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Jean-Pierre Bourguignon,

President of the European Research Council

Distinguished guests, dear colleagues,

Ladies and Gentlemen,

I would first like to thank the President of the Royal Irish Academy, Professor Mary E. Daly, and Professor Nicholas Canny, one of my colleagues at the ERC Scientific Council, for their invitation to speak to you here today. It is a great pleasure for me to address you at this dedicated event.

And it is appropriate that the event be held here at the Royal Irish Academy. The Academy's leading role in promoting Irish science and culture - both here and abroad - is well recognised.

I know also how previous debates held here have been very influential in shaping public policy. So I feel a certain sense of responsibility. And I hope that this keynote address will touch points that you will consider relevant.

Because I want to discuss what I believe is a fundamental issue, namely: how should research and innovation be funded? Are the different ways of approaching this issue having some secondary effects that could counter their primary effects?

I think that all of us would agree that science has been one of the most formidable human adventure, with deep impact on the way we think, on the way our societies have evolved, and even on the way our daily lives develop. More and more often, in recent years, innovation and economic growth have been

presented as closely linked – there is actually ample evidence to this effect – and the question of the involvement of researchers in the further link between scientific developments and innovation has become centre stage.

But many questions remain: What is the balance between supporting "blue sky" research and near-market development? What is the proper role of the public and the private sectors? Should smaller countries follow different policies from larger countries? To use the Brussels jargon - what is the right "policy mix"? I am personally even more worried by yet another question: How do we ensure that young people with the appropriate potential do not turn away from research careers?

The Minister has set out very eloquently just before me the current policy in Ireland. It is of course not my intention to comment in detail on the specific policies adopted here. However it is my intention to set out some basic principles. Principles which, I think, have a broad if not universal relevance.

The European Research Council operates according to simple principles. The ERC supports scientists from anywhere in the world, of any age and from any field of research - including the social sciences and humanities – with no predetermined targets or quotas. The ERC provides substantial, long-term funding, up to 3.5 million euros, to projects that may take up to five years under the leadership of a researcher, who can propose the organisation of the work towards the goal to her or his choice. The only conditions are that ERC funded researchers must spend at least 50% of their working time in an institution based in Europe. They must also be willing to be adventurous and to take risks in their research.

And this, with the involvement of evaluators of the highest possible calibre with the right mix of profiles, to me is the real value of ERC funding. Long term funding frees researchers from having to focus on short-term, if not immediate, impact, from thinking of having out the next publication before the full story is known, from getting distracted from the work developing by the need to write the next grant application. It allows researchers to really focus on the core of their research. In this way we hope that their work can lead to genuinely new knowledge, and, in some cases, even to radical breakthroughs.

I do not pretend that this vision of how to fund research is a new one. People who set up the ERC do not claim to have invented a new model. In fact numerous members of the European scientific community – I was one of them – campaigned long and hard to obtain from the European Commission that a scheme like ERC, that can finally rely on tried and trusted principles, be put in place. This required proper legislative changes, but, after those changes were made in the context of the Lisbon Treaty, one had to come up with a precise scheme and the right group of people to get everything up and running.

For example, the predecessor of what is today the Max Planck Society in Germany, was set up in 1911 according to the "Harnack Principle". Alfred von Harnack, the first President of the Kaiser Wilhelm Society, advocated the right of researchers to work independently of government or private requirements and unencumbered by bureaucracy. And to this day Max Planck Institutes are following this principle: They are built up solely around some of the world's leading researchers, who themselves define their research subjects and are given the best working conditions, as well as free reign in selecting their staff.

In 1918 Richard Burdon Haldane chaired a committee which produced a report recommending that general research should be under the control of autonomous Research Councils. These would be free from political and administrative pressures that might discourage or biase research in certain areas. The principle of the autonomy of the research councils is now referred to as the "Haldane Principle". The first research council to be created as a result of the Haldane Report was the Medical Research Council.

