The Game of Chemical Research

Professor Robert Madsen is internationally recognized in the fields of metal catalyzed organic synthesis. To him, top level science is about far more than coming up with clever ideas. The really difficult part is manoeuvring according to the changing winds of the funding regime, while constantly positioning yourself in new fields which are unnoticed by your competitors.

To Professor Robert Madsen, science is much like playing top level chess: Even when you achieve excellent results and feel on top of your game, you still need to force yourself into exploring new ideas. If you fail to do so, your competitors will soon be on to you.

"Similarly, if I have picked a research topic too late, I will find that just as my PhD student is about to publish his first results, an article covering the same content will be published by another group," Robert Madsen illustrates.

The Danish scientist is internationally known for a series of novel processes in metal catalyzed organic synthesis. An example is the direct ruthenium-catalyzed synthesis of amides from alcohols and amines. Amides are important precursors in organic chemistry. For instance, amides can take part in peptide synthesis – perhaps the most important process in the pharmaceutical industry – or they can be the starting point for widely used synthetic materials such as nylon.

Chance favours the prepared mind

The first results on this new synthesis path were published by Robert Madsen and co-workers in 2008:

"We encountered this idea pretty much by chance. But as Louis Pasteur (French chemist and microbiologist, 1822-1895, ed.) once said: "Chance favours the prepared mind". To me our finding emphasizes that you need to always set up your experiment in a way that allows for unexpected findings. Many groups have a tendency to work too narrowly, only setting up experiments that are practically guaranteed to yield the expected results."

Since the first publication many more have followed from various groups, involving both ruthenium and other catalysts, resulting in improved pathways for amide synthesis.

"We entered metal catalyzed amide synthesis at a truly fortunate moment, when the entire field was just beginning to take off. And I am convinced that the possibilities are far from exhausted. Many new discoveries are waiting to be made," Robert Madsen notes, while returning to the image of the chess player: "Still, this is not the path I am planning to continue. Right now I am in a period of transition considering what should be our next focus."

A game you need to play

Should you, naïvely, ask a top chess player what his preparations are for an upcoming tournament, his answer – if any – would surely be a bluff. Similarly, Robert Madsen is not ready to disclose his specific thoughts. However, on a general level he remarks:

"Internationally, few groups are on a scale in terms of manpower and financial resources that allow them to operate only according to their own preferences. And my group is certainly not one of those. This means that I will need to pick a focus which is different from what large foreign groups are known to pursue."

"Further, the funding system has a built-in tendency to always favour a new idea over the continuation of an existing idea. This may not be ideal from the scientist's point of view, as often ideas which received initial funding a few years back can be really ripe for yielding results today. However, you need to understand that game, and play it well, because that is just how things are."

Challenged with the suggestion of trying to camouflage the existing idea as a new one by adding a label which fits some current trend, he replies:

"Many scientists do just that, but in my view that is not a viable strategy. Firstly, the funding bodies know that game very well and will most often look through you. Secondly, it is actually very rewarding to constantly be involved in the early phases of scientific developments. So our strategy is to position ourselves just there."

Everything gets published!

Obviously, constantly positioning oneself in the arenas of the upcoming breakthroughs is easier said than done.

"One implication is that every time we start on a new topic, we already need to consider how to exit it. If we maintain the individual efforts for too long, we will lose the dynamism we aim for. Still, I would like to emphasize that we are never in such a hurry to exit, that we just move on without publishing our findings. It is a core value for us to always publish our work."

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Professor Robert Madsen, DTU Chemistry

To some people having to exit a relatively new field after, say, just four years when it is perhaps blooming internationally, may sound strange. But not to Robert Madsen:

"Well, actually the very first steps are the most interesting to take. If your ideas are good, you can be sure that others will continue in your footsteps and bring them to life. This is often a very labour-intensive process for which we wouldn't have the resources anyway. I have grown to accept the fact that if nobody wants to take an excellent idea of mine further, it probably wasn't that great after all!"

Projects differ, the core is constant

Robert Madsen admits that this strategy may appear to result in inconsistency:

"Maintaining the overall consistency of our work is a constant struggle. However, this is possible because all our various projects have a common core around certain aspects of organic synthesis. At some points metal catalyzed reactions may dominate, at other points it will be the chemistry of carbohydrates, bio-refining, green chemistry, or other topics. But at the end of the day, the individual projects all contribute to the same goal."