For Corporate Researchers 2- Research That Matters

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Native English readers should note some of the discussion in this book remains directed towards helping Japanese non-native English speakers develop skills to participate in research at an International level.

Introduction

This book is a message for researchers and students who want to become corporate researchers. One possible choice is to become a researcher of a university or a government research institute and to pursue an academic career. On the other hand, if you become a researcher of a company, you can put your research results to the world more directly through the products and services of the company. The number of researchers in private companies in Japan is said to be around 450,000. These researchers are working in such places as Central Research Labs, Research & Development Divisions, or R&D Centers. These corporate researchers support the forefront of R&D in Japan.

After joining IBM Japan in 1983, I worked at the company's Tokyo Research Laboratory for 26 years. Once I was made Director of the Tokyo Research Laboratory, I spent three years trying to implement my idea of a good corporate laboratory. During that time, I wrote internal blogs (“letters”) on various topics such as research methods, career formation advice, directivity of the business, to about 160 researchers in the form of a company blog.

Afterwards, I changed job and started working at Canon as a staff member of Digital Platform Development HQ, which is a corporate R&D organization with roughly 12,000 employees worldwide. While both IBM and Canon are excellent global companies, their corporate cultures have significant differences, both positive and negative. The experience in Canon gave me an opportunity to think about what technology leadership is. After leaving Canon, I gave a lecture on technology leadership using the insights gained at Canon. This lecture was given in the Department of Technology Management Strategy, Graduate School of Engineering, the University of Tokyo while I was working at the Institute of Statistical Mathematics.

In 2016, I joined the Preferred Networks, Inc. (PFN), and have been engaged in technology development and business development, using deep learning with young researchers and engineers. I have also been engaged in various government committees and learned a lot beyond the viewpoint of a single company.

Though there are differences in the structure and culture of each organization -- large US-based enterprise, large Japanese enterprise, and a start-up company, they have common problems in the research management such as the way of evaluating the research and human resources management. In the previous edition of the book [Ref. 1], I focused on the letters I wrote to researchers when I was the director of IBM Tokyo Research Laboratory.

However, when I read them again after 10 years, I feel that the contents are lacking, especially regarding to the following points:

- In the past 10 years, I have experienced and gained knowledge in research and research management in completely different environments, namely Canon (large domestic corporation), the Institute of Statistical Mathematics (academia) and PFN (start-up company), and
- How the rapid development of information technology is changing the way research activities are conducted

I wanted to revise the book. In the previous work, I presented these letters and explained the ideas behind them. The contents had a sense of reality, and it was good for understanding the state of the research of IBM Research at that time, but it was nothing more than a snapshot of a specific company at a specific point in time. I wanted to convey a broader perspective to readers, including comparison with academic research and the differences between large companies and start-up companies in the private sector.

Thus, in this book, I am going to include additional contents to cover; the case materials that I used in my lectures at the Univ. of Tokyo, insights from the activities in PFN and activities of government committees and academic societies, etc., in addition to the letters in my IBM days, and describe my thoughts for researchers in the enterprise.

I didn't necessarily stay in the lab in my career. I spent some time in a sales department, at a customer's site, a product development department, and a consulting department. From 1997 to 2000, I supervised about 10 graduate and undergraduate students at the Graduate School of Information Science and Technology, Tokyo Institute of Technology, as a visiting associate professor. The variety of experience gave me an opportunity to review the value of the laboratory by looking at it from the outside. In particular, my assignment from 2003, for the consulting department as a full-time consultant, has provided me with many suggestions on how to communicate and how to manage business, and many of these insights are presented in this book.

What is stated in this book is my personal opinion and does not necessarily reflect the policies or strategies of the organization to which I belong(ed). Researchers have different working styles and there is no such thing as the “best” style. So even if you don't see your own style in this book, you don't have to think negatively. Rather, you can think of it as your uniqueness and strength. Similarly, corporate policies and strategies are constantly changing, and even if they were right in my time, they may have since changed and become obsolete.

I hope you will find something useful in this book. Enjoy.

