Systems and synthesiology

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Editor-in-Chief Ono and Senior Editor Akamatsu of *Synthesiology* visited Mr. Hiroshi Kuwahara, Senior Corporate Adviser of Hitachi-Maxell, Ltd., former Vice-Chairman of Hitachi, Ltd., and former member of the Council for Science and Technology Policy. Mr. Kuwahara is also a member of the Systems Science and Technology Committee headed by Senior Fellow Hidenori Kimura, Japan Science and Technology Agency (JST). Mr. Kuwahara has spearheaded several proposals at the Committee based on his abundant experience in various systems developments. We held a roundtable discussion under the keywords, systems and synthesiology.

Synthesiology Editorial Board

Participants of the roundtable discussion

Hiroshi Kuwahara	Senior Adviser Emeritus, Hitachi, Ltd.; Senior Corporate Adviser, Hitachi-Maxell, Ltd.
Akira Ono	Senior Vice-President, AIST (Editor-in-Chief, *Synthesiology*)
Motoyuki Akamatsu	AIST (Senior Editor, *Synthesiology*)

Akamatsu

AIST promotes the *Type 2 Basic Research*, because we feel that the research results cannot be utilized fully in society with analytical research alone, and we need to establish a methodology for the science of manufacturing "things" or the synthetic research.

Mr. Kuwahara emphasizes the importance of systems building. Manufacturing "things" leads to enhancement of the system, and in terms of how to build without falling into reductionism, I feel that the aim of systems is similar to the aim of synthesiology, although the terminologies may be slightly different. This is why we wanted to hear from Mr. Kuwahara.

Based on your experiences, Dr. Ono, please introduce AIST's synthesiology. Then we would like to ask Mr. Kuwahara to discuss systems building.

Synthetic research and analytic research

Ono

I was a student of the Department of Physics, Faculty of Science in my university days. After graduation I studied temperature measurements and standards at the former National Research Laboratory of Metrology, Agency of Industrial Science and Technology. The Laboratory was reorganized into AIST in 2001. Then I was greatly influenced by Prof. Yoshikawa, the first president of AIST in terms of *Type 2 Basic Research* proposed by him

When I worked on measurements and standards, I had experiences where original papers of our research results would not be accepted in a straightforward way by the academic societies. Our mission was to create highly accurate national measurement standards and to calibrate measuring instruments at the highest accuracy. Since the calibration work extended to business in the private sector, we worked not only on raising the accuracy of the standards, but also on making the calibration work as simple as possible and even on considering cost performance to establish a national traceability system in Japan. Yet, results obtained in these research efforts were not very easy to be accepted as original research papers by the academic societies. We were often asked by the reviewers, "What are the novelty and originality of your work?"

We envisaged a scenario for the national traceability system that best fits the Japanese society. For example, we designed a traceability system by thinking which private calibration labs had sufficient levels of technology, what equipment they had, and how many engineers they had. However, such stories were said not to be appropriate as original research papers. Then we had to write papers only about new elemental technologies that were introduced to the national standards system.

Although it may be more or less different in different disciplines, the Japanese academic societies are mainly for "academia" itself and are not for "engineers" as in the United States, and therefore, it tends to pursue academic novelty.

I've always felt that such pursuit may lose good contact with society, and that's not healthy. When I create something valuable to society, I want to write about it in my original research paper, and I hope it is accepted. But the academic society does not often have such a mind to do so.

However, Prof. Yoshikawa said, "There is *Type 2 Basic Research*." He stated that not only conventional research for elemental technologies but also processes of integrating elemental technologies and manufacturing things are important, and that it was a new kind of basic research. Although such researches were taken lightly in the traditional scientific academy and were considered to be at lower levels, he said that that was not true. When I heard him say so I thought that this is exactly what I wanted. I thought that points of contact with society should be described in research papers, and now I have become the editor-in-chief of *Synthesiology* to realize this.

