Round Table Talk by Reviewers of the New Journal

Reviewing papers in the new style

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Reviewing the *Synthesiology* papers was exciting but hard task for the reviewers because of the new style unseen in the other scientific journals. A round table talk with the reviewers of Volume 1 No. 1 and 2 issues was held to provide frank comments on diverse issues including their impressions on the new journal, the new role of reviewers, the originality of the papers, and the practicality of the requirements to the authors.

***Synthesiology* Editorial Board**

(Kobayashi)
We have asked you to review the papers for *Synthesiology*, the journal for Type 2 Basic Research featuring papers in a new style, and I believe reviewing the papers for this journal has been quite different compared to reviewing Type 1 Basic Research papers. What are the points that you felt most strongly about in doing the reviews?

Impression of reviewing for *Synthesiology*

(Yumoto)
These were original papers for which originality was required, and I found it was very difficult to emphasize originality. It must have been much more difficult for the authors themselves, but as a reviewer, it was very difficult devising ways to bring out originality.

Another point is, I have heard direclty from authors that there were limitations due to patent and joint research conditions with companies, and many things couldn't be written up in the paper. Perhaps we should wait for the full story when the authors are formally permitted to write, but I also felt it was useful to read about research in progress. However, it is difficult to determine originality if the details of technology remain undisclosed.

(Igarashi)
If originality is sought as in Type 1 Basic Research, this journal may be insufficient. The President had said, "There are many things that cannot be written in the journals for Type 1 Basic Research when conducting research along a set scenario. One of the important functions of this journal is to describe that process fully." I felt this was not presented sufficiently. I think the reviewers must emphasize this point with the authors. Of course, the range of what can be described may be limited due to nondisclosure concerns, but I do feel it is necessary to stress this point.

(Ono)
I think the reviewers share the standpoints of both authors and readers. Compared to Dr. Yumoto and Dr. Igarashi, I have a rather optimistic impression. The authors seem to have a driving passion to write Type 2 Basic Research papers. They tried to show their passion here, and I felt they attained it. Talking with the authors, some mentioned, "For the first time I was able to write things that I couldn't in the other journals." This made me believe that authors will be able to show their vision. I may be too optimistic, but I thought we succeeded in showcasing their insights.

I reviewed two papers that did not relate to my own research discipline. In fact, it was my first experience that I read original research papers in disciplines other than my own. In my duty as a reviewer, I more or less forced my way through the manuscripts, but I was surprised that as a reader I could read it more smoothly than I though. I might be so optimistic, but my first impression is that the journal was a success, and I have high hopes for it.

(Mochimaru)

The papers submitted here are quite different in style compared to the ones in conventional scientific journals. I am satisfied with the requirements, and the papers I have reviewed were undertaken with the understanding that diverse styles are acceptable. On the other hand, it's a different story when it comes to whether the readers will accept this diversity. How do we make the readers, who are accustomed to old-style papers, understand the points the authors are trying to make in new-style papers. We must bring around a change in the consciousness and attitudes of readers. We must convince the readers that the new-style papers are also scientific papers, and point out where the originality of the papers lies along the way. To be honest, I think this balance was hard to maintain. Communicating what the authors were trying to say was the hard part.

(Kobayashi)
We placed the section for discussion with reviewers after the paper. What did you think about that?

(Mochimaru)
We received several comments from readers that "the discussion with reviewers was interesting," and I believe it was effective in delivering the message that *Synthesiology* is created by a collaboration between the reviewers, readers, and authors. I sincerely think it is a good plan because we can clearly show the process of synthesizing a scientific paper to the readers.

On the other hand, the reviewers must put in extra effort to read the papers because their comments will be published. If we have two reviewers and one is caught making off-the-mark comments, we must really do our jobs knowing that our names will be published out in open.

(Kobayashi)
In a peer review, the reviewers are usually anonymous, but we disclose our names, and I do feel that increases the weight of our responsibility.

(Ono)
We have received several responses from readers stating that "the discussions with reviewers were most interesting," though I'd prefer if they'd say the papers themselves were interesting. It seems that readers sometimes read "Discussion with Reviewers" before anything else, and if they find it interesting, then they read the main article.


Dr. Kazuo Igarashi

(Igarashi)
I've also heard the comment: "Although the content is difficult and the research is not so-called Type 1 Basic Research, it was very interesting that when a reviewer asked how the author will make changes to parts of a manuscript that were ambiguous, the author responded sincerely in the discussion session."

(Kobayashi)
Perhaps the discussions with reviewers may serve as a bridge between authors and readers.

