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Editorial

Why Basic Science Must Remain Free

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Across evaluation panels for funding and job positions and strategic research agendas a clear pattern has emerged: scientific research is increasingly expected to justify itself through immediate applicability. Grant calls emphasize industrial partnerships, technological transfer, measurable impact, and short-term deliverables. Proposals are routinely evaluated and selected not only for their intellectual value but largely for their potential to generate marketable products, policy instruments, or societal solutions within tight timeframes.

The emphasis on applicability is comprehensible but on its own it cannot justify the current trend. Public investment in research is substantial, and accountability is legitimate. Mankind is facing urgent challenges, including hunger, water supplies, climate changes, energy transition, public health crises, food security, wars and geopolitical conflicts, that demand scientific engagement. Needless to say, these problems tragically affect the life of poor people and of developing countries, while rich nations are less affected or can afford some concrete actions to mitigate their effects. Yet, in the rush toward utility a fundamental principle risks a serious danger: the protection of basic science conducted without predefined practical constraints.

Basic science—driven by curiosity rather than by immediate use—is the foundation upon which applied innovation ultimately rests. It is neither an extravagance nor an academic gratification. Instead, it is the generative substrate of any discovery. History repeatedly demonstrates that the most consequential applications often arise from investigations that were not conceived with application in mind.

Quantum mechanics was not developed to enable semiconductors. Number theory was not formalized to secure digital communication. Studies of bacterial adaptive immunity were not designed to produce CRISPR gene editing technologies [1]. In each case, conceptual investigation preceded application—often by decades. In other cases, it was technology that preceded basic science and urged for new theoretical formulations.

The problem with requiring predefined applicability is not simply that it biases funding allocations. It reshapes the culture of science itself. Somehow in our times science and scientists are looking for the approval of the public, as in a TV show. Today everything is measured in terms of performance and efficacy. Science and more in general culture are subject to a generic consensus, that for a politician means more votes (with the exception of autocracies that are pretty popular in these times).

When researchers internalize the expectation that projects must align with industrial or commercial demand, several distortions may emerge. Usually projects become more and more conservative, focusing on topics that will provide "useful" and certain results in a not-too-long period of time. Second, politically and economically interesting topics will be promoted and financed, while less visible or less exploitable research will be sacrificed. Third, in general the evaluation takes into consideration the production of deliverables and a timeframe of about 3 to 5 years. Finally, proposals that are not rooted on solid and (apparently) well established theories are discarded as non-repayable.

For all these reasons there is a real risk of weakening the intellectual biodiversity of science, that is the real and fundamental fuel for the advancement of knowledge. Basic science generates frameworks, methods, and theoretical architectures that may later prove indispensable in unforeseen contexts.

The value chain of knowledge is non-linear. Breakthroughs frequently arise at disciplinary margins, in

exploratory work unconstrained by immediate demand. To suppress such exploration is to diminish the probability space of future innovation. Remember the lesson from James Clerk Maxwell on "crossed fertilization" among different disciplines [2], to be extended to basic science and applications.

Another despicable effect of this way of doing science has to do with the younger generations of scientists. Early-career researchers are increasingly trained in environments where grant-writing emphasizes impact narratives over intellectual audacity. If funding structures consistently privilege applied framing, young scientists may never be able to experience the search for fundamentals, especially those that are more challenging and still unresolved.

Of course, basic science is not opposed to applied research. On the contrary, both are necessary and mutually reinforcing. But applicability and performance cannot be taken as stringent criteria to recognize and support important research themes. People need answer for several concrete and practical issues but other sources of funding need to nourish fundamental research without the demand for rapid and efficient solutions. In fact citizens benefit not only from new devices and innovative therapies, but also from the deepening of knowledge. People must be persuaded that this is reasonable and sustainable.

Some guidelines for a correct adjustment between these two tracks should include: (1) Appropriate funding sources finalized to support proposals that address fundamental topics with a high level of intellectual originality and methodological rigor. (2) A longer timeframe for such theoretical or exploratory work where it is unforeseeable the production of deliverables in short times, with the understanding and recognition that conceptual advances represent a strong impacting result as they prompt (in due time) and stimulate applied research with all its outcomes.

In times of economic difficulties or when political elections approach, politicians become more sensitive to these issues and the temptation to concentrate on visible outputs is real. However without the advancement of basic science, applied research eventually exhausts its substrate and will not be able to face the new challenges.

The protection and promotion of basic science is not for nostalgic aficionados of an idealized old style academic past. But rather it is a farseeing investment in science, culture, education and pratical outcomes. The discoveries that will address the grand challenges of the coming decades may already be incubating in laboratories and theoretical explorations that, at present, appear detached from immediate utility.

The responsibility of research institutions, funding bodies, and scholarly communities is to ensure that such incubators are not extinguished by a narrow and anti-scientific conception of relevance.

When I was a teenager, politicians belonging to all parties used to explain people their short- and long-term policies, including the scientific and technological progress of the society. And certainly there was a respectful attitude toward science and scientists. Regretfully we have to recognize that this powerful and sensible push is dismissed todays and politics is driven by polls, not by a wise and shared perspective in pursuing the common good. Fifty years ago these words meant something, today they would be misunderstood if not discarded as ridiculous.

NOTES AND REFERENCES

[1] Clustered Regularly Interspaced Short Palindromic Repeats, a revolutionary gene-editing technology derived from a natural defense mechanism in bacteria. It acts as a precise, fast, and economical molecular tool to modify, delete, or regulate DNA sequences in living organisms.

[2] J.S. Reid, C.H.-T Wang, J.M.T. Thompson. "James Clerk Maxwell 150 years on". *Philos Trans A Math Phys Eng Sci* **2008**, *366*, 1651–1659. DOI: 10.1098/rsta.2007.2196.