Today's science was established over a long period of time. Various kinds of factual knowledge were acquired by observing nature and entities, hierarchizing, and analyzing through reduction to elements. Although this method has been greatly successful in science, looking at the current environmental issues and the nuclear power plant accident, the current science that rests upon reductionism and analysis is insufficient in facing the reality of solving such complex issues. Perhaps, there may also be a problem in the current scientific academies that function only within their finely segmented discipline.

In fact, private companies are engaging enthusiastically in "synthesis" and "systems," but the current science has failed to reach that stage. I thought science should extend to include synthesis and systems. It would be science of designing systems and science of integration and synthesis. These are called *Type 2 Basic Research*. If AIST works harder in this field, I feel communications with industry people will go more smoothly. I hope a synthetic approach will be recognized as a method of R&D in contrast with an analytic approach.

Although development and commercialization that are done by private companies are similar to *Type 2 Basic Research* in terms of synthetic activity, public research institutes such as AIST and private companies may have different positions even though the objectives are the same. We are trying our approach while hoping to firmly position the processes of creating systems and products in research.

Systemic thinking for achieving the objective

Kuwahara

I agree with your comments overall. I come from a background of electricity. I joined the company to work somewhere between electricity and mechanics. When I entered the company, the word "systems" was not a general term, but I worked on all sorts of systems including chemical plants, batch control of chemical and food products, sequence control, thermal power plants, nuclear power plants, nuclear plant operation training simulators, and production management systems for automobiles, tires, and building materials.

For the thermal power plant, the United States was ahead in systems, and Japan was a licensee of the US. Our customer made a request that they wanted to automatize the operation. To learn the technology from the US that was engaging in the challenges in this field, I studied in the US for a year, but things were not that great as expected. What I studied hard was their "failures". I learned carefully what kind of failures there were in the past, used that experience to automate the Japanese plants, and as a result, Japan became number one in the world in the automation of thermal power plants.

The problem was what do you do when the plant undergoes unexpected failure. It can not be fixed by computers and it has to be done by persons. If we all depend upon automation only, we overlook training our operators for such cases. To control the plant during emergency, the basic policy is "stopping" it, but it has to be stopped safely. Accurate decision can not be made if the operators are not trained for such non-computer-controlled emergencies. We learned that the emergency training must be done separately while working on automation.

Dr. Akira Ono

Mr. Hiroshi Kuwahara

Also, the Japanese iron and steel making technology became number one in the world in only 30 years after WWII because of the advanced control technology. It was the issue of systems of how to control the process. In the US, the method used was to gather and analyze voluminous data and control the plant by a simple feedback method, while in Japan the original approach for a control system was developed by seeking a theoretical solution by computer. The objectives were the improvement of operation efficiency and of product quality.

Needless to say, systematic thinking and system technology are essential in R&D. The outlet of science and technology for society is the "system" as a complex body of various fields of science and technology. Systematic thinking is not about just creating systems and products, but about obtaining a solution when a certain objective is set. Since the "objective" is clearly set ahead of proposing, planning, and hypothesis formation, everything will be processed smoothly if one creates a competitive scenario to achieve the objective. I believe the key point here is that the challenger must take vigorous actions at this point, but what is the situation now? I think it is really pessimistic.

I know the importance and the value of academic papers, but the Okochi Award is given to the achievements in R&D for production engineering and production technology as well as the actual execution of advanced production method, so this is an award for the world of practice. As Dr. Ono states, heavy emphasis is placed on the papers in traditional academic societies, and the issue is how *Type 2 Basic Research* can gain acceptance as a research discipline, including giving positive evaluation to researchers active in this field.

However, I should say, the term Type 2 "Basic Research" gives us a feeling of a kind of uncertainty. Of course, I fully understand its importance, but the word, "basic research," implies that "it is not an applied research". Yet, I think we need to have the perception that there is *Type 2 Basic Research* together with applied research. The acceptance must be achieved as such . In Japan there are over 750 societies for natural science, and I think you should engage in a strong movement for creating a "systems society" and awarding the researches in this field.

