Societal and Economic Impact as Funding Criteria

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Thank you very much again for the invitation to speak to you.

While I had the pleasure to elaborate on impact from a more general perspective before, allow me to reflect now on one approach used by many funding organisations and GRC participants, namely: *the introduction of societal and economic impact as funding criteria*.

This introduction has happened in three ways:

First, the most common form of using societal and economic impact as funding criterion has been to include it as additional evaluation category with a specific weight besides scientific excellence.

Second, some research funders have also started to separate the evaluation procedures for scientific excellence and impact. So-called “impact panels” look at research projects, which were preselected on scientific excellence. They might also distinguish themselves from traditional panels by including non-scientific stakeholders such as company directors.

Evaluating the accomplishments of researchers with regard to Open Science practices has not been a common way of achieving societal impact in funding decision-making processes so far. I would nevertheless like to mention it as a third possible way, given the progress of Open Science practices worldwide.

Increasing the awareness of researchers for the societal and economic impact of their research promises a number of benefits:

It could encourage them to think at an early stage about how their research will benefit society and the economy.

They might have a better understanding of what is of relevance for society and adjust their research questions accordingly.

They may become aware earlier of how to translate their research into application.

Societal stakeholders might show more interest in and understanding of the research outcome.

However, employing societal and economic impact as *funding criteria* goes beyond raising awareness and raises several fundamental questions, that need further debate.

Please allow me therefore to elaborate on three assumptions as to what it entails, to using societal and economic impact as funding criterion and how these assumptions are reflected in the GRC Statement of Principles.

My first assumption is: *Asking for research impact will not lead to more, but to different research impact.*

Asking researchers to make a case for societal and economic impact in their proposals will confine their proposals to the so-called *known unknowns* – the problems, which society already conceived of at the time of research. This will make the impact of their research more predictable *ex ante* and short-term, which is a valid and often necessary strategy, for example if a government needs research to address a sudden health or energy crisis.

Societal and economic impact assessments can therefore be a valuable instrument in mission-oriented settings, applied research or priority areas as they assist in selecting research proposals, which are expected to be better capable of contributing to specific short-term objectives and missions. You will find this reflected in the GRC Statement of Principles as principle number 10.

At the same time, one should not forget, that these objectives and missions change over time, not least because they are subject to political debate. In order to be able to adequately and timely respond, when new societal challenges emerge, one should avoid gaps in scientific knowledge in areas, which are not of societal interest at one point of time, but might be so later.

My second assumption is: *Not all forms of research will be suitable for societal and economic impact assessments.*

Orienting researchers towards predictable and short-term impact is not risk-free. It could happen at the expense of the *unknown unknowns* – the problems, which society did not already conceive of at the time of research.

Many research projects, that resulted in key technological advancements, would not have fulfilled impact criteria of today, when they were first proposed.

Examples include Hertz's electromagnetic waves, Einstein's general theory of relativity, Drude's investigation of the reflexion of metals, and Bardeen, Brattain, and Shockley's work on semiconductor contacts. Without them, we would have missed many things: from radio communication to transparent touchscreens, integrated circuits, processors, memory chips or GPS.

These examples show, that curiosity-driven, fundamental and not thematically confined research can lead to disruptive innovations, although researchers do not explicitly calculate it or plan for it prior to conducting their research.

We therefore need room for curiosity-driven research, which remains free from societal and economic impact assessments. Even if its societal or economic impact might materialise only years or decades later. Often, new insights from one research area depend on connections with other disciplines – but these links take time to emerge.

Investments in curiosity-driven research, whose impact manifests in unexpected ways and undetermined timeframes, will very likely be more difficult to sell to policy-makers than research, that produces short-term impact. However, they are equally essential. By including these reflections in principle 12 of the GRC Statement of Principles, we hope to establish a point of reference, which GRC participants can come back to in negotiations with respective policy-makers.

My third and final assumption is: *Ex ante assessments of societal and economic impact should not be expected to increase societal trust in research.*

While impact assessments will increase the awareness of researchers for the societal and economic impact of their research, it is far more doubtful whether they are a suitable instrument to contribute, on a more general account, to increased societal trust in research.

On the contrary: Requesting promises of immediate practical benefits in project proposals – from the creation of jobs, to the defeat of major wide-spread diseases, to the salvation of the world at large – could result into a spiral of one-upmanship between impact requirements and impact promises. A proliferation of ultimately undeliverable promises from researchers would not strengthen societal trust in research as much as it would threaten to ruin it.

I therefore believe that we need to ask ourselves whether research funders truly have better chances to gain societal trust by building research funding on promises of societal impact, which can be disappointed, or by seeking understanding of policy-makers and the public, that scientific progress often is unpredictable in limited timeframes.

The GRC Statement of Principles perfectly ends with this plea for raising awareness in particular outside of the research system for the genuine way, in which impact is generated during the research process.

Consequently, I will contend myself at this very point as well, looking forward to very stimulating discussions in our panel.