How to define research impact

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Thank you for inviting me here today to discuss the very important subject of how to define and measure research impact.

I am afraid that you have been exposed with what people call "a growing unease amongst policy-makers and the public with the lack of immediate societal or economic impact of publicly funded research".

A sign that this pressure is mounting can be seen in the fact that the 2019 Annual Meeting of the Global Research Council (GRC), bringing together heads of research funding agencies from around the world (last year 70 countries represented), has put on its agenda a discussion on ways in which research funding organisations respond to increased expectations of societal and economic impact.

In reality the question of the economic value of science is as old as science. We are told for example, that in 1850 the great scientist Michael FARADAY was asked by the then UK Minister of Finance: "*What is the practical value of electricity?*" To which he is said to have replied: "*Why, Sir, there is every probability that you will soon be able to tax it.*"

So this debate is not a new one, and one can say very broadly that two schools of thought regarding it have arisen over the years.

Firstly, there are those who believe in a science led approach. That is one should fund the best proposals put forward by scientists themselves as selected by their peers as the surest way to generate good science, that will then have a societal and economic impact.

Secondly, there are those who believe in a relevance led approach. That is one should fund projects in priority areas defined by society to ensure scientific results which are relevant to the needs of society.

[1] Many thanks to Benjamin TURNER for his help in the preparation of this speech.

On the face of it, these seem rather different approaches and the discussion of which is the best can become rather heated with real consequences on how research funders operate and how they relate to the scientific community.

You will sometimes see the debate framed as being between, on the one side, idealistic academics who wish to do "science for science's sake" and be left alone in their ivory tower, and on the other, hard headed policymakers protecting the interests of the taxpayer and caring for the wellbeing of citizens.

One can also consider the photo negative of this framing, namely when scientists claim that bureaucrats and politicians should not meddle in a scientific process which they do not understand.

Needless to say, it is rather unhelpful to frame the debate in either of these ways.

So let me be clear from the start: I have no doubt all of us, who have been given the responsibility of spending (large) amounts of public money, feel a strong obligation to spend it well. Researchers tend to be very highly motivated individuals, and go into science to make a difference. Let's face it: the overwhelming majority of us do not go into science to become rich!

The question therefore is not one of motivation. In reality, the only question is how to achieve the best results.

And here I think there is actually a large measure of consensus.

For over 200 years economists have been studying the classical factors of production: land; labour; and capital. But, starting with Robert SOLOW (who won a Nobel Prize for this work), economists in the 1960s and 70s came to realise that at most half of the historical growth could be explained by known factors. The rest could only be explained by introducing a new factor of production: technological progress.

Nobody now disputes this claim. The issue, if you are primarily concerned with economic impact, is therefore how best to support technological progress. And here again there is a high level of consensus. Firstly, it is accepted that technological progress requires both basic or curiosity-driven research and applied research. Secondly, it is accepted that governments bear the central responsibility to fund basic research. That is, because the applications of such research cannot be foreseen, and possibly come with a long time-lag between fundamental discoveries and their translation into practical returns .

And again very few now dispute this form of "division of labour". According to the OECD's latest innovation strategy document from 2015, *"public investment*

in scientific research is widely recognised as an essential feature of effective national innovation systems. Public research plays a key role in innovation systems by providing new knowledge and pushing the knowledge frontier. Universities and public research institutions often undertake longer-term, higher-risk research and complement the activities of the private sector. Although the volume of public R&D is less than 30% of the total OECD R&D, universities and public research institutions perform more than three-quarters of total basic research."

So why then do we in the basic research community feel that we are under constant pressure to justify our activities and our budgets? I believe that there are two related reasons.

The first one is that, even if the importance of basic research for technological progress is accepted, the way science relates to it and to economic growth is inherently complex and still poorly understood. Too simplistic models tend to prevail in the minds of many: *"you put enough money on a problem and it gets solved"*.

The misunderstanding may arise from the many successes of the past. As people have seen a sustained stream of findings, technologies and innovations appear decade after decade, a number of people have come to think of it as an easy and, in the end, predictable process. Non-scientists could therefore grow impatient and imagine they can order whatever "innovations" they might like, as if from a menu.

But of course we know that science does not and cannot work that way.

First of all, let me insist that we do care about the scientific impact, the ways disciplines transform themselves because of new results, new concepts, new models... and of course also for experimentalists new scales, whose exploration becomes accessible due to new tools. We must always recall that new domains of science appear all the time, and new combinations of science become fruitful and require a new attitude and a new spirit from scientists.

Coming back to technologies, we must stress that they harness natural phenomena. These phenomena exist in the world regardless of our desires.

It was not decided one day that better means of communication were needed and then somebody discovered electromagnetic waves. They were found by Heinrich HERTZ who emphasised the beauty of physics and who based his work on the theoretical considerations of Sir James Clerk MAXWELL.

