

# Hope for *Synthesiology*

— Discussion with Prof. Lester —

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Prof. Lester has outstanding original ideas based on plentiful and profound investigations on the role of the industry and academia in the course of innovation. He emphasizes the importance of interpretive approach where the direction of an issue will be cleared by communication, in addition to the conventional analytical approach where a solution is achieved by setting distinct problems. Here we present a discussion with Prof. Lester on the newly published *Synthesiology* which includes scientific papers based on Type 2 Basic Research.

## *Synthesiology* Editorial Board

Interviewer; Naoto Kobayashi (Senior Executive Editor)

Interview of Prof. R.K. Lester by Kobayashi on March 3rd, 2008 at MIT.

(Kobayashi)

Thank you for joining this discussion on *Synthesiology* today, at Professor Lester's office.

Today we would like to introduce our new journal named, *Synthesiology*, which we launched in January this year. This journal aims to publicize papers not only on analytical basic research, (we call this Type 1 Basic Research), but also on what we call Type 2 Basic Research based mainly on synthesis or integration, and also on product realization research. We are very happy to have this journal and we would like to extend this journal so that it would attract more attention in the world.

My first question is, what do you think of this journal, or what is your impression of this new journal?

(Lester)

I think that it serves a very valuable role. My understanding of the purpose of the journal is that it addresses a type of research that is normally quite difficult to publish for a number of different reasons; one reason is that existing journals often find the motivation of achieving some practical objective not appropriate for them and this is, I think, typically true of academic journals where the objectives have to do with advancing the discipline or advancing the state

of knowledge without any particular reference to practical objectives. So that's one reason why publication of this kind of research is different. Another reason is that often this kind of research is carried out in companies or in other organizations where it is treated as being proprietary. The information may not actually be proprietary, but because all of the other work that goes on in that organization is proprietary, it is just assumed that this too should be proprietary, and so it doesn't typically appear in the public domain.

So there are two reasons why this kind of research is typically not published. As I understand it, the objective of this journal is to provide an opportunity to publish this kind of work, so I think it's a very welcome addition to the field.

(Kobayashi)

Thank you. Have you ever seen any similar journals or papers?

(Lester)

I think that it's possible to identify some publications that perhaps serve a similar purpose but they tend not, I think, to be peer-reviewed. There are a number of research organizations that have a particular practical mission, such as the Gas Research Institute or the Electric Power Research Institute (EPRI) in the United States. These organizations publish journals, and the contributions are directed towards a particular practical objective, but I don't think these are quite



the same as this new journal, *Synthesiology*. One of the reasons is that, as I said, the articles aren't typically peer-reviewed. And in some cases also they tend to be less fundamental, less basic in character. So they may be more oriented toward product realization, or service realization or whatever.

(Kobayashi)

Later we would like to discuss more about peer review or reviewers because this is also very important. But, I'd like to go to the second question. We have made some requirements for the paper. Every paper should have some kind of objective toward society, a scenario to realize the objective, and elements of synthesis—this can be taken from Type 1 Basic Research, and the assessment and the future work. And among these, we also selected two main items: one is the scenario, and the other is the synthesis. To make a practical application, the objective and the scenario are very important. The second is the methodology to realize this scenario. We must take things and make a synthesis, and it is very important how to realize this synthesis. This is up to the author's originality. So we selected these requirements for the papers. What do you think of them?

(Lester)

Let me ask you a question before I answer your question. What exactly does "scenario" mean in this context?

When you ask the authors for a scenario, is it that you are really asking them to show how the development they are presenting can be reduced to practice, how it can be implemented -- how to get from here to there, how to get from where the development currently is to the actual use in practice?

(Kobayashi)

Yes, it is just a scenario and not an actual realization.

(Lester)

But might there be another way to say this? The author is asked to provide a sort of roadmap, to show that there is some pathway that could be followed in order to achieve implementation. Is that it?

(Kobayashi)

Yes. President Yoshikawa says that it needs a logical chain.

