You Cannot Engineer a Nobel Prize

When truly important research begins, nobody says, "This will lead to a Nobel Prize." More often than not, researchers do not even know whether anything useful will come out of it. That is how **Prof. Dorota Gryko** of the Institute of Organic Chemistry of the Polish Academy of Sciences describes the reality of scientific discovery. In her view, Nobel Prizes cannot be planned, commissioned or strategically engineered. They emerge from fundamental research, often in unexpected ways, and almost always in hindsight.

How difficult is it in chemistry to identify in advance which research projects might eventually prove truly groundbreaking?

DOROTA GRYKO: Major scientific discoveries usually emerge from fundamental research, which is why it is very difficult at the beginning of a project to predict whether it could eventually lead to something on the scale of a Nobel Prize.

I recently read a biography of Irène Joliot-Curie. In her case, when she and her husband discovered artificial radioactivity, it became apparent relatively quickly that they were dealing with a genuine breakthrough. Situations like that, however, are rare.

By contrast, the discovery made by Jennifer Doudna and Emmanuelle Charpentier, the CRISPR-Cas9 system that became one of the most important tools in modern genetics, can ultimately be traced back to research on bacterial cultures used in yoghurt production.

Of course, one can identify areas where a major breakthrough might eventually lead to a Nobel Prize. Effective cancer treatments, new sources of energy or efficient storage of renewable energy all come to mind. In practice, however, the path to such discoveries is rarely straightforward. Take organocatalysis as an example. David MacMillan and Benjamin List received the Nobel Prize for their work in this area, yet intensive research on organocatalysis had already been taking place in industry during the 1970s.

The Nobel Prize remains a dream for many scientists, but receiving it requires not only outstanding results. It also requires luck and being in the right place at the right time.

From your perspective, what is the hottest area of organic chemistry today?

Data-driven chemistry is certainly one of the fastest-growing fields. At the same time, Nobel Prizes have recently been awarded for work connected with computation and artificial intelligence, so it is difficult to imagine a similar area being recognised for a third consecutive year.

It is also difficult to identify a single dominant field. Since the pandemic, RNA chemistry has been developing exceptionally rapidly, particularly research into RNA modifications. That remains one of the most active areas today.

Another important direction is the search for new dyes and functional materials for applications in energy devices. Photochemistry is also attracting enormous attention. When David MacMillan received the Nobel Prize, he was actually one of my own predictions, though not because of organocatalysis. I thought his contribution to photochemistry might eventually be recognised.

Will photochemistry produce another Nobel Prize? It is difficult to say, especially because that would imply a second Nobel Prize for the same scientist.

More broadly, the most exciting developments increasingly occur at the intersection of biology, physics and organic chemistry. These interdisciplinary areas are where many of the most interesting questions now arise. Organic chemistry interacts there with biology and physics, and increasingly with computer science and statistics as well.

From my perspective, both biologists and physical chemists very often need organic chemists. Biological imaging is a good example. Without organic chemistry, it would simply not exist. Physics plays a crucial role, of course, but suitable dyes are equally essential. Natural dyes can sometimes be used, but they often fail to meet all the necessary requirements.

You mentioned data-driven chemistry. To what extent is chemistry today still about laboratories, glassware and chemical reactions, and to what extent is it becoming a discipline of computation and modelling?

That depends on the resources available within a particular country and laboratory. Data-driven chemistry undoubtedly makes many aspects of research easier. Sometimes I find myself wondering what the future role of the organic chemist will be. One can imagine a world in which a computer designs a molecule and plans its synthesis, while highly automated laboratories carry out the process.

The more I think about it, however, the more convinced I become that chemists will remain indispensable, especially when it comes to creating new methodologies. A computer may suggest a reaction, but the question of how to perform it in practice often remains unanswered.

One of the problems is that negative results are rarely published, even though they can be just as important for other researchers as positive ones. For data-driven chemistry, negative results are every bit as valuable as successful experiments because they are still data. The problem is that the system largely ignores them. This is not specifically a Polish issue. It is a broader weakness of modern science.