In 1939, Abraham Flexner wrote a famous manifesto with the title *"The usefulness of useless knowledge"*. It was the blueprint for establishing the Institute for Advanced Study in Princeton, later home to Einstein, Gödel, von Neumann, Oppenheimer and many others.

And after the Second World War, Vannevar Bush made similar arguments in "*Science, The Endless Frontier"*, his 1945 report to the President of the United States. This called for an expansion of government support for science, and the creation of the National Science Foundation. Famously he stated: *"Scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown. Freedom of inquiry must be preserved under any plan for Government support of science."*
I don't wish to argue that there is a consensus on how what is the best way to fund research. Far from it. There has always been competing visions. The Haldane principle has remained enshrined in British Government policy, but has been criticised and altered over the years. In 1939 J.D. Bernal argued that social good was more important than researchers' freedom in deciding the direction of research. Solly Zuckerman criticised the principle in 1971 for its artificial separation of basic and applied science. To him this led to the elevation of the status of the former.

During the 1960s and the 1970s, there was a growing feeling that science could not just be left to scientists. Science, and especially technology, should be harnessed to societal needs.

This led to the creation of innovation agencies, innovation-focused industry policies and other new ideas, such as *grands projets,* aiming to shift attention towards fostering innovation as well as fulfilling societal needs.

The OECD was instrumental in this shift: In 1963, a working group led by Christopher Freeman (a great admirer of JD Bernal and who later founded the Science Policy Research Unit at Sussex University) produced the Frascati Manual, which defined how to collect R&D statistics.

The same year, the OECD organised the first international meeting of ministers of science; Two years later, it established a committee and an internal department for science policy, led by Jean-Jacques Salomon. This promoted the idea of the existence of a 'technology gap' between the USA and the rest of the world. This gap justified the need for science policy.

The 'OECD line' came to be that:

1. Research should help reach national, politically-determined goals.
2. Research should be planned and organised to that end.
3. Research should be more interdisciplinary, in order to solve real-world problems.
4. The universities were rigid, organised by discipline and unable to change themselves. They should be 'reorganised' in order to contribute more to the solution of societal problems and to reach national goals.

The increasing state R&D budgets of the time had high mission content. And new terminologies such as 'strategic research' and 'targeted research' began to emerge.

Alongside this increased emphasis on the utility of research, the 1960s saw the emergence of active “industrial policies”. Governments, especially those of the UK and France, attempted to create national champions in industries deemed essential to the health of the national economy and defense. Among the favoured sectors were high-technology industries such as aerospace and computers. Again part of the motivation was to narrow the “technology gap” between Europe and the US. There was also a widely held belief in scale as the key to international competitiveness.

These developments have continued. Ben Martin, the highly respected expert in “Science policy and innovation studies” from SPRU, has summarised what has been learned in fifty years of innovation policy: Policymakers have not been short of advice on how best to enhance the impact of technological research through prioritisation of research areas. And today policymakers are being asked by the European Commission to pursue Smart Specialisation and Key Enabling Technologies.

But the approach of Bush has never gone away. Countries have continued to fund basic or blue sky or curiosity driven research throughout this period as ERC does it today (on average most countries allocate around 20% of their national funding to basic research).

Because the fact is that, despite fifty years of innovation studies, the best form of intervention is still not clear.

Martin concludes that we have achieved a greater understanding of the complexity of the innovation system. But he admits that this makes it harder to give clear recommendations to policymakers.

And the disadvantages of a top-down approach have become more apparent.

With some exceptions the industrial policy of the 60s and 70s was generally not as successful as its advocates wanted it to be. Policy-makers tended to overrate the risks and costs of market failures. And to underestimate the risks associated with government failures.

From the 1980s on there was a shift towards horizontal, non-selective policies aimed at improving the environment for all firms. Both at the national and at the European level (through the Single Market Programmes), more emphasis was placed on competition. The ability of governments to support their industries was curtailed. And previously protected sectors such as telecommunications and electricity were partially liberalised.

At the same time new institutions were established: The Framework Programme and Eureka were set up to promote intra-European cooperation in research.

