The meaning of excellence and the need for excellence in research[1]

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I have been given the difficult task of talking about the meaning of excellence in science and the need for excellence in research. Many of us have an intuitive feeling of what excellence in science is, but when we try to analyse the concept more closely, it becomes a philosophical problem and much harder to explain. Let me start with a situation that should not be too difficult to grasp. If we look back on the history of a scientific discipline after a delay of at least ten years, it is often relatively easy to identify the breakthroughs, the seminal ideas, the critical experiments or observations. We know that these were all examples of excellent science. But even with a historical perspective, we are bound to miss some of the best work. The history of a scientific discipline always appears to be more streamlined in retrospect than it was in reality. We forget the ideas that proved to be wrong, the blind alleys, and all the excellent scientific work that is sometimes necessary to show that an idea is not useful or not supported by evidence. We must remember that that true excellence is needed not only in the discovery of new and fruitful ideas, but also in finding evidence that some approaches are invalid. There are other instances where excellent work was done, but was later forgotten because the time was not yet ripe for the underlying ideas to gain acceptance.

If it is difficult to identify all the excellent research carried out within a scientific discipline in retrospect, it is immensely more difficult to identify excellence in contemporary science or to make predictions about the future. But this is what we try to do when we are requested to evaluate candidates for research or teaching posts

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at a university or when we try to select the best people and projects from a pile of applications for a research grant.

I am familiar with this problem from a long history of working in research councils. One of my current engagements is to chair one of the panels responsible for choosing high-achieving young researchers for the Starting Grants from the European Research Council. We have already started the third round, and constantly face the problem of deciding which proposals seem to be most promising and meet the standard of excellence, and which applicants have the necessary potential. So the real problem is that we are trying to make predictions about future excellence in science.

Rather than trying to give you a prescription for how to judge excellence, I would like to share some rather fragmentary ideas that will show why the issue is so important and which pitfalls we need to avoid.

Excellence in retrospect

As I have already mentioned, there are a number of examples of excellent work that was done but then forgotten, simply because the time was not yet right to assess it properly and to follow up the new ideas. Most of you probably know better than I do that the first law of thermodynamics was discovered independently at the same time by three scientists – Joule in England and Mayer and Helmholtz in Germany. Clearly, science had progressed so far that the time was ripe to abandon phlogiston theory, with the old concept of heat as a substance, and to propose the modern concept of heat as a form of energy. This was a scientific revolution in the sense used by Thomas Kuhn – a paradigm shift. Many of you will also know that the second law of thermodynamics is older than the first. The second law defines an upper limit for the amount of physical work it is possible to obtain from a given amount of heat and available temperature difference. The second law was first formulated by Carnot in France about twenty years before the first law, when most physicists and engineers still believed heat to be a separate substance. If we read Carnot's publications carefully, it becomes clear that he in fact already considered heat to be a form of energy. However, the time was not yet ripe for his insight into the nature of heat, so this aspect of his work was neither well understood nor properly assessed by his contemporaries, and did not result in any experimental investigations. From today's perspective, we therefore judge the second law of thermodynamics to be Carnot's major contribution to the science of heat.

We can find similar examples in many other scientific disciplines. Not so very far south of here, in the mid-19th century, Gregor Mendel made fundamental discoveries on the inheritance of certain traits in pea plants, which more than thirty years later became part of the foundation of modern genetics. The significance of Mendel's work was not recognized until the turn of the 20th century. His excellent work would have been completely forgotten if it had not been rediscovered by three later geneticists, Hugo de Vries, Carl Correns and Erich Tschermak. They acknowledged Mendel's priority, but if they had not done so, Mendel would probably have been completely forgotten and his ingenious and pioneering work would have had no real scientific or practical impact. It is possible to say in very clear terms that scientific discoveries that undoubtedly carry the mark of excellence may easily be forgotten, not only because the time was not right, but also because they were not properly published.

Interestingly, the discovery of the two laws of thermodynamics is excellent counterexamples to the often-cited linear innovation model. This model assumes that the process of innovation starts with a breakthrough in basic science, a new concept or new theoretical understanding; this precedes applied research, which in turn is followed by the development of an actual product and maybe even a new industry. The steam engine patented by James Watts was constructed before the science of thermodynamics was even established. In fact, his steam engine provided an important stimulus for Carnot's investigations and for the subsequent development of theoretical thermodynamics with its modern approach. In contrast, the linear model clearly applies in the case of electricity and electromagnetism. First came Faraday's discoveries, then Maxwell's electromagnetic equations combining electricity and magnetism, and only after that the development of radio communications and finally television and modern telecommunications. Although Faraday already had some practical visions of what might emerge from his work, it is highly unlikely that Maxwell had any idea of how far his theoretical work would lead mankind.

In medicine, we have the example of Fleming's discovery of penicillin. This discovery was accidental, and although Fleming had long experience as an army doctor and a bacteriologist, he did not envisage the practical impacts of his discovery. Applied research came considerably later, when Florey and Chain developed methods for industrial production. Mass production of purified penicillin did not start until the end of the Second World War. All three scientists shared the Nobel Prize, but Fleming's citation, unlike those for the other two, did not mention the practical use of penicillin in medical treatment. The story of penicillin is a clear example of the linear innovation model, in contrast to the story of thermodynamics and the application of its laws.

