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**Introductory Talk, “Global Symposium on Scientific Breakthroughs”,
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Ladies and gentlemen,
dear colleagues,

First of all, let me thank you, dear Yuichiro and the JSPS, for having organized this symposium. I remember very well how the two of us discussed your ideas for this event and the central topic of this year’s Annual Meeting of the Global Research Council during dinner at a Mexican restaurant in Washington DC, sometime in April last year. The restaurant was just across from a basketball arena, packed with fans and incredibly loud – but the atmosphere was unique, and it was a truly inspiring experience. Just as inspiring and thought-provoking, I may say, as this event, and so I look forward very much to the discussions we will have in a few minutes.

Now, to provide some food for discussion, let me depart for a moment from my actual assignment here, and allow me to put forward two general arguments instead: I would like to contend, first, that we face a crucial paradox when we try to fund scientific breakthroughs or, to use another term: scientific innovations. The paradox is this: Real innovations are those breakthroughs that come about unexpectedly. And this means we cannot actually plan for and organize them. So, in our funding strategies we have to institutionalize something that we cannot actually institutionalize.

My second argument is more programmatic: I would like to emphasize here that if we want to have innovative research systems, we have to nurture and fund different types of innovations. This is far from self-evident, I think: If you take a

look at public research policies, you will find that they often operate on the assumption of a linear value chain. The idea here is typically that funding strategies and research expenditures fuel scientific innovations, that scientific innovations lead to new applications, that new applications translate into products, and that those products, in turn, assure the wealth and well-being of our societies.

Such linear concepts often privilege scientific innovations of one particular kind. Let me call this type of innovations the ‘old New’. By that I mean those kinds of scientific innovations that contribute to pre-defined sets of problems, that follow common paths and simply refine existing paradigms and approaches, and that we can therefore anticipate to some extent.

We typically find such innovations where researchers seek to find solutions to the social, economic, and environmental challenges of our world, where they make the social, ecological, or economic usefulness of their knowledge the key criterion of their work, where they conceptualize their research in terms of ‘problems’ and ‘solutions’.

This, of course, is legitimate and important. However, it often comes along with certain path dependencies. It means that researchers move within pre-defined paradigms, and that current grand challenges – not scientific curiosity – determine what kind of research researchers can legitimately pursue. And it means that the kinds of innovations that researchers are expected to produce are limited to finding solutions to those problems defined by society itself.

Now, the point that I would like to make here, is that the innovativeness of our research systems not only rests on the ‘old New’: on those innovations that we can plan for, predict, or ask for. To a large degree, the innovativeness of our research systems also depends on what I would like to call the ‘new New’: it depends on those surprisingly scholarly insights and scientific breakthroughs which we did not expect, which we did not plan for, and which we did not predict or anticipate. It is these insights, borne out of scientific curiosity, which lead to the truly transformative breakthroughs that change the ways we think and act, precisely because they openly break with our expectations.

American sociologist Robert K. Merton called this “serendipity”, by which he meant that, in our quest to look for something, we often find something entirely different. And we can easily see just how important this principle is when we think of Christopher Columbus: After all, Columbus only discovered America by accident, and yet, in retrospect, that discovery was nonetheless somehow quite relevant.

So, what I would like to suggest is that we have to keep the importance of the 'new New' in mind when we speak of funding strategies. And, as I said at the beginning, this means that we have to address a crucial paradox. As research funding or research performing institutions we have a great interest in predictability: We want to make sure that the research projects we fund or pursue will yield the results we expect – yet, as I just said, the real transformative innovations are those that break with our expectations, that disrupt what we had anticipated and predicted.

The challenge for us as research funding organizations, then, is this: on the one hand, we have to be aware that our funding strategies cannot shape processes of innovation themselves. They can only create framework conditions under which these processes take place. And on the other hand, we have to find strategies that nonetheless allow us to organize these processes of innovation that we cannot actually organize.

At the DFG, we try to deal with these challenges in two major ways. For one, we keep our funding decisions free of any social, economic, political or other influences and deliberations. We do not ask for impact or for employability. We only ask for exceptional scientific quality and for academic originality.

Secondly, we fund researchers in what we call the response mode: this is with permanently open calls, without deadlines, thematic stipulations or demands. Individual researchers, groups or institutions can submit grant proposals to the DFG at any time and on any research topic. We also listen very carefully to what researchers and universities need and have to say, and based on their proposals, may launch strategic funding initiatives.

This way, we aim at creating a climate of opportunities, where researchers enjoy the freedom to try new ideas which may sound unconvincing to others, but which still lead to major new insights and innovations.

And, I strongly believe, such a climate is essential to facilitate scientific breakthroughs.

Thank you.