Ono
Certainly, it is difficult in the traditional, segmented academic societies.

Akamatsu
How do you think one can learn such systemic thinking or systems synthesis ability, and what do you think are the qualities necessary?

Kuwahara
What is most important is the "spirit of never-give-up" shared among researchers and designers trying to accomplish competitive systems. It means that what is important is the state of mind for the "spirit of never-give-up", that one will never compromise until he/she achieves fully the objective. If the person has that spirit, whether he/she succeeds or not depends on the ability to create the system. The person with the ability should think of something totally unimaginable and come up with some new competitive system. Therefore, it is necessary to keep "training people" toward that direction by giving them actual hard work for a while ("a while" means until an academic system methodology is established). Excellent system designers have plenty of past experiences and wide-ranging knowledge, and draw them out according to the objective to create the scenario. I have been wondering whether it is possible to theorize such thinking process soon, and we must try to realize this. Other abilities include the ability to cooperate with other people to gain other's help, the ability to organize the total process, and the ability to make appropriate compromises.

Akamatsu
For researchers, it is important to maintain the goal without wavering in executing the research. When one feels, "Well, this will do", it is usually when the goal becomes fuzzy and self-evaluation loosens.

Kuwahara
In industry, easy compromise will always lead to failure. If one looses, one will know for sure. It is a very severe and cold finding.

Ono
It means that the criterion of evaluation is very clear in companies, but it may not be so clear in academia. Objective self-evaluation is important.

Systems building and scenario

Akamatsu
Earlier, Mr. Kuwahara said, "If we have a clear objective, once the scenario is written everything will be easy thereafter." As the editorial policy of *Synthesiology*, we ask the authors to "write your scenarios", but this is rather difficult.

Dr. Motoyuki Akamatsu

Ono
We tell the authors that a *Synthesiology* paper is requested to describe two things, i.e. a scenario to reach the goal and elemental technologies to be integrated. The scenario includes why the authors wanted to do the research and what the authors thought at the beginning of the research. The elemental technologies include materials and parts for the authors to select to do the research. However, since most researchers are just familiar with *Type 1 Basic Research*, they have a hard time writing. Even though they are the ones who actually did the research, they often say, "I don't recall how I came up with my own scenario."

Kuwahara
That's the point. They've got the answer, and they must think about how they arrived at the answer. I have experiences where I come up with a hint for a system when I am hanging out with people unrelated to my work, or in my leisure time. I think various input lay behind scenario building.

Ono
I think so, too. It may seem to be a flash thought, but even a flash thought will not come to you unless there is a base. So, I ask the authors to write what their bases are. That is quite difficult, but I wish to gradually systematize this process.

Kuwahara
Were the papers of *Synthesiology* about this?

Ono
The point might not have been clear. I guess that insufficient communication between the editor and the author is the excuse.

Kuwahara
Before going on to the scenario, it may be useful to do a breakdown of "what knowledge you have, and which other knowledge you poured in to achieve the objectives" Such efforts are important for system generation, and I support such efforts.

Another thought I had was that the researchers are almost always evaluated by their papers, but perhaps that may not lead to an outlet emphasizing policy. *Type 2 Basic Research* is fine as is, but the assets of systems are the discoveries that result into patents. I think additional value should be placed there.

Ono
Conventional papers were of analysis and breakdowns, weren't they? One wrote about taking a watch apart and described what came out. We didn't have a paper that described the act of assembling the parts to make a watch.

Kuwahara
Yet, when we ask the researchers to do *Type 2 Basic Research*, they will do it if it is the development of the *Type 1 Basic Research* in which they were involved. The researchers will never do it from someone else's *Type 1*. Japan will not be able to achieve innovation in this way. To break this is my primary request to AIST.

Ono
That is the best part about AIST's *Type 2 Basic Research*. At AIST, we encourage the researchers to get out of the "octopus hole" or of the compartmentalized way of thinking to transcend the framework of segmented scientific academies and to look at things with a bird's eye view.