Is excellence in research always needed? I believe not. Sometimes we need good reliable science, but not necessarily excellent science, because there is a piece of standard research work which has to be done and done reliably. A new treatment may for example need to be compared to an old method to determine which is better, or a new piece of equipment needs to be developed.

Judging excellence in contemporary scientific work

How can we judge excellence in contemporary scientific work – or in proposed future work? This is the situation we have to deal with in evaluation committees for grants and in selection committees for new research positions. My opinion on these issues, which I wish to share with you, comes from years of experience of this kind of work in the Research Council of Norway, in the European Science Foundation and now in the European Research Council.

In the evaluation process, many scientists and nearly all university and research council administrators love all kinds of bibliometric tools. There is of course a simple explanation for this. The "bureaucracy" would like to have a simple quantitative tool, which with the aid of a computer and the internet would give an "objective" measure of excellence. However, excellence it not directly related to the impact factor of the journal in which the work is published, nor to the number of citations, nor to the number of papers published, nor even to other more sophisticated bibliometric indices. Of course there is some correlation, but in my view it is weaker than many people would like to believe, and uncritical use of these tools easily leads to the wrong conclusions. For instance, the impact factor of a journal is mainly determined by the very best papers it publishes, and not by much larger number of ordinary papers it publishes. We know very well that even high impact factor journals like *Science* and *Nature* or high impact journals in more specialized fields publish less excellent papers from time to time.

I often meet scientists who see obtaining high bibliometric factors as the prime goal of their work. Too many of them are not really excellent, but have been lucky or work in a field where it is easier to obtain many citations. A high publication rate and a large number of citations for each paper is often a sign of what Thomas Kuhn called "normal science", which of course is necessary, but often somewhat boring. If you are working with established methods in a popular field, you can be fairly sure of having your papers published. I could give you details of some medical fields where I know that this has happened or is happening today. Scientists in such fields have many publications and citations, but their research is not necessarily excellent.

So we really must fight to prevent judgments from being based primarily or exclusively on bibliometric methods, which I think have become a disease of modern times. Take these data into account, yes, but do not treat them as an absolute truth.

So what is the alternative to bibliometric methods? It is of course to use some kind of peer-review process. But even peer review has its disadvantages. It is time-consuming, and it only functions well if the chosen peers are not only good scientists themselves, but also have a broadminded approach to what constitutes excellent scientific contributions in their own field. Too many scientists who are excellent researchers consider somewhat naively that the only right way to do science is exactly the way they themselves do it. And often these scientists become referees, advisors or even the decision makers at various levels. This attitude to excellence can have serious consequences, since really promising scientists or projects may get lost in the selection process. I often find it necessary to remind a committee of peers of the truism that excellent contributions to science can be made in very different ways and at very different levels in the research process.

Where can we find excellent science?

I am sorry to repeat this truism – but there are many ways to do good science and even excellent science. One researcher may formulate a new hypothesis based on the failure of an older one. This may be an important contribution even if the scientist involved is not able to test the hypothesis herself. Other scientists may develop important new methods or equipment, or painstakingly collect data in a laboratory or in the field using established methods. Similarly, just collecting the necessary data or doing the right experiments to test a new hypothesis formulated by someone else could also constitute excellent and valuable science. Critical reanalysis of other people's published or unpublished works, with full acknowledgement of the original author, can result in important new insight and thus represent excellent work in science. There are many different scientific activities which can be excellent if they are done in the right way.

Interestingly, even formulating a hypothesis that are later proved to be wrong can be an excellent contribution to science. May I remind you that the well-known neurobiologist Sir John Carew Eccles from Australia believed for a long time that connections between neurons were electrical, not chemical as we know today. He first defended his incorrect hypothesis through a variety of experimental work which he published over many years, but later proved himself to be wrong and changed his position. He was finally awarded the Nobel Prize in Physiology and Medicine for all this work, which is a superb example of what is meant by excellence in research.

What are the consequences for European research councils?

So what are the consequences of all this for the work we do for instance in the European Research Council? Do we manage to identify the likely excellence of researchers and the research they are proposing? I believe those who are invited to be committee members, panellists and referees do the best they can. But the real issue is what is the excellence we should be funding, given the public character of the money whose use we are deciding on in this European institution.

The US can give us a good indication, especially the National Science Foundation and National Institute of Health. The latter is the prime funding body for biological and medical sciences. The American system tracks a whole innovation line, from very basic to very applied research. In the EU, the Framework Programme has shown much more interest in applied results, in line with the Lisbon goal of increasing European industrial competitiveness. My impression from the European Research Council is that we are now opening up the quest for excellence to include the whole innovation line, as we wish to support not only basic research but also applied research ideas provided they are at a frontier of knowledge – frontier research. Excellent research and excellent ideas can found at all points on the innovation line, from the very theoretical end of the scale to the very practical one. It is very often the same people who have ideas at different points on the scale. And we must give them a chance.