Chapter I: Research That Matters

The results of corporate research should have a visible impact to the world. I call it "Research That Matters". "Matter" means that people move and the world changes. According to Don Stokes's Pasteur’s Quadrant (Figure 1), R&D can be divided into 4 quadrants on 2 axes [Ref. 2]. One axis is whether research is relevant to the advancement of knowledge. The other axis expresses the relevance to immediate applications. The work of the physicist Bohr is typical of basic research motivated by the upper left quadrant, namely intellectual curiosity. On the other hand, what Edison did was to invent a light bulb and perfected it through engineering, so that it would be useful in practice, but he was not interested in exploring the principle of why it worked. Pasteur had a keen sense of purpose to cure human diseases, this led to him proposing basic research of bacteriology. Don Stokes said that the Pasteur type of research, "Basic research with a strong application motivation" should be supported by the U.S. government.

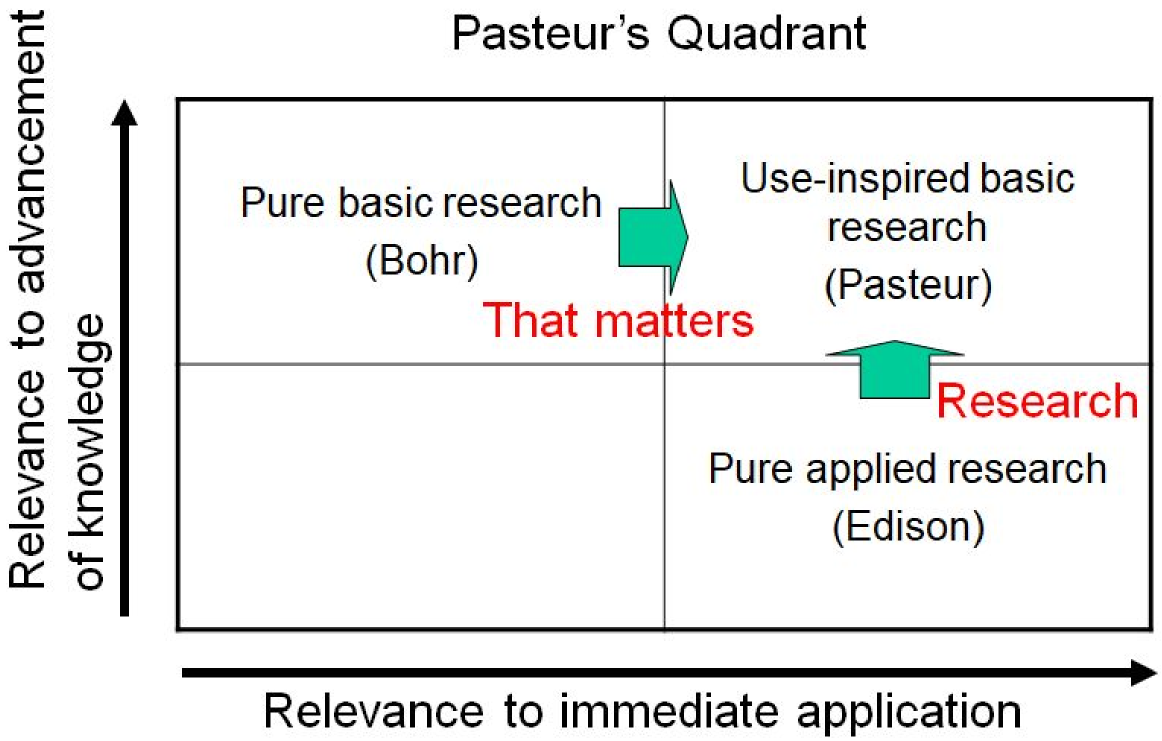

Figure 1. Pasteur’s Quadrant

Edison was a great inventor, but not a researcher. As researchers, we should focus on doing our research work, but we should also seek to understand the principles behind it.. This is the meaning indicated by the arrow "Research" in the figure. On the other hand, research should not only pursue pure knowledge, but also have an impact on the world. To put it simply, we should always be aware of the problems of our customers and society, and consider the problem context, principles and mechanisms required to solve them. This is the meaning of the arrow "That Matters".