Shift in role of reviewers: from reviewer to coauthor

(Akamatsu)
Talking about first impressions, I do think the discussion with reviewers is important. Maybe I said too much, but I did say a lot. I made plenty of suggestions, almost to the point of being a coauthor. First, I read a manuscript from the perspective of a reader, and think about what the readers should get out from the article. When I feel that "the paper doesn't say enough," I comment on everything from the structure of the paper to its logic, which is quite different from reviews done in ordinary journals.

The primary objective I have in doing my review is whether "the author can describe a good scenario." Specifically, what was the issue or reason for starting the research, and what was the focus. If there is clear description of what the issues are and what research has to be done in a certain field, I think people from other fields will be able to see that, "The people in this field are thinking this way when they do their research."

(Kobayashi)
I reviewed one paper on optical devices and another on standards in issue no.1, and a paper on materials in issue no.2. Since they were all physics related, the individual elements were clear, and I could readily see the scenario and synthesis of how things were combined to achieve the goal. However, a common problem was since the authors were in the process of product realization, not everything could be written due to nondisclosure agreement with companies, and I found that frustrating at times and I'm sure the authors felt this as well.

Another point is the originality as mentioned by Dr. Yumoto. I saw originality because the papers I reviewed were written by people in similar fields to my specialty. I do feel that people of other specialties may find it difficult to judge the originality of synthesis because the component technology may not be original.

The reviewers of conventional journals decide whether the knowledge presented in an article is truly innovative from the standpoint of their respective disciplines. However, in *Synthesiology* we look at how knowledge can be used, and I felt in this context the role of the reviewers is quite different.

Next, do you think the points that the authors wanted to appeal to in their papers were presented appropriately? As readers, were they useful or interesting and what do you expect in the future?

Useful papers

(Igarashi)
I think there are three points that make the readers feel that a "paper was useful." One is the reader can see a useful scenario as required by the journal. Second, the description is useful in terms of technological content because points that were not written up in Type 1 Basic Research were presented. And third, it is useful to show that AIST is engaging in the new movement.

I reviewed two papers, and I thought the scenarios were developed in a style which would be useful to readers.

(Mochimaru)
I really had no idea how *Synthesiology* was advertised. Suddenly, several partners in joint research said they wanted to read it, so I gave them copies of the first issue. I think they learned somewhere that "AIST put out a new journal." People commented "I read your paper" or "it's pretty interesting." They were not eyeglass people.

As Dr. Akamatsu said, the reviewers are like representative of readers who give advice on how to communicate the message of the paper to a wide-range of people. Although I don't necessarily mean that a paper has to be popular among readers, I think whether the intent of an author is communicated effectively must be checked at some point. As a proposal, we can set up a "designated reader," who doesn't necessarily have to be fixed, and get comments from that person. It is necessary to check whether the intent of the paper is being effectively communicated from time to time.

(Akamatsu)
The "scenario," as mentioned by Dr. Igarashi, is a major element that allows people of other fields to understand papers content more deeply. Another point is whether it is useful to researchers in the field. I expect that when knowledge, which researchers of Type 2 Basic Research maintain as implicit knowledge or things that they figure out among themselves and believe are important in conducting their research, are organized in a thesis form to discuss "why this was done," knowledge presented in such a style may eventually become a part of formal knowledge. By engaging in discussions relating to these issues, researchers may come to realize flaws in their logic. People in the same field can understand the thinking process of the authors involved or how the target was set in progressing to the next step.

We heard only positive comments from the authors, but I think there should be more "awareness" about problems in their own research, such as this was no good or this was not good enough when writing up their research in a paper.

(Ono)
I think authors are aware that. Type 1 Basic Research can be written up as a 100% complete story, but they cannot present a 100% complete paper in this journal for Type 2 Basic Research. The story here is "my first target was this, but we've reached only this level," and such an incomplete paper will not be accepted by the existing journals. I do feel that some authors have come to accept the fact that a paper could be incomplete, while others were unable to write a paper on their results because they could not accept the concept of writing a paper in an incomplete manner.

(Kobayashi)
I did get a similar impression. I said to a certain researcher, "Don't you think you can write a scenario about how your research can help lead to a sustainable society?" but the person responded that can't be written so he'll write only up to a certain point. A scenario is "a chain of hypothesis," as President Yoshikawa said, but I think there's a nature hesitation on the part of a researcher in how much hypothesizing one can do. The readers, on the other hand, want the authors to present a birds-eye view for the 21st century, so I hope the authors will go ahead and do it.