The surge in US productivity growth from the mid-1990s, linked to the rapid application of information technology, led European governments to encourage the growth of entrepreneurial high-technology firms on the American model.

By the early 2000s some progress had been made. But there was still a wide productivity gap with the US. And in several high-technology sectors, such as information technology and biotechnology, European firms were lagging behind their American counterparts with the notable exception of the aerospace industry, through Airbus. There was also a growing concern about de-industrialisation. Especially with the shift of manufacturing to China and other emerging economies.

These changes were reflected in government policies to support research and innovation. At the EU level for example, there was more emphasis on "excellence" and creating a European Research Area. Such approaches were also taken by some national governments, e.g. in Denmark and in Germany, which established "Centres of excellence".

However, the financial crisis of 2008-09 led to another shift. Governments again provided short-term help to ailing industries such as the car manufacturers or the banking sector. But they also began to consider whether a more active industrial policy might be needed in the longer term. Some economists argued for a revival of sector-based policies in a form that would avoid the mistakes of the past.

So where does that leave us? Are there some basic weaknesses in the analyses put forward? Is the situation in Europe homogeneous enough that a uniform policy can work?

We can see that research and innovation policy, including industrial policy, has gone through several cycles.

Different national models have also come and gone. Over the years policymakers have been told to look to the Soviet Union, to the US, to Japan, to Finland, and now to Korea.

But there does seem to be one thing on which everyone does agree. That is, basic research is a classic public good in economic terms. The output of basic research is 'non-rival'. This means that many people can consume it at the same time without it being used up. And it is 'non-excludable'. This means that it is hard to stop people getting access to it. Actually, there are going on considerable access to give to it more Open Access. Therefore, the private sector will be very reluctant to spend adequate amounts to basic research, leaving it to the state to fund it.

The fact is that the ERC plays that one role in the research eco-system which we are sure makes it necessary for States to fund it.

This was very well formulated by the Irish former Commissioner Geoghegan-Quinn when she spoke here at the RIA in November 2012. She said that ***"…intellectual inquiry is a valuable and worthwhile pursuit in itself. Science satisfies our need to understand the world around us, to understand the great variety of life on earth, and to understand ourselves."***

And it is always worth repeating the testimony of Robert R. Wilson before the US Congress in 1969. The discussion was about the value of building an accelerator at Fermi's lab. He was asked whether the accelerator would help US defence and security. His reply was: ***"…it has nothing to do directly with defending our country except to help make it worth defending."***

Yet, even though I believe that the points made by the previous prestigious persons I just quoted are indeed true, it is not the only reason why basic research must be funded. It is simply a fact that frontier research can lead to tremendous economic rewards, by giving rise for example to completely new economic sectors, to many important new products, to major industrial developments, affecting the computer industry as well as pharmaceuticals. Many critical new steps have their origins in publicly funded research conducted at universities and research institutions; many of the commercially successful inventions we now take for granted and which have driven economic growth come from research conducted with no commercial purpose.

Think of Google: at its base is a piece of pure science, i.e. an imaginative algorithm (by the way, it has taken scientists some time to realize how powerful that algorithm can be to prove theorems). The software “Page Rank”, developed by Sergei Brin and Larry Page, came from a project produced with the help of funding from the NSF. The grant, covering several other things, was worth 4.5 million dollars. Google is now worth nearly 400 billion dollars!.

The link between curiosity driven research and applications is not new. Take the members of the UK’s Royal Society in the 17th century. They investigated such esoteric phenomena as magnetism, optics, universal gravitation and the motion of heavenly bodies. But it was obvious to them that this would lead to practical use in instrumentation in watches and engines, indispensable for carrying out trade on land and across the oceans. It went much further as it laid the basis for the industrial age.

Vannevar Bush was no ivory tower academic. He led six thousand American scientists in the U.S. Office of Scientific Research and Development (OSRD) during World War II. This was the body through which almost all US wartime military R&D was carried out, including the first phases of the Manhattan Project. He also later founded Raytheon, a major US defence contractor to this day. So this was somebody that was intensely interested in getting practical results. Yet he understood that allowing freedom to scientists, targeting in piority the brightest, was a way to get these results.