Let me tell you a little about my own experience. I joined IBM Japan in 1983 after finishing the master's programme in Information Science at the Tokyo Institute of Technology, and was assigned to the Japan Science Institute (later renamed to the Tokyo Research Laboratory). At that time, research in the Fifth Generation Computer was popular in Japan, and I studied the Prolog logic programming language for natural language understanding before starting to work. I then worked on natural language processing and machine translation in particular for about 10 years and published several papers at top conferences. As a result, I was given a Ph.D. from Kyoto University. Unfortunately, however, it is also true that these studies did not have any impact on the world.

In 1991, I spent six months at a customer's site to put our machine translation system into practical use. This customer was a development organization of an automobile company, and was translating design change notices from Japanese to English, in order to send a design change notices to their overseas factories. A few full-time human translators worked on the tasks every day. I was sitting next to these translators and spent half a year encoding their translation know-how into our translation system. After six months, our translation system was able to translate Japanese written by Japanese engineers into English with reasonable accuracy.

Unfortunately, this system was never used in production. The customer decided that using a translation system would not reduce the total cost - since the accuracy of machine translation was not 100%, it would have been necessary to manually check it and, therefore, impossible to eliminate translators. In order to apply it to a machine translation system, it would have also been necessary to hire an operator to input a handwritten text into a computer. From that experience, I learned that research results alone do not make for a practical system.

Afterwards, I worked in several research areas such as information retrieval, hand-writing character recognition, and multimedia, but they did not lead to impactful results (one exception is information retrieval, which later became a successful text mining technology thanks to the continuous effort of my colleagues).

In the summer of 1996, I was transferred to the Internet Division of IBM, USA for one year. My exposure to various Internet technologies greatly changed my career. At that time, a technology called "Push Delivery" was popular on the Internet. PointCast, which as a typical example would receive news in the background while the PC was not in use and then display it on the screen. In 1997, I was tasked with evaluating the "Push" technologies and got interested in XML when Microsoft started suggesting the use of XML for push delivery. XML has its roots in SGML, a document markup language originally created by IBM, so I thought that IBM would naturally already have a team working on the XML technology. I tried to find any team working on XML in IBM, but surprisingly I couldn't find any. After I returned to the Tokyo Research Laboratory in the summer of 1997, I started a small team to develop XML technology.

This decision turned out to be the right one -- our team published the world's first XML Processor (document analysis tool) that was fully compliant with the XML 1.0 standard on the same day when the standard was released. This technology was made available from IBM's alphaWorks as a free download and attracted a lot of attention. Because of this, we were lucky that Addison Wesley Co. in U.S.A. asked us to author a technical book of the XML technology. The book was published in the U.S., translated into six languages including Japanese, and more than 60,000 copies were sold worldwide over the two published editions.

I also became interested in Internet security during my stay in the US, and I decided to work on it when I was given a position as a visiting associate professor at the Tokyo Institute of Technology which I did in addition to my research in IBM on returning to Japan. At the Tokyo Institute of Technology, I had a small lab with a few undergrad and graduate students. Luckily for me, I learned two completely different technologies at the same time: XML and security. There were many XML and security experts at the time, but it was safe to say that no one knew both at the same time. In the meantime, the necessity of the mechanism which guaranteed the security of XML began to be recognized, after it was proven that XML could be used for data exchange between businesses on the Internet. We proposed many digital signature and encryption methods for XML and contributed to the international standards through the World Wide Web Consortium and OASIS. Since XML became the basis of Web Service afterwards, the Tokyo Research Laboratory often played the representative role of IBM at the standardization meetings. Today, XML as a data exchange format has been replaced by JSON which is much simpler, but the names of many of the members of IBM's Tokyo Research Laboratory, including myself, can still be found in the standardization documents for XML security and Web service security.

The period when I was working on XML or Web services (from 1997 to 2003) was the most productive in my research career. That was between the ages of 39 to 45 - I was not young as a computer scientist, but I could feel that I was making a difference in society.