Mr. Kuwahara mentioned, "Systemic thinking does not always create outlets, but systemic thinking is essential to obtain a solution for objectives." I totally agree. I think it is the same for joint researches. The point is that doing joint research is not of value itself, but that we have to do research jointly because one cannot realize societal values alone. Therefore, we must consider the scenario for a system.

While the levels of the systemic thinking and scenarios may differ in corporate minds and with those of the AIST researchers, I think good joint research will evolve by sharing common factors. I think we had not been enthusiastic about sharing a scenario until now because our attention was paid to details of technology. If corporate people would discuss scenarios a bit more, I think we can talk about what are different in the scenarios, what are the same, and what we can share. I expect such talks will take off from there. What do you think?

Kuwahara
As one example, there is the seawater desalination system. Currently, the seawater is filtered through the reverse osmosis film. When I hear the explanation on its principles, I am surprised that there are many things that are done without knowing the exact basic principles about water, salt and others. The researchers should clarify and understand basic principles first, and then try out what would happen if this or that film is used or if some biological treatment is applied, or if certain intermediary treatment to the sea water is applied. Various options should be studied for achieving competitive systems. This is currently being done as a JST project. We are working to make this Japanese desalinization process technology overwhelmingly competitive in three, or if not, five years.

We do not have organized thoughts about the principles or laws, and many people in the academia do not know about systems or synthesiology. To find common grounds for our respective fields, I think it is important for us all to study "what system generation is". We are starting a study session for systems technology in a project of the Center for Research and Development Strategy. I hope people of AIST will join us. Basically, joint work will be the best solution.

Akamatsu
Until about 1965, I think the university professors and company people worked closely together to solve various issues. After 1965 and well into the 70s, the companies gained force, and the companies became leaders in system generation and the professors were left behind. I feel that the situation is still continuing. We must do something about this.

Kuwahara
I was the chairman of the Japan Society for Technology of Plasticity. If one has a firm objective of “I want to make this hardware by Plasticity”, studies could be completed and the solution could be obtained using the plasticity technology only. However, if safe and secure society and environment are involved, IT comes in, police system enters, and various services become relevant. This cannot be dealt with by segmented, vertically divided academic societies alone.

Today, our government is promoting the export of Japanese systems overseas and national budget is allocated here and there. Now is the chance. Japan has advanced technology for water, environment, food, and others, and considering the export industry, I think major contributions can be made if the points discussed herein are brought together for system generation.

Effective industry-academia-government collaboration

Akamatsu
AIST is spending effort to promote the collaboration among industry, universities, and public research institutions. I think the industry-academia-government collaborations will become more important in the future.

Ono
Actually, I don’t think the industry-academia-government collaboration is going very well in Japan currently. The reasons are due to the budget and the organizations. Yet, besides that, I think there may be a gap in the consciousness of researchers and engineers. If we understand each other more clearly on what our objectives are, what our differences are, and what can be shared as a common goal, I think industry-academia-government collaboration may be pursued more smoothly.

In AIST, research group leaders have their own scenarios in pursuing their research. We can present these scenarios to obtain better understanding of corporate people, and we are able to communicate with each other at scenario levels. Can we expect such efforts from the corporate side? Are there any barriers such as the issue of corporate secrets?

Kuwahara
That barrier is not small. We must sign a non-disclosure agreement, we must set limits on paper writing and the publication at academic societies must be done after patents are filed, or the researcher must never talk about what they are doing in the project. All of these are for protecting against unnecessary disclosure of corporate strategy. The researchers are itching to publish as soon as possible, and I don’t have a good general solution on that aspect. For now, we must go case by case, and some good solution must be provided for each individual project and I think we can do it.

Akamatsu
Dr. Ono mentioned that the industry-academia-government collaboration is not going very well. What should we consider to do an effective collaboration?