(Lester)

So each step must be related to the previous one. Now I understand. So now let's get back to your question. These four things (objective, scenario, element and synthesis) are required of the author. And your question to me is what is my thinking about those, how do I think about those things. That's the question, right? So clearly, these items differentiate articles published here in the new journal from a typical article published in an academic journal. In the

latter case there may be a statement of objective, but there is rarely a statement of relevance to society, and there is almost never a statement about how one is likely to be able to move the development to practice. The elements, I think, probably do appear in other publications, but the focus on integration is different, because, in most cases, academic journals are organized around disciplines and so integration tends not to be part of the tool kit. And then assessment and future work, I think these maybe are somewhat similar to what appears in existing journals. So I think that the distinctive items here are relevance, scenario, synthesis; and it seems to me that those requirements are going to lead to a different kind of publication, practically speaking.

(Kobayashi)

Whether these are accepted among the researchers or not, that is a problem, right?

(Lester)

For some researchers, they will probably welcome these requirements because they are very much motivated by the desire to produce work that is somehow relevant to society. So I think these items will be received differently by different researchers. Some researchers will look at them and say, "I don't think I have anything to say about these issues. But other researchers will say, "I'm very happy that these items are required because these are things that have motivated me and they describe how I do my research. So I think those requirements will be received differently by different scholars or researchers.

(Kobayashi)

One problem is how to review whether the objective, scenario and relevance are good or not. The review should be objective.

(Lester)

You are suggesting that there may not be an objective standard to apply to determine whether a particular statement of objective or a particular statement of relevance or a particular scenario is of high quality or not. Whereas in the case of a more traditional publication, your point is that you don't have to justify the contribution by its relevance or even by what the objective is because the objective speaks for itself in a traditional journal. If the frontier knowledge has been expanded or extended, then that is a sufficient condition to judge that it is a good contribution, whereas in this case, the criteria for judging whether it is a good contribution may be subjective. But in fact I don't think that's quite true. I think that the judgment of whether an objective is good, or whether something really is relevant, or whether a scenario is a good one—I don't think that these judgments can be made only by people who are working in the same technical field. Actually I don't think they can be made by people working in the field

at all. They can only really be made by practitioners. It's no use asking a theoretical chemist whether a contribution is relevant to the development of, let's say, some industrial advance because that theoretical chemist may not know what industrial development matters or not. In some ways there probably is an objective measure of what's good or what isn't good, but it's not a measure or standard that is necessarily going to be known by researchers in the field. A peer in the same discipline or in the same field may not be able to make a judgment about how good these statements are.

(Kobayashi)

If the reviewer is not in the same field, he or she may be able to look at the objective, relevance and scenario in terms of logic. If it is logically not good then, I think, it is not accepted as an article. An important element is a good chain of logic.

(Lester)

I think you can judge the logic without necessarily having deep knowledge of the field. If the scenario is supposed to consist of logical steps, then I think maybe it's possible to judge the strength of the logic without having deep knowledge of the technical field.

(Kobayashi)

Let's look at an example. Take the environment problem which is very important in the 21st century. To reduce carbon dioxide is a very high priority work and most people will agree that this is a big objective. If the steps to achieve this goal are logical, then the scenario is OK.

(Lester)

So that's an example of a scenario. If I understand you correctly, the ultimate objective here is to see an emission reduction, but to get that you have to do something here, and to get this you in turn have to do something there. One of the requirements that the editors have identified is that the author has to lay out these stages in order to get to objective. Is that right?

(Kobayashi)

If this is logically OK, then that can be acceptable. But, of course, we have many alternatives.

(Lester)

So you want to have what we might call "an existence proof". You want to show that it's possible to get to where you want to be logically.

(Kobayashi)

Also the originality of the synthesis of technology is important. In this journal, we have selected six papers and the individual authors synthesize for the realization of some results, I think, with an originality. But if anyone can easily think of the method then it doesn't have much originality.