The basic circuits used in computers were not found by people who wanted to build computers. They were discovered in the 1930s by physicists dealing with the counting of nuclear particles, their topic of interest.

Many of the commercially successful inventions that have driven economic growth in the last decades come from research conducted with no commercial purpose. This was clearly stated by Claude SHANNON, one of the founders of "Information theory", that today lies at the heart of all the tools we are constantly using, from cell phones to television, and that made internet possible. Here is what he said: *"I am very seldom interested in applications. I am more interested in the elegance of a problem. Is it a good problem? an interesting problem?"*.

In 2012 one of the first to recognise the significance of CRISP-R, that allows a totally new approach to genetic engineering, was Jennifer DOUDNA, who began to work in this area because she thought *"the chemistry might be cool"*.

Now, my second reason is that we scientists need to do a better job of explaining how science works. Sometimes we too easily say, *"leave it to us, just give us more money and great things will happen"*. If we behave like this, then we ourselves are contributing to the impression that science is easy and predictable, when we all know it is hard work and we often fail!

We need to be clear that basic research is essentially trying to understand how things work. This can in some circumstances lead to identify new phenomena, i.e. phenomena nobody has ever come across, or develop new concepts thanks to which we can make sense of these otherwise unexplainable features.

We also need to be honest that not every project or research programme will deliver a "breakthrough" in our knowledge. There is indeed no linear process by which scientists make discoveries, then harnessed through a complex chain of actions.

We therefore need to contest the idea that asking researchers to address impact ex ante as only possible basis for having their research funded can do no harm. Forcing researchers towards expected, intended and immediate impact risks missing out on truly transformational discoveries. It can also lead to gaps in scientific knowledge in areas, which are not of societal interest at one point in time, but might be so later, and even become of paramount importance. In recent years health care has provided ample evidence of the need to anticipate society's needs.

Moreover, requiring scientists to set out what the impact of their project will be beforehand may decrease trust in science if these promises are not delivered.

Further, the channels through which basic research feeds into the economy are many and diverse. It is not just about the occasional breakthroughs. Fundamentally, basic research increases the stock of useful knowledge, both codified (e.g. in terms of publications) and tacit (skills, knowhow and experience). It plays a decisive role in training skilled graduates and researchers in solving complex problems, produces new scientific instruments and methodologies, creates international peer networks through which the latest knowledge circulates efficiently, and can even raise new questions about societal values and choices.

A strong science base allows countries to be at the forefront of knowledge creation because, without this knowledge, individuals, firms or countries lack the capacity to identify and absorb potentially exploitable knowledge created elsewhere.

Of course we could say much more on the interactions between basic research, technological progress and economic growth.

It is time now for me to conclude.

While I do not believe that limiting research funding to a toughly selected group of researchers is enough to guarantee that any particular scientific project will have a strong "impact", it is clear to me that our fundamental duty is to try and educate policy makers about the very process by which science feeds innovation.

We must explain to them why it is legitimate to refuse to get into the game of trying to demonstrate the unknowable a priori, and to limit the indispensable breadth needed to consider science from many angles.

In a nutshell, the best bets are made when scientists are pushed to their boundaries, when submitting research proposals, and the most competent evaluators are confronted with these challenging projects. You may have to press them to take risk, as our community is actually spontaneously conservative and needs to be put outside of its comfort zone to accept some bets. This is precisely what the European Research Council is about, and I hope it plays its part in this process of educating policy makers.

Finally, and this is not a minor issue, we must not forget that the most essential constituents of the research system are the researchers themselves, the human beings who make all this exist and function. In consequence it is of the greatest importance that the system provides them with a decent career path as nobody would enter a demanding working environment without being given some

assurance that there is a chance to advance and be rewarded. It is there that you, in charge of important European universities, have a central role to play as you are the main providers of employment for them and the key channel to bring talented young people into the profession. Of course to do that you need to be given the means to create the right conditions.

If these basic conditions are not met, we should not be surprised that the best researchers simply leave Europe to carry out their dreams elsewhere or leave research altogether. Any country, region or institution that wants to improve its capacity to deliver the best research needs to get these conditions right, and Europe would benefit greatly by providing the right platforms to share best practices.

In particular, together we need to plot out a sustainable career path for talented young researchers across Europe. Getting this right is far more important than any particular rules we might decide to apply to our calls for proposals. This is why we need to prepare ourselves in the right way to the fight to secure future resources for activities we all cherish…

Even if these activities are so relevant, in the long and short term, for the development of the society we live in, a society that must respect the core values academia represents. A key one is academic freedom, presently threatened in a number of countries in the world, including in Europe. We must defend it without compromise. This is our utmost duty!

I thank you for your attention.