(Ono)
I think such scenarios are discussed hotly within a research group. A research group can't function without discussion of such scenarios. But the results of such discussions remain within the group and don't make it outside. I think this journal is trying to bring such discussions into broad daylight. The reason they never came out before was partly because there was no path for them to do so, but the main reason is there was fear the results may be stolen when disclosed. Such scenarios are an asset of researchers and

Dr. Motoyuki Akamatsu

research groups. This time, the authors spilled out the scenarios in their head for us, and although this is good for society, I was a little bit worried that it might disadvantage the authors who were sincere enough to do it.

(Mochimaru)
Dr. Ono's concern and Dr. Akamatsu's comments are very true to an author like me. I do become aware of things when knowledge is formalized within myself by writing it out. When I write, I can qualify "whether the method used here was optimal or not."

How much can a specific case be generalized? This is something that cannot be stated with confidence. I write a bit more only when the reviewer comments, "How about it?" Therefore in my paper, much is written in the section describing the discussion with the reviewers. It was important to get advice from the reviewer. In that sense, although in a sense it is spilling one's brains out, I feel it is a useful exercise for the researcher to write out their thoughts in an organized manner.

(Yumoto)
Speaking about scenarios, I think biotechnology is slightly different. Rather than a set scenario, it develops from a certain breakthrough. To find the objective of their research, rather than shooting straight at the center of the target, the biotech researchers try for a breakthrough by at first elaborating the periphery of the target, in the so-called a shotgun style. Since it is an emerging field, such individuals may not admit having a specific scenario in mind when they are asked, "Did you start out with that scenario?"

In conventional journals, we wrote as if we aimed at the result from the very beginning, but if we make the future scenario too clear, we can't apply for patent, and that is very difficult.

(Akamatsu)
I think that leads to the discussion of originality. The originality of Type 1 Basic Research is "novelty of individual elements," and it requires a third party to appreciate that originality. From the perspective of Full Research, I think diffusion of science and technology to society is delayed when it is carried out by a third party. That is, there may be problems with the paradigm of evaluating the value of researchers according to the novelty of the individual elements of their research. Until now, the thing that has been most secretly and jealously guarded from others has actually been the process of creating the "thing," and I think hiding this process weakens the driving force for the product to be used in the society.


Dr. Noboru Yumoto

In other words, knowledge must be used by others. If the synthesis process is disclosed to all, the successor can go on to the next step. I think that is one of the values of knowledge. Therefore, I think the steps of synthesis should be considered as originality.

Were the requirements described logically?

(Kobayashi)
Dr. Akamatsu's comment just now refer to the specific requirements for a paper. Were the "establishment of research objective," "presentation of scenario," "selection of elemental technology," "combination of elemental technology," and "evaluation" described logically? Please comment including the practical aspects.

(Ono)
They depended pretty much on the authors. Some authors described every elemental technologies evenly and explained what they did with them, which is what I expected in the beginning, while others, particularly biotechnology researchers, described the main-and-sub relation where one elemental technology was overwhelmingly important so the next steps could be made only by making a breakthrough in that area. And they described how they added a sub elemental technology later to create the "product."

(Igarashi)
Many people looked at the scenario as a backtrack rather than a forecast, and when authors conduct their research, they do not necessarily have the scenario as described in the paper in their head. They ran into dead ends, took detours, and then they picked up the track and marched forward. If we track their routes, it is irregular, but looking at the flow from a larger perspective, I feel the authors went in the direction as described in the scenario.

On the appeal point of the journal, the content of each paper is fine, but seen from the viewpoint of the reviewer, I feel the author should set the main theme. Reviewers may have diverging opinions, and I don't know how to handle this aspect in the future. Perhaps the committee chairman can ultimately dictate what should be.

(Mochimaru)
Since there are two reviewers for each paper, the authors are confused when the reviewers' comments are in conflict.

"What should I do?" I think it will be good to have a principal reviewer who will make the final decision while listening to the comments of other reviewers.

Normally, in submitting papers to a journal, the author thinks, "A reviewer is someone with the power of life and death over my research paper." Therefore, the common practice is to succumb to whatever the reviewer says. The author feels that his/her prose becomes a thesis only if he/she agrees with whatever the reviewer says. This must be changed gradually.

(Kobayashi)
Currently, the reviewers and the authors are acquainted with each other and can discuss things between them, but in the future, we will be receiving submissions from outside AIST and we must review papers of people we do not know. Also when we ask outside people to do the review, we need to have some sort of ground rules. As Dr. Mochimaru suggests, perhaps it is better to have a principal reviewer who can integrate the comments of the reviewers.