If I was to be asked which one, between the first ten years of my work on machine translation or the six years of my work on XML, had a larger impact, I would reply that the latter had the most impact. Yes, I was able to write a thesis and get a Ph. D. in machine translation, but the work was a dead-end and it did not result in making an impact on the world. On the other hand, the work of XML did not allow me to write many academic papers, but it affected a great many people through standardized documents and published books. Through these experiences, I am convinced that the true value of corporate research should be measured in terms of impact in any form. When I became the director of the IBM Tokyo Research Laboratory in 2006, I expressed my idea succinctly with the term "Research That Matters".

What does it mean by "matter?" I think it can be rephrased as "make impact". There are various ways of making impact. You can write an impactful paper that is referenced hundreds of times. Or, in the case of computer science, developing software that is used by people all over the world, such as Linux, would have a big impact. Our work on XML and

Web services greatly influenced the IT industry through achieving a level of standardization in secure data exchange.

Since its inception, the start-up I joined in 2016, PFN, has demonstrated new deep-learning technologies one after another, and is making an impact on the world. In 2016, at CES (Consumer Electronics Show), the world's largest trade show held in Las Vegas, PFN demonstrated that multiple deep-learning powered miniature cars could autonomously drive through an intersection, without bumping into one another. During the same year, in Germany, our team won the 2nd prize at the Amazon Picking Challenge, a competition for industrial robots -- this was achieved in spite of it being our first time participating in the competition. Inspired by these technologies, Toyota and FANUC[1] decided to work with PFN and made significant investments in PFN.

Figure 2. "A collision-free car" demo at CES (left). Amazon Picking Challenge (right).

In the following chapters of this book, I will describe my ideas on things that you should be careful about in order to conduct research that "matters," what kind of career path you should consider, and what kind of organization and environment should be able to do such research. These are the ideas that I have been offering to my fellow researchers during my career.

[1] FANUC is the world’s largest supplier of industrial robots.

Chapter II: Conducting Research

Any research that matters must consist of the following three steps:

1. Choose a good problem (research subject)
2. Solve the problem
3. Connect results to value

Selecting a good problem is the entry point for research, but it is also the most difficult step for many researchers. If the problem setting is bad, it may be meaningless to solve, or it may be too difficult to solve in the first place. Once a problem is set up, the main part of the research activity is to solve it. If a solution is found, it is incorporated into products and services and/or published as papers. In the end it must be connected to value for the company.

The absence of any one of these three steps does not constitute meaningful research. I don't think Japanese universities and graduate schools provide systematic education and training on the above-described "Method of research". I myself didn't learn this much in college, and I spent a few years as a company employee trying to figure out how to plan my research. When I joined IBM, the Tokyo Research Laboratory (called the Japan Science Institute at the time) had just been established and nobody could teach me how to do research. It was only 7 to 8 years after joining the company that my papers were consistently getting accepted for publication. It was at about this time that I felt I had finally acquired my own research style. Looking back, I think that Japanese researchers, in particular, are not good at setting the entry and exit steps of research, i.e., the steps 1 and 3.

In this chapter, I would like to give you a hint by dividing the research activities into three steps: the entry, the main body, and the exit.

2.1 Choosing a Good Problem

The first aspect of good research is choosing good problems (some companies call it a “research theme[2]”). The ability to choose a good problem is an important quality for a researcher. In my letter to the researchers at IBM's Tokyo Research Laboratory in May 2006, I explained my thoughts on what a good problem is.

Letter "Choose a Good Problem"

One of the most important qualities for researchers is to "Choose a good research problem". In this context a "problem" may constitute things such as "developing a system that satisfies the characteristics of X"). There are infinite problems in the world. With a limited

[2] The word "theme" is often used in Japanese companies to refer to an individual research subject.

number of researchers and a limited life span, the question of which problem to solve is very important. First and foremost, you have to choose the problem that makes sense. No matter how great the result is, it won't make much sense if it doesn't have an impact on the world.