Kuwahara
To strengthen the industry-academia collaboration and for the science and technology to make contributions to the development of society, the Council of Science and Technology Policy must acknowledge *Type 2 Basic Research*, and the government must provide budget to this field. It must gain general acceptance in the academic arena. And then there must also be an unspoken perception that the traditional basic research will not be eliminated.

Ono
AIST is recently encouraging the consortium method where collaborations are done with multiple companies, and this is being done for solar cell projects. AIST and each company sign separate agreements to ensure that information does not flow directly from company to company while the objectives and information for the basic parts are shared among the participants.

Expectation for AIST on systems research

Akamatsu
You talked about the importance of systemic thinking. There is a scenario before the system is created, and I believe the methodology of the scenario for creating the system is synthesiology.

Kuwahara
I have no objections to your comment. The 20th century was called the age of systems, but in Japan, thermal power, nuclear power and chemical plants are almost entirely copied from others. The systems that were created originally in Japan are quite small in number and they are iron-steel manufacturing and railroad train control.

Therefore, I would like AIST to engage in systems research in the 21st century. One example is solar power generation. If you want to do it as *Type 2 Basic Research*, I hope you do at least about one-third of the system, hopefully the kernel of the system. That way, the rest can be an opportunity for system engineers at AIST to learn and grow in the actual project.

Akamatsu
I think it is the matter of how to create “the ability to think systems” rather than the “thing”. In traditional science and technology, the scholarship was considered to be the manufacturing of “things”. What is truly important is how to “think about things”, and it is necessary to position this as part of the discipline. The “ability” part of the discovery ability is important.

Ono
We would like to advance friendly competition of discovery ability with private corporation researchers.

Kuwahara
That will be great. My proposal is for us to cooperate and set a path to summarize “how it is actually done”, and to consider together what must be further enhanced and what the academic positioning is in the future.

Akamatsu
We would like to work on that. Thank you very much for today.

(This roundtable discussion was held at Hitachi Maxell, Ltd. in Chiyoda-ku, Tokyo on May 9, 2011.)

Article contribution after the roundtable discussion

The Editorial Board asked the participants to contribute articles on subjects that could not be covered in the roundtable discussion and they are as follows.

Hiroshi Kuwahara:
Speeding up the development of systems technology

It is commendable that the stance of placing importance on the relationship of science and technology and society, such as the returning the results of science and technology R&D to society and promoting R&D with consideration for the outlet, is gaining acceptance.

The contact points of science and technology with society is all systems in the field of industry. They include extremely wide ranges from simple systems (such as home appliances) to large-scale complex systems (nuclear power plants, various smart systems, etc.). However, it is regretful that not much effort has been spent on R&D in this field, and it is an urgent issue considering the progress of Japan.

Then we must ask, “what a system is” and “what the system building technology is”. Although these are important basic understandings that are essential to the future systems related R&D, their analyses are almost untouched, and it must be done immediately. When people understand these points correctly, only then can we move to the next step. At this moment, it is reckless to start theoretical building right from the initial stage.

As someone who has somewhat deeper experience in systems building, I shall attempt to set a bold hypothesis as follows. I would like to see a discussion.

Typical procedure for systems building

Step 1: Clearly define the objective of the system.

Step 2: Seek the essence of the system. Investigate what principles are expected to be processed and how they are applied in terms of physical, chemical, or social sciences, for the system.

Step 3: Widely gather the findings, knowledge, and research results that may be necessary for building the system based on Step 2.

Step 4: Extract the necessary items, or if some item is missing, assume new technology that is desired and is realizable. Then design several system building plans.

Step 5: Evaluate them quantitatively according to the objective, and the related parties convene to evaluate and discuss them. Make any additions if necessary, and determine the final plan. In this case, price and realizable time scale must be raised as important evaluation items.

I think many people, including researchers, have gone through similar experiences in the past. As people bring their experiences to the discussion, I hope the form of what we are pursuing will take shape. I fear that unless the discussions start from actual experiences, it will end up as a hollow theory.

I had a valuable opportunity to engage in discussions with the people of AIST, and it was extremely significant. However, I also felt that both parties will be hardened into their own ideas and the investigations will go off into different directions if they are left as they are.