(Lester)

It has to be non-obvious -- is that the point? It should not be obvious. If the combination or synthesis is obvious, then it's not a good contribution.

(Kobayashi)

Yes. In this paper—"To the Low Cost Production of Highly Functional Optical Elements" by Dr. Nishii(see *Synthesiology*, vol.1 no.1 p24-30)—Dr.Nishii has proposed the use the mold method of glass, lens or optical components. The old method is not sufficient to make good devices. Imprinting to make some structures on the lens is needed, but imprinting on optical devices has been very difficult. Recently some people in their companies have developed a method, so he joined this mold and imprint methods together in order to make very good optical devices like this. This is a good combination of mold method and imprinting. This is a very original combination.

(Lester)

If I understand this example correctly, it's a case in which the author brought a rather conventional method together with a new method and combined the two things. And the contribution that the author made was to identify the new advance in imprinting and see that it could be combined with a traditional method. So that was considered to be a good contribution. I think there is maybe also a higher level of contribution, one that also involves integration or synthesis, in which the author actually proposes a modification to one or more of the items that are being integrated, so that they actually can be integrated. In other words, the author sees an opportunity to integrate two elements or two components but only if one or both of those components are somehow modified, and the author actually proposes the modification prior to the combination. That might be an even more valuable, original contribution.

(Kobayashi)

In discussing with the authors, I have thought of some kind of different way of synthesis. Maybe you are more familiar with this. My idea is that the first type is, in German, "aufheben", a Hegel thesis, to make a new concept with the combination of the different thesis. The second one is a breakthrough type. There is a very important key technology, like this here, with many peripheral knowledges. But this cannot make good on its own and so, in the process, something is combined to, as shown here, here, here...

(Lester)

And these things are known?

(Kobayashi)

No, but they should be modified. The third is more objective or scenario-driven or strategy-driven type. It might be a little bit different from manufacturing things. These have an equal

weight, but make some integration or combination.

(Lester)

So the contribution here is to identify the elements that are needed to achieve the objective, whereas here the contribution is to develop this new important thing.

(Kobayashi)

I thought these six papers in *Synthesiology* are related with these three types of synthesis. This is my selection and I don't know if it is appropriate or not.

(Lester)

I think this is helpful. But perhaps one can say – although maybe you will disagree -- that all of these things could be done in a Type 1 Basic Research setting, but what differentiates Type 2 Basic Research from Type 1 Basic Research is that in Type 2 Basic Research, you actually start at the end. Or maybe you don't start there but at least you have an idea of some practical objective that you want to achieve, and here, and here, and that motivates the synthesis, whereas in Type 1 Basic Research, I would argue -- maybe you will disagree with this -- that even in Type 1 Basic Research, it's possible to have a synthesis but in that case it's not driven by some practical objective in the world. My understanding of the difference between Type 1 and Type 2 is not that you only have integration in Type 2 and you never have integration in Type 1. I think you can have integration in both cases. But the difference between Type 1 and Type 2, in my understanding, is that in Type 2, it's the motivation for the integration that is different. In Type 2, the reason for the integration is to accomplish some practical objective. So you have to move between the practical objective and the opportunity for integration, whereas in Type 1, you are not motivated by the practical objective. You just are motivated by the opportunity to synthesize or the opportunity to integrate. This is just the way I see this. We can identify a number of areas in Type 1 Basic Research where different disciplines come together: for example, biochemistry. There, you have integration or a synthesis of two disciplines. But what's happening is that the frontier of knowledge is being advanced but not because of a practical objective, but

because there are opportunities in the two fields to bring them together. And I think that in the case of Type 2 Basic Research, what's driving the integration is some practical objective. So that's how I see the difference.

(Kobayashi)

I agree with you that the motivation, or driving force or the objectives are different. I think you mentioned something in your book about innovation\*, and you make some description on analysis and interpretation. Is there any relationship or similarity?