Similarly, it is important to know how difficult it is to formulate the problem. A great many problems in the world are easy to solve, and researchers need not spend their precious time working on them. Also, many other problems are extremely difficult or impossible to solve, and working on them may be a waste of time. Therefore, it is important to aim somewhere in the middle - a difficult problem that we don't know how to solve today but will probably be solved in a year or two. The first step in good research is to formulate a problem that, if solved, will have an impact on the world and can be solved if you provide sufficient focus on it.

== End of Letter ==

Research tasks should never be simple. Solving a problem with a known solution is not called "research". If you've solved all the problems you've set up, you haven't tried enough as a researcher. I want you to keep that in mind.

One researcher who is good at selecting research subjects is Tamiya Onodera of IBM Tokyo Research Laboratory. He was involved in the design and implementation of a C extension language called COB (C with Object) in the late 1980s, before C++ became a major object-oriented extension of C. It was a modern programming language with type safety like Java and memory management via garbage collection, without sacrificing the convenience and performance of C. It was practical for solving many types of problems and its implementation, developed by Onodora himself, was efficient and stable. I wrote a number of programs in the COB language. The core of the machine translation system described above was written in the COB language. COB had an ambitious design at that time, but I think it was an elegant language that never sacrificed performance or practicality. Onodora has since made a major contribution to IBM's products by working on improving the memory consumption and performance of the Java language and large middleware programs. Today, he leads research activity on quantum computing.

The following letter goes on to describe my own experience.

Letter “Look for Problems”

Some might say, "Even if you want to choose a good problem, the problem is often given by your boss and there is no room for you to think about it." It is true that a broad theme is often given from above, but I think you can find technical challenges in it. Small technical challenges are fine at first. I worked on a handwriting recognition project for a few months in 1993. My goal at that time was to write a search program that would find the word candidate with the highest score by matching the recognition results to a dictionary. A well-known approach was beam search, which could be implemented simply. However, it was slightly less efficient because of the common suffix (character strings at the end of a word) are processed independently (e.g., words ending with “ -tion”), and good candidates might be discarded due to lower scores in the first part of the word. I then noticed that the structure of a dictionary (data structure called TRIE) could be seen as an automaton, and wondered if it was possible to combine words with the same suffix into a common path using the automaton minimization techniques. I went to the library and surveyed the automaton minimization algorithms and found a paper on an algorithm that could be minimized in the *n*log (n)* complexity. As a result, I was able to improve the accuracy and efficiency of the overall handwriting recognition system.

I believe that whatever subject is given, you should be able to find a good technical challenge on your own. If you can't find technical challenges in your work, talk to your boss, friends, or mentor. Or you can talk to me directly. Then, trust your taste and find a good problem.

So how do you get a good taste for research problems? There are many approaches to this, but one way of thinking is to touch on the research content and ideas of many people. To do this, you should always be interested in a variety of things and keep an eye on what's going on in the world. When I listen to people, I think about things like "Oh, this story is similar to the problem I had during my previous study on XX" or "This technique seems to be applicable to the problem of YY." Always add something new to your repertoire of interests. Even if you don't know if it will help in the future, I'm sure it will. In particular, I recommend that you study mathematics, logic, and other foundation disciplines from time to time.

One of the researchers I admire is Prof. Takeshi Tokuyama of Tohoku University (currently Kwansei Gakuin University). He used to work at the Tokyo Research Laboratory, and when I was stuck with my research, I often went to see him for advice. As you know, Dr. Tokuyama is an authority on algorithm theory especially in computational geometry, but he also has a wide range of knowledge such as cryptography, graph theory, and data mining, and publishes papers in various fields. Having so many pockets and being able to pull stuff out of them at will greatly broadens your research and refines your sense of formulating good problems.

== End of Letter ==

Yasunao Katayama of IBM's Tokyo Research Laboratory is another researcher with "many pockets.". He originally specialized in physics and received a Ph.D. in electronics from Princeton University. He made contributions in physics, semiconductors, and information theory before he invented a very high-speed forward error correction code. Then he invented the optical / wireless high-speed interconnect for data centers. Recently, he proposed an innovative way of wave-based natural computation and received a best-paper award from IEEE [Ref. 3]. He continues to come up with surprising ideas in new fields, and I think this is because he has a solid foundation in multiple fields such as semiconductors, physics, and information theory, and is always interested in various applications. This is an example of a good researcher.