There is a momentum now where various activities are starting up, such as the investigations of systems technology at CRDS, at AIST, and at the Transdisciplinary Science and Technology Research Group (Federation and Committee), as well as the emphasis on systems at the Council for Science and Technology Policy, consideration of systems at the Council on Competitiveness Nippon (COCN), and others.

The mutual collaborations of these activities and the activities that may arise in the future are important and significant, and I hope the industry-academia collaboration will bring about wonderful results.

Profile

Hiroshi KUWAHARA
Graduated from the Department of Electrical Engineering, School of Engineering, The University of Tokyo in 1960. Director of Omika Works, general manager of Electric Systems Business Section, managing director, senior managing director, vice-president, and vice-chairman of Hitachi, Ltd. Former member of the Council for Science and Technology Policy, Cabinet Office. Chairman of Hitachi-Maxell, Ltd., Hitachi Cable, Ltd., and Hitachi Kokusai Electric Inc., Special consultant of Hitachi, Ltd. Former president of Global Water Recycle and Reuse System Association. Former vice-chairman of Japan Federation of Engineering Societies. Currently, Senior Adviser Emeritus of Hitachi, Ltd. and Senior Corporate Adviser of Hitachi Maxell, Ltd.

Akira Ono: Future of systems and synthesiology

I have spent all of my working time in the government and the academia. Before the meeting, I was a little bit concerned about how much common ground I could have with Mr. Kuwahara, who had worked as a member of the Council of Science and Technology Policy, and is the opinion leader of R&D in the private sector. Yet, when the roundtable discussion was finished, I saw commonality in several points with him, and I am grateful to him for our sharing this precious opportunity.

I was able to reaffirm that the essence of “scenario” and “integrated and synthetic research” emphasized in *Synthesiology* was deeply related to systems research and systemic thought. I feel that I now see the path and issues for good collaboration among researchers of universities and public research institutes like AIST and researchers and engineers of private companies.

Science was born in 17th century Europe, and reductionism has been very successful. Scientists have conducted researches using analysis as a main tool under the belief that it was important to understand to the lowest layers by breaking down and hierarchizing various phenomena. This approach has been firmly established in the time span of three hundred years, and it is still greatly effective. Yet looking at the environmental issues and the nuclear power plant accident in Fukushima, we can see that reductionism alone is insufficient in dealing with the systems and complex problems. The current science cannot respond to such social demands. Also, as a result of thorough practice of reductionism, the scientific academy has become extremely segmented, and the members have become content with studying narrowly within their own disciplines.

Figure a is a comparison of processes in the analytic approach and in the synthetic one. Processes in the current science (*Type 1 Basic Research*) that are mainly of the analytic approach are illustrated in the upper part of the figure. In contrast, processes in the synthetic approach (*Type 2 Basic Research*) are shown in the lower part of the figure. The current science starts from nature and entities, where human beings are one of the entities, on the right-hand