(Lester)

What is the relation between Type I and Type II research, on the one hand, and the analytical/interpretive distinction, on the other? I think this is not so obvious but still it's possible to talk about this. So let's say we have Type 1 and Type 2 and I think we understand the difference between these types of Basic Research. It's a little bit difficult because the distinction we developed in our book between analytical and interpretive approaches is a distinction that applies to the development of a new product or a new service rather than to basic research. So in order to address your question I have to translate a distinction that was developed for one context into a very different context. I think that perhaps the best way to do it is to say that, in each case, i.e., both in research and in product development, there are only two situations that can arise. In one situation, the problem is well understood and the task is to solve the problem. Maybe it's a very difficult problem; let's say you have a theorem in mathematics that has never been proved, and it might take ten years or it might take fifty years to prove it. But even though the problem is a very difficult one, it is still a well-defined problem that has to be solved. This example of a theorem in mathematics is, I think, a Type 1 Basic Research problem. But you can have situations in which the problem is well understood in Type 2 situations as well. Let's take one of the examples here. Maybe the problem is to establish a measurement scheme for ranking health risks -- that's the problem. So we have to develop a scheme which we can use to compare different kinds of health risks. That's a practical problem. It's a difficult problem but we can state what the problem is and we can work hard at it and maybe

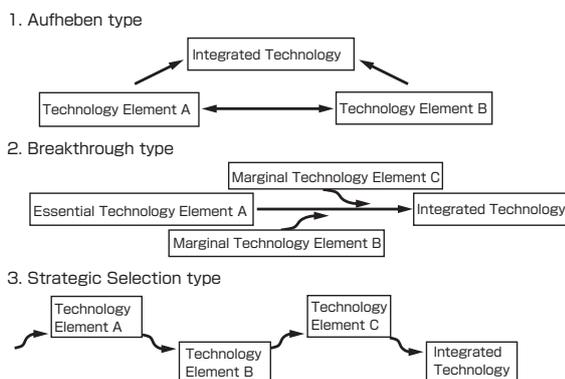


Fig. Different types of synthesis.



Prof. Richard K. Lester

we will solve it. But there's also another kind of setting in both Type 1 and Type 2 research where the problem is not understood -- where we don't exactly know what the problem is. We don't have a problem; we have a 'situation'. Let's imagine that in mathematical research we have a number of branches of mathematics which are addressing a given situation and that maybe they are not consistent with each other. But the different researchers in the different branches of mathematics each see only part of the situation and they don't recognize the inconsistencies in their approach. But when they talk to each other over a period of time, the nature of the problem becomes clear. There is a process that goes on that brings together the different researchers in different fields, in different branches of mathematics. They talk to each other, and over time, they discover that there is actually a problem there. They didn't realize that there was a problem initially. They didn't realize there was a paradox. But as they talk to each other with each one bringing a somewhat different perspective to bear, they discover that there is a problem that needs to be solved. Similarly in Type 2 Research, the situation might be that a company or the scientists or engineers within the company are in discussion with a regulatory body about setting a standard for, let's say, a chemical or something that the company is making, for which the company needs regulatory guidance. So the engineers in the company go to the regulators and say, "we need some guidance here, we need you to tell us what we can do and what we can't do". And the regulator starts to look at the chemical that the company is developing, and he might think, "that chemical is actually quite similar to another chemical that somebody else is developing. And are they similar enough that we should be thinking of them in the same way or in different ways? And a conversation starts, bringing in the other company. And maybe over time, as the companies and the regulator start discussing the situation, they see the possibility of developing a ranking scheme for different chemicals or for ranking the risks from different chemicals. But when they started talking about this, they didn't see that possibility. So this is a very different kind of situation. This second type of situation can apply in both Type 1 and Type 2 Research. Its characteristic feature is that the problem isn't well-defined, and maybe isn't even defined at all at the beginning. And the distinction that the