Takuya Akiba of PFN changed his job from the National Institute of Informatics in 2016. He was originally a researcher on algorithms, but at the same time, he is a world-class contender in competitive programming. Despite not being an expert in deep learning, because computation took too much time he tried to speed up the training of a large deep learning network using a parallel computer. PFN was building its own supercomputer named “NM-1” with 1,024 modern GPUs and, by using it, achieved the world's fastest speed on the Imagenet + ResNet 50 benchmark. His deep knowledge of distributed algorithms and the ability to implement them enabled him to do so, taking advantage of the rare opportunity where ordinary researchers would have had to reserve 1,024 GPUs for a few weeks to achieve the same computation. The training time in 15 minutes was four times faster than the previous world record set by Facebook, drawing attention from all over the world. His choice of the research goal, carefully estimating what kind of result would have an impact and the likelihood of realizing it, qualified the effort as good research.

As with any other laboratories such as the Institute of Mathematical Sciences and PFN, IBM Tokyo Research Laboratory received visitors from outside research organizations. In such cases, we asked them to give lectures to researchers as much as possible, in order to stimulate and provide new perspectives to our researchers. A few days after I wrote the above letter, Professor Takeo Kanade of Carnegie Mellon University came to the Tokyo Research Laboratory and gave a lecture. I was most impressed by the fact that Professor Kanade seemed to be very happy when he gave a lecture on the content of his research. I wrote the following letter to reinforce my belief that research issues must ultimately be interesting and enjoyable.

Letter "What is Interesting Research?"

You heard Prof. Kanade’s talk two weeks ago, didn't you? It was visual and full of jokes, and I think it was a first-rate entertainment. That's exactly like a Kansai[3] person! Moreover, the way of presenting itself was probably the key message of Prof. Kanade, "Research is fun and enjoyable." Wasn’t it?

Then, how can we do interesting and fun research? Good research should be interesting and fun. As for good research, Prof. Kanade mentioned the following words by Allan Newell of artificial intelligence fame:

- Good science responds to real phenomena or real problems
- Good science is in the details
- Good science makes a difference

The first is that good research can be produced from the simple idea of "Why is this?" or "I wish I could do this" in the real world. The second, I think, is that to solve such a problem, you have to go into the details. In other words, these 2 points are connected to the notion of "an amateur idea and professional execution" which Prof. Kanade says in [Ref. 4]. The 3rd one, “Good science makes a difference,” is, in our way, "Research that matters".

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[3] The Western part of Japan, where people are known to have entertaining characteristics.

Prof. Kanade quoted Newell once again at the end of his presentation and added that "the problem is waiting for you to solve it." In other words, there are many good problems in the world and it is our job to find and solve them.

Therefore, to be a good researcher, you need to have a sense of choosing good problems. Did you read my thoughts on this in my last letter? Prof. Kanade also said that "Most problems in the world are too difficult to solve, or otherwise they are useless to solve." So how do you develop a sense of choosing good problems? When I asked this question at the dinner table, he thought for a while and told me to think about "What kind of demonstration are you going to do if the research is successful?".

Prof. Kanade says that, when you demonstrate your research results, it is not enough to be asked "How does this work?" but the question that you are asked should be "How much is this?" In other words, a good demo makes people to think about "This technology could be used for my problem" or "It could be used for that problem" as soon as he or she sees it. The purpose of the demo is to help people understand the impact of the technology on the world. In other words, the novelty of the underlying algorithm or the difficulty of realizing the technology is second in priority to the purpose of demonstration.

In his speech, he repeatedly said that "research must have a scenario" but I am sure he meant the same thing as above.