The machine needs data to work with.

Exactly. If all we have are positive results, our picture of reality becomes distorted. Without negative data, it is impossible to work rigorously or draw reliable conclusions.

Do Polish researchers have recognised areas of special strength? Are there fields where we genuinely belong to the global forefront?

In organic chemistry, certainly. One example is the data-driven chemistry developed by Prof. Bartosz Grzybowski. Another is the chemistry of functional dyes. There is also considerable potential in selected areas of chemical biology and photochemistry.

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prof. Dorota Gryko

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Those are simply examples from my own perspective. I am sure there are others, and I would not want them to be overlooked or underestimated.

Many Nobel Prize-winning discoveries in recent years have emerged from international networks rather than from researchers working within a single institution. How visible are Polish scientists in this global system of knowledge production?
Nobel Prizes are not always the result of direct collaboration. Quite often, they arise from parallel work on similar problems being carried out by different groups at roughly the same time, with each building on previously published results. I do not know whether David MacMillan worked through close partnerships with other groups. From Jennifer Doudna's biography, one gets the impression that she and Emmanuelle Charpentier certainly relied on existing publications, but that does not necessarily mean they were part of a tightly integrated collaboration.

As far as Poland is concerned, I would not say we perform poorly in terms of collaboration. Much depends on the field. There are areas where collaboration is simply unavoidable because no single group can simultaneously specialise in theoretical chemistry, organic chemistry and physical chemistry.

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We also rarely have the funding required to build very large research groups consisting of dozens of people with separate leaders responsible for different aspects of a project. In Japan, such structures exist. One principal investigator oversees several subgroups working on different dimensions of the same problem.

In Poland, at least in my institute, that remains unusual. As a result, we rely heavily on collaboration. And it is worth emphasising that we do so successfully.

Collaboration brings additional benefits as well. It helps build professional networks, improves access to grants and increases visibility. Visibility is crucial in modern science. Publishing results is no longer enough. Researchers need to present their work, attend conferences and become recognised both within the scientific community and beyond it.

Your field also relies heavily on interactions with industry. Does that create a risk of focusing too much on applications at the expense of the kind of risk-taking that often leads to major discoveries?
In my view, both fundamental and applied research are necessary. If we look at the biggest industrial innovations, however, they usually begin with a discovery, and only later does industry become interested. Once research reaches a certain level of maturity, industry naturally starts paying attention. At the same time, it must be said that in Poland there are very few companies interested in supporting fundamental research, at least in my area. The situation is completely different in Switzerland, Germany or the United Kingdom. There, industry actively seeks out collaborations with scientists. In Poland, things do not work that way.

Fundamental research does not generate profits tomorrow or even three years from now. Sometimes it never generates direct profits at all. What it can do is produce enormous benefits a decade later. Space programmes provide a good example. The United States wanted to reach space, but in the process hundreds of smaller discoveries emerged that continue to shape our lives today. Achieving that required enormous investment. I am not sure many companies, or even many governments, would be willing to make a comparable commitment today. That is why most breakthrough discoveries still originate in fundamental research carried out at universities and research institutes.

Can researchers somehow be encouraged to focus more on this kind of work? The first answer that comes to mind is probably money.
If we are talking about fundamental research, then in organic chemistry it already dominates. We do not have a strong pharmaceutical sector that could naturally absorb many practical applications. There are, of course, major pharmaceutical companies developing new technologies, but even there introducing change is difficult.

I recently discussed this issue with people at one such company. I asked why they still relied on palladium rather than cobalt or other metals. The answer was simple: palladium-based technologies have been refined to such a degree that replacing them would require enormous investment. Attempts are being made, but it does not work in such a way that something better in the laboratory immediately finds its way into industrial production.

I also visited a factory where one compound was still being manufactured using a method developed in the 1960s. Only now are efforts being made to modernise it. The reason is obvious. If the existing process works and generates profits, it is difficult to justify an expensive change. Companies make rational decisions about where to invest and where to maintain the status quo.