Bottom-up, curiosity driven ERC funded research is starting to produce results. Since 2007, over 4 300 research projects have been funded in some 600 distinct institutions in 30 countries. Some 20 000 PhDs and post-docs have been employed on ERC teams.

The direct output of the ERC is the science it funds. The publication of the results of research is an essential part of the scientific method. Scientific publications report the findings of original experimental and theoretical work in appropriate scientific journals as well as the methodologies that led to the results.

The expert judgment of scientists in the field is necessary to properly assess the scientific importance of a particular publication. And even then the significance of some papers can be missed for many years. Nevertheless, if we look at the 30 000 articles, that came out of the research projects funded by ERC, then **12%** of the articles published between 2008 - 2010 were in the **top 1%** most cited articles in the world in 2012. This is impressive given that by definition only 1% of articles can be in the top 1%, and less than 1% of articles with an EU author are in the top 1% on average.

So thee are very encouraging early signs that the ERC has already supported some of the most significant research worldwide in the last few years.

But let me be clear again. I do not claim that science leads directly to economic growth. With Google the science part was not enough. a lot of work and entrepreneurial spirit had to be mobilized to make this huge step. The most obvious one is the economic model which leads to one of the wealthiest companies of the world when the use of its products is free to the huge majority of its users. It took an entrepreneurial vision, the development of a business model, venture capital and many talented people to turn that algorithm into one of the world’s biggest, most profitable businesses.

Of course the innovation process is a complicated one, with many linkages and feedback mechanisms. And it needs to be understood at a systemic level. It is also increasingly global. Innovation does not follow a neat linear model in which "an innovation" follows directly and rapidly from "a research project". And there is no way to constrain the benefits in the same geographical location as the research takes place.

To explain this I think it is very important to understand something about research. And that is for every breakthrough there are many more results which are only of interest to scientists in the same field. And even more results that are what we could politely call “for the record”, but one must be very careful

that “results for the record” may turn to highly relevant ones when looked at from a different angle or in combination with others.

A significant proportion of all research papers are never cited. This does not mean this research should not have been done. It is simply that for any real research project the outcome is by definition unknown, or it is not really research. Even if lots of research produces little impact, some research produces immense impact. And we do not know which is which beforehand.

Furthermore, I would argue that what is true of research is also true for the applications of research. One should not think that investing in applied research is a surer thing while investing in basic research would be risky and uncertain. It is very difficult to predict from where a truly important new development will come. Even experienced and successful venture capitalists hope to succeed with only a small percentage of their investments, and many of them are real bets. The best ones are very conscious of that, and this is precisely why they diversify their investments.

Many of the building blocks of the computer age: Turing machines, programming languages, even the internet itself, can be traced back to genuinely new ideas conceived in the academic world. This being said, I don’t think anyone predicted the massively interconnected world in which we live today. Smart phones, tablets, soon watches and countless applications that have radically altered the way information is produced, accessed and processed, pass through cyberspace and interact with people anywhere around the globe with a formidable transforming effect on the way industry is organized, without even pointing to completely new areas. Investors now talk about “Industry 4.0”.

Nobody asked for this. But these technologies meet deep human needs that have been long thought after. These domains have seen the creation of multi-billion euro companies from nothing. They have radically changed the way we work and play. And we are only beginning to see the effects of these technologies in our daily lives.

The link between intellectual creativity and innovation requires some deeper thinking. It is a critical relationship which cannot be judged merely in terms of patents and statistics.

To achieve such great innovations with lasting impacts on our societies, we need further developments in basic scientific research. Only led by the principles of academic curiosity and scientific excellence can we hope to discover the unexpected. We must invest in understandings before we can expect tangible applications.