**Interviewer: Naoto Kobayashi**

book makes between analytical and interpretive is that the interpretive process is what happens when you move from not understanding the problem at all to having a clear picture of what the problem is. And you encounter these situations in both Type 1 and Type 2 Researches. In both cases you want to move from a situation where the problem isn't well-defined to a situation where the problem is understood. That is the interpretive process. And then once the problem is understood, then you use analytical methods to solve it. And what the book argues is that in innovative organizations it is important to have both processes. It's important to have the interpretive process and it's important to know how to manage it, because it involves a very different kind of management from the management of the analytical process. And it's important not to cut the interpretive process short, not to shut it down too quickly. And so to go back to this situation here, I think the distinction that's made here applies to both Type 2 and Type 1 Basic Researches. We would argue that you need to have both of these things going on, in both cases. So that's why, as I said at the beginning, it's not so obvious how this translates. I think that this distinction difference of analytical from interpretive is relevant in both Type 1 and Type 2 Basic Research. And it's also relevant, I would say, in product realization, because there too you need both interpretive and analytical processes.

**(Kobayashi)**

I have a question. You have shown a diagram of Bohr type, Pasteur type and Edison type for the nature of the research. Does this correspond to some process?

**(Lester)**

I think it does. I think it is very closely related to what you are talking about. I think Pasteur is really more Type 2 and Bohr is Type 1.

**(Kobayashi)**

Some people think that in Type 1 the analytical method is more usual than interpretive. Is that so?

**(Lester)**

No, I think both play a role in each case. This is an important point.

**(Kobayashi)**

So even in Type 2, we have analytical method and interpretive method.

**(Lester)**

Yes. But I think the difference is, in the interpretive process in Type 2, you have to bring --we talk about this as being a conversation. The interpretive process is like a conversation. It may not exactly be a conversation, but it's like a conversation. And the difference is that in Type 1 Basic Research, the conversation is between—the question is who

is involved in that conversation. The people who are involved in that conversation in Type I Basic Research are generally within a given discipline or, in some cases like biochemistry, for example, they are in two disciplines. But in the Type 2 case, I think the people who are involved in the conversation, some of them are from scientific disciplines but some of them are from the world of practice. So that's the difference. The difference is who is involved in the conversation.

(Kobayashi)

We would like to consider the reviewers and readers. With Type 1 Basic Research, the readers are within the discipline and most of the readers know where the frontier of knowledge is. In case of Type 2 or in *Synthesiology*, the readers are in many fields, outside the field, of business etc. Also we need different kinds of reviewers. As you said conversation is important.

(Lester)

Also in order to judge the relevance, you need reviewers who can assess the relevance. I think that one of the challenges for the journal is that you have a very broad readership because you have multiple disciplines and you also have multiple application domains. For example, in this first issue, in one case you have health care, in one case you have environmental regulation, in one case you have personal health. So you have multiple application domains, as well as multiple disciplines. So the challenge is how to appeal to readers who might know a lot about personal health care but may know nothing about environmental regulation. And also you've got to bring in people from different disciplines. But that maybe is less important because your researchers are people who actually bring multiple disciplines together. The challenge is that the reader is unlikely to know more than one application domain, and so the question is going to be how is a paper in another application domain going to be for (the reader). Here is health care, and here is environment, to take two examples from the first issue. A reader who knows a lot about health care probably isn't going to know much about the environment and will that reader be interested in articles about the environment? Maybe the thing that would make such a reader interested in an article about the environment would be if the article was really about how to do a certain kind of synthesis. Then it might appeal to readers with knowledge of other domains.

(Kobayashi)

This is what we would like to aim at. Now, the reviewers for this volume are all from inside AIST because people who think about Type 2 Research are very few, but we must extend it to the outer world. Next time we will invite some reviewers from the outside who know about Type 2 Basic Research. In the future, we would like the reviewing process to be done outside AIST like other academic journals.

(Lester)

I think one of the opportunities for this journal, perhaps, is to make it a place for people in companies who are doing Type 2 Basic Research, because there are many people in companies who do Type 2 Basic Research, especially in Japan, perhaps, but also in other countries too.

