Theoretical research without projects

This work is about theoretical researchers: mathematicians, computer scientists and theoretical physicists. People like them invented the computer that you are now using to read this text. People like them invented the very notion of computation. They are scientists, but they don't work in a lab. Their tools are the computer and the blackboard; their job, solving problems.

More often than not, theorists cannot solve the problems that they pose. More often than not, while trying to crack them, they bump into new, more fascinating questions. It is by following those wild inspirations that they discover anything worth discovering. If they are doing their job well, they should not know what they will be working on in six months' time.

When theorists apply for public funding to carry their research, though, they face a big problem: most funding agencies demand a 2-5 year research project. In the grant proposal, theorists are expected to list all the questions they intend to answer in the next few years.

Unfortunately, that is not how theoretical science works: one cannot "plan" discovering mathematical calculus, quantum cryptography or neural networks. It is impossible to anticipate these ideas: they just happen. And the time it takes to develop them to full maturity is typically much shorter than the time it takes the research agency to decide the outcome of the grant that should fund their authors.

Agencies also demand to know the theorists' "methodology". Namely, they want theorists to explain how they intend to prove this or that theorem. The honest answer is that they don't know. If they did, the theorem would be proven already, and they would not be applying for funds to crack it.

For the working theorist, applying for research funds is therefore a long and unethical task. It involves concocting an elaborate fantasy where they pretend to know what they are going to discover in the next few years and how. This takes a lot of time away from their research, around one month for the most important grants, with consequent loss of resources. Most importantly, it involves lying in an official document.

We have reached this situation because, up to now, research policies have been based more on political fashion than on solid science. To progress beyond this point, we need an open scientific debate on research funding practices, where the scientific method is applied to this problem, i.e., with hypothesis, models, and experiments.

This is what we do in our paper [1]. We propose a research funding scheme by which each research unit (be it a single scientist, a group leader or a whole institute) applies for funding, but does not specify how much. The decision of how much funds (if any) must be awarded to each unit is taken by the funding agency, based on the recent scientific activity of the unit and the prior funding which such a unit was enjoying.

Of course, we work under the assumption that the agency has a clear notion of what type of science it wants to sponsor. There are many ways to quantify or evaluate scientific productivity, and deciding which one suits best reflects a political stance towards research. We personally advocate for evaluation methods based on peer-review rather than, e.g., bibliometric data. However, as we show in [1], once an agreed measure of scientific productivity is adopted, the distribution of research funds is no longer a political problem, but a mathematical one.

We start by modeling the research system as a collection of research units (research groups, people or institutes applying for funding), each of which possesses a "scientific productivity function". These functions that relates how much science a given research unit can produce with the funds it holds to conduct research. They are also unknown, i.e., neither the research agency nor the scientists themselves can tell how they look like.

Relying on our mathematical model of the research activity, we show that there exist systematic procedures to decide the budget distribution at each grant call, with the property that the total scientific production of the research community will be frequently not far off its maximum possible value.

The simplest of such procedures is what we call "the rule of three", by which the funds that each research unit receives after a grant call are proportional to the research output of the unit during the previous grant term. In mathematical language, funds are distributed according to the formula:

$$x_i^{k+1} = X\frac{g_i^k}{\sum_j g_j^k}.$$

Here x_i^{k+1} are the funds that research unit i obtains after grant call number $k+1$; g_i^k, the research output of the unit during the k^{th} term; and X, the total science budget for the $(k+1)^{th}$ term.

The returns of this research policy must be compared with those of "excellence" schemes, whereby, under equal research outcomes, researchers which were funded in the past have a greater chance of receiving further funds. As we show, the latter policies very likely converge to configurations where the total scientific productivity is an arbitrarily small fraction of the maximum achievable by the research system. They are hence riskier than the rule of three.

In [1] we also study to what extent research policies can be cheated by dishonest research units. We conclude, for example, that hacks of the rule of three would require either influencing the evaluation stage or a coalition of research units.

In sum, what we propose is a radical reform of the current grant system. This reform is not a magical recipe for all the problems of scientific research. By itself, it won't eliminate the focus on popular topics, short-term goals, and conservative research. On the other hand, it won't force theorists to engage in unethical practices, its funding decisions will be transparent and it won't require the applicant to waste months of working time in writing project proposals. Moreover, we argue, using mathematical models, that it has the potential to steer the scientific community to a situation of maximal scientific productivity.

We hope that our work serves as the starting point for an academic (i.e., not political and definitely not administrative) debate on the way to manage publicly funded science.

[1] M. Navascués and C. Budroni, arXiv:1810.11387.