In my experience the more visionary private sector leaders do very much understand the importance of government support for fundamental research. They understand the wider benefits of excellent science I mentioned above, in particular high tech companies need highly skilled, trained and versatile people. The added value of well trained, well motivated and ambitious people can never be stressed enough when the focus is often too much on amounts, on processes and on structures. There are indeed more or less proper environments for researchers to develop their projects, but the key issue is to challenge them to deliver the most ambitious projects they can think of. This is precisely what ERC does through a totally “bottom-up” approach. It has organized itself to be ready to appropriately evaluate projects of all kinds submitted by researchers. This is a real challenge, that actually turns out to be also one of the reasons why ERC has been able to attract an exceptional group of evaluators because they see this hard work as a formidable intellectual adventure.

Unfortunately, I speak to you today at a time when several governments have taken the decision to cut many areas of government spending. The cuts applied to the research budget have been particularly severe in the countries the most hit by the crisis such as Spain, with annual average decreases of -9.1% from 2008 – 2012[1]. Public R&D investments in Spain have returned to the levels of 2005-2006.

Faced with overall cuts I can see the temptation to cut basic research funding first as something whose return cannot be easily claimed in the short term when political leaders are under pressure to deliver results precisely in the short term. It appears to them easier to decide to fund technology X or solving problem Y than to say we are funding new knowledge and counting, on the basis of past experience in many different settings, for something to come out of it soon, or even not so soon.

But if we look at the most successful regions and countries of the world in terms of innovation, we see that their achievements are without exception built on a bedrock of excellent basic research.

This approach produces not only new knowledge, but also highly skilled researchers, access to international networks, new technologies and spin-off companies.

This is even more true as a country or region moves closer to the technological frontier. When countries are at certain stages of development they can enjoy "catch-up" growth simply by adopting existing technologies. This becomes increasingly difficult as economies mature. Europe has recognised this with the so called "Europe 2020 Policy" which makes research and innovation central to achieving its future goals.

I think that there is one particularly pernicious effect of such cuts: it seems that, unfortunately, their effect seems to fall disproportionately on the young ones, including in research. It is particularly important to continue to provide opportunities to young researchers to establish themselves as independent actors with adequate means to develop their own project. We must make sure that the most original and high calibre young researchers see a future for them here in Europe. At this moment data on researchers employment in Europe are vey shaky, with in particular very little visibility on the proportion between stable and unstable jobs. It is clear that the latter have grown very considerable in all countries and in particular in some disciplines where young researchers see with considerable apprehension the possibility of a career being postponed again and again. European research funding bodies such as the ERC cannot act as a substitute for national research funding. This is first and above all because the ERC's budget, for example, amounts to only 2% of EU public expenditure on R&D. The responsibility to support the potential of human curiosity rests equally with national and regional governments as well as with charitable foundations.

In the past couple of decades, Ireland has made significant advances in its research and development investment and has rapidly caught up with other European Union member states. Ireland's top academic institutions are able to attract high-level researchers, who are drawn by quality scientific equipment and infrastructures and the prospect of working with excellent colleagues and students. In 2003 Ireland was ranked 36th in the world for quality of scientific research output, but by 2010 the ranking was 20th. From 2000 to 2008 total spend on publicly-funded R&D in Ireland more than tripled (from €290 million in 2000 to 938 million in 2008). But thee has been a pick in 2008 in government funding for research in Ireland that has gone in 2013 to its 2006 level. In

[1] European Commission - *"Research and innovation as sources of renewed growth"* June 2014: http://ec.europa.eu/research/innovation-union/pdf/state-of-the-union/2013/research-and-innovation-as-sources-of-renewed-growth-com-2014-339-final.pdf

such circumstances it is not inappropriate for policy-makers to determine research funding priorities. But the fundamental argument of this keynote address has been that, in all research systems, it is essential that a significant proportion of funding is designated to exploratory, curiosity-driven research, thus offering some perspective to their brightest talents.

So my message to you today is simple. Ireland has made great strides. You have been through some very dark days. But I have every confidence that you will emerge even stronger than in the past if you do not neglect the foundations for future, sustainable growth.

If science is to make truly influential innovations, funding cannot be short-sighted. To maintain a healthy research ecosystem, it is right to invest substantially in long-term curiosity-driven research as well as in more targeted endeavours.