(Akamatsu)
In the requirements, I think the most difficult point is to determine "what is the result?" When we hear "result," we tend to write up the result as in Type 1 Basic Research, but it is doubtful whether such a result is the type of result required by *Synthesiology*. We must consider this. Although we still do not clearly, perhaps they must express their results in terms of *Synthesiology*.

(Mochimaru)
There is the matter of "significance of a scientific journal," and it is accumulating *Synthesiology* through the vehicle of journal. Dr. Akamatsu wrote, "Each paper is an archive of case study of *Synthesiology*, and *Synthesiology* is created from that archive." However, from the standpoint of the editor of the journal, it does not happen automatically, and we must make an active effort.

(Ono)
Synthesiological methods are actually present, but I feel we are still far from generalization of the methods. I want to focus on the level of "product" that goes out into society and how the "product" is useful to people.

(Akamatsu)
I think it is not easy to draw the line clearly between Type 1 and Type 2 research. One of the important points is to find value for people who progress from Type 1 to Type 2 Basic Research. We must build a path to encourage people engaging in Type 1 to move on to Type 2 Basic Research.

I think we should present how one can move from Type 1 to Type 2 Basic Research by writing papers in *Synthesiology*.

(Ono)
Perhaps that's what is expected by us. I agree with this totally. I hope the results of Type 2 can be clearly presented, even if they are minor

(Akamatsu)
When we ask, "Please write upon the result of your Type 2 Basic Research," will the author be able to describe it? As mentioned earlier, it is matter of "what is a result?" The result of Type 1 Basic Research is a discovery or invention with great impact. If a discovery has great impact, the underlying knowledge must be useful to other researchers.

When the result of Type 2 Basic Research is explained in the form of a specific product, it may be at the point a product exerts influence on people in society. But if we consider the problem from the perspective as to whether it can have a great impact on other people doing Type 2 Basic Research, there may be some doubt. Even if it is extra work for the the authors, we should ask, "What are your result in terms of *Synthesiology*?".

(Mochimaru)
I think that the journal should ask the authors, and authors and reviewers to engage in a discussion with an open-ended question. We do have a place for an open review, so we can take a moment to think over abstract concepts.

Relationship of papers and companies

(Kobayashi)
Next, let's move to the subject of the relationship with companies when writing the paper. The authors mentioned that there were many things that couldn't be included in their papers due to the patents and know-how nondisclosure agreements. Regarding how this should be dealt with in the future, we can't say much. When the author says, "I can't write about it," I don't know what to do as a reviewer. This problem also arises in hearing and evaluation, and when a researcher says, "I can't talk about it," then we'll have to respond, "Well, then we can't evaluate it."

(Igarashi)
Companies are very sensitive about the disclosure of know-how. I think that this will be a frequent source of questions.

Dr. Naoto Kobayashi

(Yumoto)
If we receive more submissions, I think that part will soften a bit. If papers are submitted and the authors say they want their articles published, we can say, "Can you describe a bit more?" If the author says, "No, I can't do that," then we can say, "We can't publish it!"

(Ono)
Since I don't have that much experience doing joint research with companies, this may be a wild-pitch comment, but there were instances when I was reviewing papers when I felt why they couldn't talk about things. Aren't you overstretching the scope of joint research? If you are relinquishing the originality of your research into joint research and are being limited by nondisclosure agreements, isn't that diminishing yourself as a researcher? I know this may sound unorthodox, but can't anything be done about it?

(Mochimaru)
I've done lots of joint research, but basically I agree with Dr. Ono. My boss has the same opinion. Basically we are public servants at a national institution, and we do not do research for the company. Even if we do joint research with companies, the findings and methods that we obtain in our activities will be eventually publicized. This is the basic premise.

In joint research, though I don't know what others do, I feel there is no precise agreement with the partner about the final academic reporting.

(Igarashi)
In practice, a detailed agreement is made when a contracdt is signed. There are conditions to which we must agree, like waiting a year and half due to patent matters. I understand Dr. Ono's point, but we must make these rules clear, and the researchers must set up the scenario taking the time gap into consideration.

Synthesiology as originality and learning

(Kobayashi)
Now, on the originality of papers, as Dr. Yumoto mentioned earlier, and whether we are heading toward "synthesis as an academic study," which is the heart of the journal. What was the status after publishing issue No. 1? How was it for the review of issue No.2? And what do you think is the general direction?

