to art. Work in science is not very different from art. It is impossible to force scientific developments in any way. It can only be inspired. The selection of a particular type of questions, either basic or applied ones, to be the preferred ones, leads in the long run to a reduction in quality. The mixture is fruitful! Strategic programs without a significant degree of freedom, fail to take into account the mechanisms of science. Questions without depth give shallow answers.

7. The need for changes

The objective of the government, to have return in value for the money spent on science is justified. The money should be used effectively. I argue that a science policy that alienates the scientist from the policy maker is not effective. A policy that makes a gap between the two sides is not cost effective, and should therefore be changed. It is time to address the scientists in a more correct and more inspiring way. Better scientific results may then be *expected*, and so may better economic results.

The single scientist seeking funding is vulnerable to arbitrariness, and lack of definition of suitable programs for his/her research. A scientist in a group is much more robust. The research group can act as a buffer against the institution it is in, and against the imperfect public or industrial funding situation (It is impossible to be perfect). The group, more than the individual, has a possibility to follow its own logic, in spite of funding policies. Since the group is the core for all scientific progress, policies may be built that address the group better. The group must not be exploited in such a way that all its projects are applied. *The science policy level must ensure that there is some degree of freedom preferably for all persons in the group.* Most strategic programmes have failed to recognise this.

The science policy level must further be inspiring to all scientists in a nation. Let me take an example that avoids addressing the research group as an entity. In Japan there is now a proposal that the government funds only 30 of the universities in the country. This policy leaves out the competition element between research groups inside and outside the border of funding. A competition based on talents and merits, independent of institutions leaves a chance for many more, and may give a better return of investment for the country I have enjoyed to work in.

8. Basic and applied research at the university

I have argued that basic research is characterised by the type of questions to be solved. The questions are not linked to the institution the scientist happens to work in. But universities happen to have more people in basic research than other institutions. This means that the universities more than other institutions have suffered the increased pressure.

The possibilities to work with basic questions are fewer now, and the funding is less diverse. The impact on the quality of the university teaching may be serious. I expect us to see this ten years from now, when the generation that was hired in the seventies has retired. Universities need to be included in a discussion on how to form, strengthen and fund their research groups. If we can develop criteria for the

individuals and their research group, universities may cope better. Good working conditions for research groups everywhere are essential.

9. Concluding remarks

The mere fact that this workshop is arranged is promising. We are here to deal with a borderline between basic and applied research that has shifted. There are worries about the location, not being beneficial. I have argued scientists themselves work with basic and applied questions in the same way as always. We cannot change the definition of basic or applied research. In my view, it is impossible, to change them so that they correspond better to a use in the present science policy context.

We must instead change science policy so that the policy:
Is more encouraging, less strict and controlling.
Reflects that the ideals of the society at large, and not mainly the economic ones.
Gives more internal freedom for the research group, to pursue its new questions.
Provides a good mix of basic and applied research questions.

A science policy of this kind will keep talents in science, because they feel appreciated and important. More results can be expected, because this will appeal to the ambition of the people. My hope is to narrow the gap between the scientist and the policymaker in this manner. Good concepts of basic or applied research in the context of science policy must be inspiring for the individual scientist in any research group. The definition in the Frascati manual for basic researchs seem fine. Let us use it.