For that reason, fundamental research deserves stronger support and greater appreciation within the Polish funding system. In my field, industry is unlikely to finance breakthrough discoveries, partly because there are relatively few relevant companies and partly because truly risky projects require resources that most firms are unwilling to commit.

Apart from funding, what most hinders breakthrough research?

One major challenge is attracting young, exceptionally talented people into science. Today, those individuals have many attractive opportunities outside academia. If they choose a scientific career, it is usually because they are genuinely passionate about it. They feel that this is what gives them satisfaction and that nothing else can replace it.

Research is demanding work. It has many advantages. It is fascinating, intellectually rewarding and deeply satisfying. But if you want to operate at the highest level, being a scientist is a seven-day-a-week commitment. It involves sacrifices, long hours and salaries that are not always satisfactory. The problem is not only that salaries are low, although they often are. The problem is also instability.

We had a young researcher at our institute who was successful, internationally recognised, highly regarded and excellent in his field. He knew that when grants were available, his financial situation was good, although his free time was limited. What he could not accept was the lack of long-term stability. Eventually, he left science. For Polish science, that was a loss.

This is difficult not only for individuals but also for entire research groups. Funding from the National Science Centre is becoming more restricted. Once it was possible to hold three grants simultaneously, now two, and perhaps soon only one. What happens then? For the strongest groups, which have already demonstrated that they operate at a world-class level, such limitations represent a waste of scientific potential.

We cannot allow leaders of outstanding research groups to worry constantly about whether they will have twenty people in their team today, five tomorrow and twenty again a few years later, all while losing expertise along the way. Without financial stability, it is difficult to talk seriously about breakthrough discoveries.

For younger researchers, the situation is even harder to accept. They increasingly ask themselves whether it is worth it, especially when they know they can submit a single application to a company and receive stable employment and a decent salary.

A second issue is intellectual independence. Every grant application requires something new and original. In practice, this means that every four years researchers are forced to jump to a different topic. We try to ensure that these topics evolve naturally and remain connected, but they still have to change.

Then publications become necessary because otherwise the grant is difficult to justify. If someone holds three or four grants, each one must formally concern a different topic, otherwise questions arise about double funding. As a result, only a relatively small number of people can continue working on any given problem.

If you could change one element of the Polish science system, what would have the greatest impact on the country's chances of producing world-class research?

Funding remains fundamental. One only has to look at which countries dominate science and where most Nobel laureates come from. The relationship is unmistakable. When it comes to exceptionally gifted individuals, Poland has nothing to be ashamed of. I meet such children through organisations that support young talent. They are remarkable.

Recently, a young girl came to see me and said that she wanted to study chemistry. I told her that was wonderful. She replied: "My parents won't allow it. I have to study medicine because that offers a stable and secure career." That exchange perfectly illustrates how scientists are viewed today.

A scientific career is not widely seen as stable, attractive or secure. And please do not tell me that the solution is simply more science communication. Our calendars are already overflowing.

Whenever I hear that researchers should also be evaluated on outreach activities, I have one question: will we receive funding to hire people who can do that work? Because there is simply no more time available if we are simultaneously expected to conduct research at the highest international level.

Could a Polish scientist realistically be considered a serious Nobel contender within the next decade?

In organic chemistry, the name that comes up most often is Krzysztof Matyjaszewski. His name regularly appears in informal discussions, although he works in the United States. That alone shows that the scientific potential exists. As for the next decade, ten years is a long horizon and predictions are difficult. If the question is whether a Nobel Prize-winning discovery will emerge entirely from research conducted in Poland, I have my doubts. The current international situation, including wars and geopolitical tensions, does not favour stable long-term investment in research.

I also share the view held by many scientists that Poland places too much emphasis on applied research. Perhaps we should think more seriously about creating conditions that allow researchers to focus on fundamental science, the kind of work that ultimately generates genuine technological innovation, even if not immediately. That, after all, is where most Nobel Prizes begin. ■