(Yumoto)
In issue No.1, after more than three exchanges of comments and through revisions by Dr. Ono and Dr. Kobayashi, I realized that certain things that seemed not original to someone in that specialty may be significant as a *Synthesiology* in people of other fields.

Since I didn't have a complete understanding of *Synthesiology*, I initially expected content similar to that of Type 1- Basic- Research- like papers. I feel there is a long way before I can fully understand what *Synthesiology* is and whether the papers fulfill the appropriate requirements. I do think I am making progress.

(Igarashi)
I reviewed two papers. One of the papers was closely related to my specialty, so I read it without considering the synthesiological significance. Now when I look back, I think perhaps I should have considered the synthesiological aspects more. In the other papers, I had strong feeling as a reviewer that the contents should be revised to match the purpose of the journal or to increase its appeal, so I commented frequently, "Why don't you change the paper to follow this direction."

I do feel that authors and readers as well as reviewers will have more awareness for synthesiological concepts as more issues are published.

(Ono)
I think it is not very easy to see where originality lies in the papers. Putting it very bluntly, it would be enough if the authors say, "It was fun writing," and the readers say, "It was fun reading it." There was another point that comes up in talking the authors. To question "Could this paper have been written by another person if he had the same information?" all the authors stated with confidence, "No, this paper could be written only by myself." Perhaps that is the author's originality at the root level. Something that can be written by this author only, that's also fun to write and fun to read.

Dr. Mochimaru Masaaki

Dr. Akira Ono

What else do you need? Sorry for being so simple.

Perhaps that's too wild statement. Then, what's the difference between the papers in the new style and the review papers? Several authors mentioned the review papers aren't expansive enough to write about what was described here in this journal. "If I was writing , I wouldn't put it this way." Also, major companies publish "technical reports" periodically, but these "technical reports" talk about the features of new products in words of science and technology, but they're nothing more. Normally they don't offer the thinking behind the product, failures, or alternatives. Since those are the source of power of a company, they can't disclose them. *Synthesiology* spills that part into open.

There's concern that by spilling it out, won't the researchers lose the source of their power, but I think a community where people can spill out their guts is a good community. When science began to bloom in the 17th century and academic societies were formed, it started with sharing Type 1 Basic Research results among all members of the society rather than keeping it as personal secret. Instead of thinking that the researcher lost something by disclosing their know-how and scenario, researchers were given the honor of exercising influence over society. They praise the researcher's originality. If we can create such a community I think science and technology will take a bigger step forward.

Scientists and researchers in companies should not be limited totally within the framework of the company, and I believe they do desire to contribute to the progress of science and technology. I think it may not be very easy for company researchers to write for *Synthesiology*, but I do hope that they will eventually overcome such limitations.

(Mochimaru)

The basic purpose of a *Synthesiology* paper is to explain the "learning" process such as how the researcher synthesized the story or why they made certain choices. I had difficulty in doing that, but I think I was able to produce a paper that smelled of the author. I think this should be continued with conscious effort. As we archive synthesiological methods, it is very important to describe how the choices were made and how things were synthesized.

I have read other people's papers as well. Although they were not presented at an abstract level, they are original, and I think they are fairly successful for the start of a new journal.

(Akamatsu)

In history, the originality of Type 1 Basic Research was to make a major discovery or an invention, and by publishing them as papers, the patron (or employer) thought, "I think this researcher will find more interesting things."

On the other hand, in seeking originality in building a pathway for research so the result will be useful in society, it is necessary to both show that the author can synthesize things in these ways and is capable of making his/her research result useful in those ways. In *Synthesiology*, there are descriptions of awareness and selection of issues, and I think originality lies in such awareness and selection.

I share Dr. Mochimaru's opinion on synthesiological methods, and I think it will take more time to become abstracted and generalized. But I also think that if it is unattended, it will be a mere pile of knowledge, so we must make an effort to shape them into science.

(Kobayashi)

Although accumulating the knowledge of *Synthesiology*, creating archives, and then analyzing and abstracting them might be the work of an editorial board or AIST, I do believe it is the work of synthesiologists, and I wish we can do it for them. In that sense, I think we have made our first steps towards this goal. In the future, I hope we will have many submissions from industry and overseas as well as within AIST, and people will gradually understand our way of thinking. Thank you very much for today.

Participants of round table talk: Motoyuki Akamatsu, Kazuo Igarashi, Akira Ono, Naoto Kobayashi, Masaaki Mochimaru, Noboru Yumoto.

(February 22, 2008)