(Kobayashi)

The final question; even up to now, in private companies, they have many technological reports non-public or made public like in NTT, Fujitsu, Toshiba that are very useful for the engineers. But these are probably not reviewed by peers. This *Synthesiology* aims at the academic. What is the barrier that we must remove?

(Lester)

I think one of the barriers, if you're hoping to attract authors from companies, is going to be a concern about disclosing proprietary work. Another problem or challenge is going to be to bring peer reviewers. I think some of the peer reviews have to be done by practitioners, people who understand the goal. I think that's going to be the key. And some of those people are going to be people in AIST who have a very good understanding of the goal. But if you want to broaden this, maybe you have to bring people from the outside.

(Kobayashi)

Also some of the professors, for example at MIT, or Harvard, or Stanford, know how to solve the real problem and make it in the application field?

(Lester)

Yes, certainly at MIT the culture is one in which people are motivated to work on practical problems. So some academics will have that knowledge.

(Kobayashi)

So the conclusion today is that to make a good journal, especially in Type 2 Basic Research and product realization research, conversation or communication with many fields is important, even with reviewers and with readers. With academic journals, they are also based on conversation among people in many different fields.

(Lester)

Yes. One last point I would make: if this journal succeeds, I think it will make easier the movement of researchers into and out of universities, which I know is an important objective in Japan. If you have a journal that is academic -- a peer-reviewed journal -- but that addresses this Type 2 Basic Research, it might make it easier for researchers in industry or in AIST to move into universities, and back from universities into industry. I think the journal might help to promote migration across that boundary which, I think, is very important in Japan.

(Kobayashi)

You can do that in the United States.

(Lester)

You can do that. Why can we do that? I think partly because it is possible for people in industry to publish in Type 1 Basic Research journals, so there is an opportunity for those people to move back and forth. But in Japan, I don't think it is so common for people in industry to publish in Type 1 Basic Research. It doesn't happen so easily.

(Kobayashi)

Lastly, do you have any future advice for the journal?

(Lester)

I hesitate to offer advice as you have been thinking very hard about this and I don't think I have very much to add. In terms of the strategy, what may be valuable—although I hesitate to say this because it may not be the right thing for you—but I wonder whether a valuable step would be to highlight certain areas of application over others; in other words, to say, “this journal is about Type 2 Basic Research, but we are going to emphasize certain areas of application”. I think that the challenge is to move this beyond AIST. That's where, in the long run, you want to go. So the question is how to do that. You can't go directly from the current situation where the reviewing is conducted entirely within AIST to “involving everybody”. I mean, in a sense, it's like President Yoshikawa's point about describing a logical progression or scenario. The goal here is to have a general journal that is read very widely. But to get there you will have to move in stages. And the question is how to think about these stages. One way to think about them would be to say that the first step you are going to take when you move beyond

AIST is to focus on a particular domain of applications—maybe the environment, or health or energy. Then draw in a readership and a reviewer-ship around those areas and then maybe the next step is to increase the number of application areas. I think the same scenario method you are calling for in the preparation of articles may also be used to plan the development of the journal itself. In some sense, the journal is also a ‘product’, or the practical realization of a research activity.

(Kobayashi)

Thank you. Today's discussion is very fruitful and helpful to us. And in thinking of the future of *Synthesiology*, it is valuable. We would like to express our sincere gratitude for joining our discussion.

(Lester)

My pleasure. I'd like to congratulate you on the publication of your first issue.

(\*)R.K. Lester and M. J. Piore, “Innovation”; Harvard University Press 2004.

#### **Profile of Prof. Richard K. Lester**

Director of the Industrial Performance Center (IPC) and a professor of nuclear science and engineering at the Massachusetts Institute of Technology (MIT). Born in 1954 and graduated from Imperial College (UK). His research focuses on industrial innovation and the management of technology. He has led several major studies of national and regional productivity, competitiveness and innovation performance commissioned by governments and industrial groups around the world.