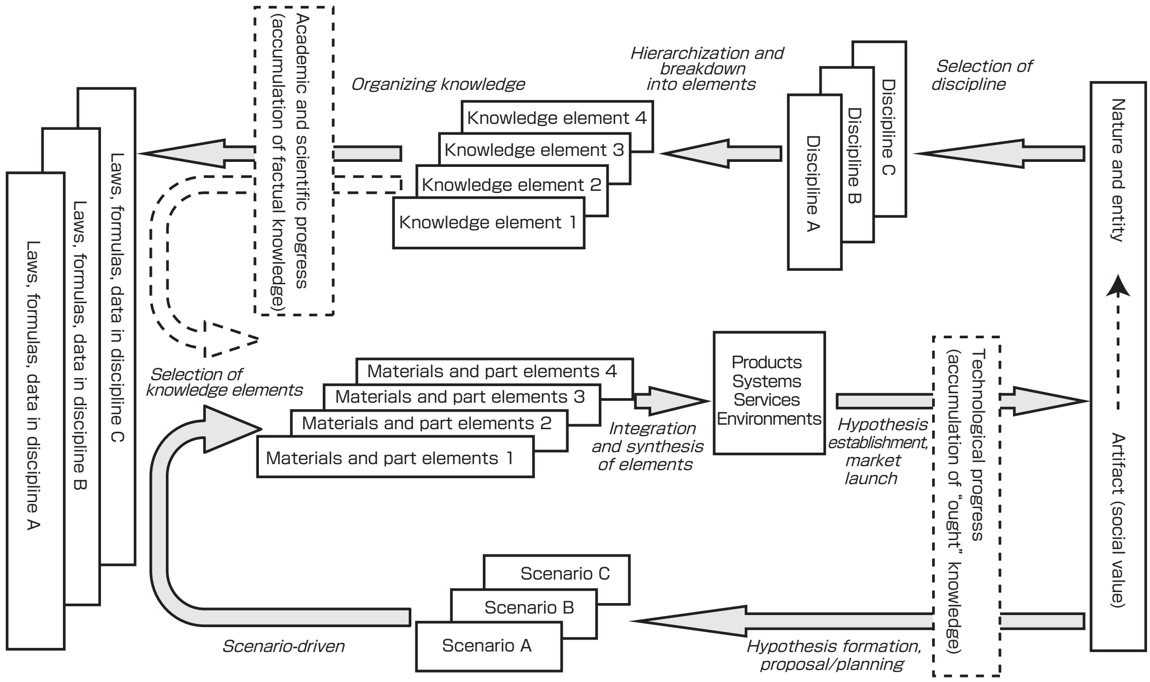

Fig. a Processes of analytic and synthetic research

side, and these are analyzed from viewpoints of individual disciplines. For example, there are physics, chemistry, biology, mechanical engineering, electrical engineering, and others. Nature and entities are observed from individual viewpoints, ranked into hierarchy, broken down into elements, and knowledge elements are organized to enable logical understanding within individual disciplines. For example, in Discipline A, laws, formulas, and data in physics are organized, while in Discipline B, those of mechanical engineering are organized.

On the other hand, human beings have manipulated nature and entities, or have used technology to create "artifacts" that are socially valuable. In solving current complex issues, the final goal of making "artifacts" on the right-hand side in Fig. a, or social values, cannot be attained unless scientific results transcending various disciplines are used. The lower part of Fig. a shows what processes are taken for synthetic R&D. If there is an artifact that one wishes to create, one must make a scenario for realization. In a company, it may be proposals and planning, but in general, it may be "hypothesis formation". The hypothesis for creating a target artifact is considered by researchers and engineers, and various scenarios are made. Researchers and engineers engage in scenario-driven R&D based on the scenario that they consider best. Optimal items are employed amongst the knowledge elements accumulated in the individual disciplines to create materials and part elements. Then products, systems, services, and environments are created as "artifacts".

Created artifacts are put into the market, undergoes evaluation, and returns to the proposal, planning, and hypothesis formation, to enter the loop of evolution. If the created artifact is recognized as an "entity", it will be incorporated into the above loop and becomes the subject of scientific (*Type 1 Basic Research*) analysis.

Current research papers mostly describe the upper processes in Fig. a. The academia highly evaluates the act of organizing knowledge elements such as how an important knowledge element was discovered or how important laws or formulas were found. On the other hand, the lower processes are also an advanced intellectual activity, and I wish to consider it as an extension of science. To do so, it is necessary to separately define the originality and novelty of the lower processes. This is a challenge of *Synthesiology*.

Mr. Kuwahara indicated that it is important to consider how to set up a scenario when the artifact that one wishes to create is determined. This is also another challenge of *Synthesiology*. I think that a person who completed good R&D valuable to society must have had a good scenario. I want him to reconstruct and describe his own scenario in retrospect. When the industry, academia, and government share such scenarios, I hope it will greatly promote mutual understanding and further collaborations.