

## Are We Interested Enough In Our Research?



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### Introduction

In what feels like no time at all, two years have passed since the dawn of the 21<sup>st</sup> century. It seems only the other day that scientists in the various fields were declaring the new century as the century of the @, or whatever their specialization happened to be, but there can be almost no-one who would take issue with the idea that the new century, which has begun with the decoding of the human genome, is set to be the century of a new approach to the biosciences: even advanced technologies which appear at first to be unconnected to the latter, such as nanotechnology, information technology, and robotic engineering, now seem to be evolving under a constant awareness of the influence of the biosciences.

At the risk of a little self-publicity, I would now like to turn to our research laboratory, which came into being with the new century.

Our researchers, that is myself and my students, are attempting to create original research in the field of the biosciences, with their wide-ranging connections to other disciplines, through two opposing principles: (1) creation of a new and flexible chemistry contributing to the progress of the biosciences themselves and (2) establishment of a chemistry based on functional materials and molecular materials using principles deduced from the biosciences. Although it may sound presumptuous coming from such a

novice, in the present article, for what it is worth, I would like to present the aspirations and approach of our research group as a case study from the field of research and development.

### Undertaking Research: Establishing and Developing a Research Topic

The scientific foundation of our group is in organic chemistry. In working out the laboratory's research topics we pay due respect to this foundation but are occasionally brave enough to depart from it, in the constant aspiration of creating a new branch of chemistry. In setting and developing concrete topics, there are three things we try to keep in mind:

- Do we find it interesting?
- Is there an individual angle and original input in the precise delineation of the topic?
- Is the research topic worth the challenge?

I think that one of the most important aspects of research in academic life is originality. But where does originality come from? An as yet inexperienced researcher such as myself does not have a ready answer to this question, but it seems to me that the things one finds interesting are the best reflection of one's individuality. In other words, I think originality comes from researching the things that one finds truly

interesting, whatever others may think.

Once interests have been followed and a few outline research ideas have suggested themselves, an important point in the process of giving the topic a concrete shape is being able to take an individualistic angle. If the thing one is supposedly interested in has merely been passively suggested by others, it will be hard to find such an angle. On the other hand, when one is competing with the rest of the world in a certain research area, having an original angle to take on it makes it possible to present findings with more conviction, and, more importantly, to pursue one's research with a certain degree of security free of the worry that one might be copying someone.

Finding one's angle will of course depend greatly on the background of the laboratory, and one's own history and priorities as a researcher, but I believe this is precisely the path to take for individual researchers and organizations in order to survive competition from the rest of the world.

When an individual angle has been found, the final decision has to be made as to whether to pursue the topic; only those subjects should be accepted where one can ask oneself if they are worth the challenge and answer yes. There are times when one cannot say yes to a single one of ten different ideas. Given that it is a subject to which one will devote part of one's own life, not to mention several years of one's students' precious youth, choices which will mean being enclosed in a narrow world of self-indulgent research are to be avoided if possible. A good choice is one where success will reverberate widely and will have an element of the unexpected to surprise all who hear. In addition, it is best to choose a subject which gives the sense of being likely to contribute to the scientific world of the future rather than the present. If a topic occurs that can transcend time and place in such a way as to produce a paper that someone will still read with interest in a few decades' time, that is one worth devoting any number of years

to. For the purpose of selecting a topic that will not be swept away together with the short-lived concerns of the day, Kyushu is not a bad place to be. With a little distance between oneself and the center of debate, one is unlikely to be diverted by short-term issues and will be able to focus on the task ahead in a settled frame of mind.

### **Topic Development and Laboratory Management: Research and Education**

Around five years ago, I came up with a research topic in an area of which we had absolutely no previous knowledge. Unknown subject areas are risky because they are more likely to end in failure than not, but that is also precisely why they provide the added thrill of the swings between excitement and disappointment caused by unexpected findings. At the time, there was a research line which had led to reasonable results but whose future course of development was now to some extent obvious, and which therefore had lost its excitement for me; the new topic came out of chance discussions with acquaintances and friends. Since it is not possible to devote great strategic resources to such uncertain and risky topics, I decided to tackle it in a two-man team with a student who had just started his master's course. Sure enough, however, there was a string of unforeseen difficulties and we faced an uphill struggle.

It was last year that this research project, beset with problems, finally showed signs of getting off the ground. The steady but low-profile efforts of the student during his master's course and through into his doctorate course were paid off all of a sudden, or so it seemed, with a high-profile result: the discovery of the world's first and unique low-molecular weight hydrogel. The low-molecular weight hydrogelator which we isolated from a chemical compound library, and which has the properties of a polymer even though it is not one, gels water at very low concentra-

a ) 25



b ) 65



c ) 72



tions. We found some substances which could solidify almost any organic solvent. What was more surprising was that several of them displayed volume-phase transition (a behavior known only in a certain number of specialized polymers), that is, when heated they contracted without melting (see photograph for example). It was also found that they were able to fix enzymes and proteins without impairing their activity, and they are now seen potentially as novel biomaterials.

Without going into the detail of our scientific strategy to date, I think we can say that the reason we were able to persevere for five years was that our individual angle on the subject did not lose its originality during this period. If we had been outpaced by another research group during this period, I think that we would have been forced to change our research topic. After presenting our results we received questions and comments from a wide range of people: How long have you been at this stuff? ... How did you stick with such a difficult project for five years? ... How did the student manage to keep up? ... and

so on. There are a number of possible answers, but I think it was because the student did the following four things:

- took an interest in intermediate processes
- took a forward-looking approach to his efforts and saw setbacks as opportunities to sharpen his abilities
- gave full consideration to unexpected findings instead of ignoring them
- realized that new topics are inevitably isolating

Building on his existing qualities, the student was helped to grow in this approach in large part by the atmosphere emanating naturally from the senior and junior researchers working around him in the laboratory. Alone in his research topic, what sustained him was constructive discussion and comment from those around him. The higher one aims, the easier it is to become disheartened at everyday setbacks on the way to a goal as yet out of sight, but even where setbacks are encountered, strength can be gained from the feeling that one has been through a learning process and come out of a difficulty with sharpened abilities. The feeling of interest was also an essential element in this. This attitude allowed me to freely bring up various approaches and solutions for discussion, and suddenly the student had changed from a pupil to an equal partner in research. Once this process was complete, the student (who had not had good grades) showed a surprising ability to make astute comments and actively offer ideas. Students like this, who, though their suggestions may be naive, show an attitude of interest, undergo a real transformation at some point. Observing this process at close quarters is one of the delights of the academic life.

One of my most important policies in managing the laboratory is to run a student-centered operation. In other words, encouraging the transition from stu-

dent to fellow researcher is the prime consideration. Even in our information-rich society, this is a massive task which demands resources in terms of both time and patience, but I believe it also makes for a stronger laboratory in the end. Of course, it is also something one can do out of interest... In practice this means repeatedly emphasizing the following points to have them understood and internalized:

- realize you are competing with the rest of the world
- you are also competing with yourself
- total disclosure of information inspires responsibility

These are all obvious points, but among students who are used to passive examination-oriented learning styles, there are many who have not taken them on board. Regarding information disclosure, it carries the message that the professor believes in the students as fully capable adults. I want to make the students realize that once they have been given the information, it is their individual responsibility to work out how to make use of it. Since luckily everyone now has electronic mail, information on the curriculum, seminars, defective measuring equipment, papers of interest, and symposia are now all passed down as far as new fourth-year undergraduates. Information does not originate exclusively from the staff side, but is also actively put out by students. Of course I also try to conduct face-to-face sessions as an exchange of information with a fully capable partner. With the aim of transforming as many students as possible into researchers, running the laboratory is like shaking a flask.

### In Place of a Conclusion

I have made bold enough to present my ideas on starting up a young research laboratory. As the

approach to research and development differs between universities and enterprises, there will no doubt be readers with divergent ideas. However, even though the approach to research and development may be different, I am sure that there will still be many common points. An honest commitment to one's research leads to fresh creation and is essential when it comes to achieving a concrete result. It is also definitely the case that the Japanese society of the future will rely on being able to create products that can succeed in the world.

I sometimes think that I would like to plant firmly in Japanese soil a culture of honest commitment to rules of research emphasizing originality in competing with the world and respect for each other's originality. I feel that part of my duty is to educate young human resources who will go out into the world filled with this spirit.

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