In the spring of 2018, PFN decided to exhibit a demonstration of robot technology at CEATEC in Makuhari that fall. Many members gathered and discussed what kind of demonstration they would do, and decided to demonstrate "A Tidying Up Robot" in which toys and clothes scattered on the floor would be cleaned up in a mock family living room (Fig. 3). The biggest technical challenge was to integrate state-of-the-art technologies such as image recognition technology, robotics technology, voice recognition technology amongst others in a very sophisticated way. The demo performed in the exhibition for one week without any major issues. The demo was well received, and many people asked if the technology could be used not only for household cleaning, but also in many situations we hadn’t considered before. In that sense, I think Prof. Kanade would have said that it was a good problem setting.

Figure 3. Demonstration of "Tidy Up Robot" at CEATEC

2.2 Solve the Problem

Once a research subject is set, it is the main body of research activity to solve it. Even if you already have a concrete idea of how to solve the problem, first you should take a step back and look at the whole problem and think about how others would solve it. Then reduce the problem to a set of smaller subproblems until each can be solved directly. Let's briefly review some of the common problem-solving techniques you can use.

Split the Problem into Subproblems

Often the problem at hand is too complicated to be solved directly. The concept of divide-and-conquer (Divide-and-Conquer) is a common approach to solving complex problems. In order to solve the problem A, it is decomposed into subproblems B and C. Then solve them independently, and synthesize the solutions to solve A. The method is simple, and most people use it unconsciously.

The difficulty lies in decomposing the problem without overlapping and without leakage. This "without overlap" and "without leakage" is sometimes called MECE (Mutually Exclusive, Collectively Exhaustive, often pronounced "Me-See" in the Japanese business scene). Originally, it seemed to be a concept which McKinsey, a consulting company, had as a methodology in the company, but it became well known as a tool of logical thinking.

In many cases, it is easy to see that there is no overlap between B and C when problem A is decomposed into subproblems B and C, but in some cases it is difficult to see whether they cover all cases of A. Yoshitake Suwa's methodology called "Vertical and Horizontal Questions" can be used in such cases [Ref. 5]. This methodology is very simple, as it just repeats the following two questions:

1. Name one cause of the problem
2. Remove that cause from the problem. Has the problem been solved?

If the answer to Question 2 is NO, then return to Question 1. In this way, all the factors can be accounted for. The key point is that it's hard for people to list all the factors at once, but it's relatively easy to list just one problem. This idea is similar to Toyota's "Why five times?" (Methodology to find the true cause of the problem by repeating "Why?" for five times) but I think Mr. Suwa's "Vertical and Horizontal Questions" is the best problem solving tool I have come across.

Generate Hypotheses

Suppose problem A is divided into subproblems B and C, what should we do if there are multiple possible solutions to subproblem B? Since research is the activity of solving a problem with no known solution, it is likely that some solutions would fail. Solving the problem requires trial and error. In research, trial and error must be divided into two phases, hypothesis generation and hypothesis verification.

In the hypothesis generation phase, we enumerate the strategies for solving the subproblems. Search prior-art papers and list all applicable techniques. Brainstorm with the team and generate many ideas. In the hypothesis generation phase, it is important to widen the window of the view as much as possible and put many ideas on the table.

After the hypothesis generation phase is finished, we enter the verification phase - the hypotheses on the table are compared and examined, and verification is carried out in order by prioritizing the hypothesis. It is important to be clearly aware of whether you are in the hypothesis generation phase or the verification phase. Once a hypothesis has been chosen and entered the verification phase, even if another promising hypothesis is conceived in the process, the focus should be on testing the current hypothesis until some result is obtained. In this way, even if the result of the verification is negative, the knowledge that "The hypothesis does not work" can be noted. The important thing here is to record the reasons for your judgment as "It doesn't work" in your papers or research notes. That way, if all the other hypotheses fail, you can decide whether to go back and dig deeper.

There is often an expression in the know-how book of problem solving that hypothesis generation and verification are cycles. This is true in the sense that the cycle of selecting and testing each hypothesis is repeated, but at least in research, you should enumerate as many hypotheses as possible before testing any one of them. In many cases, verification is time consuming and costly. Starting to test the first hypothesis that comes to your mind often leads to a waste of time because you may find a better hypothesis later. The worst thing to do is, when testing one hypothesis, to jump at another hypothesis that comes to your mind before the test results are available. Your research is expensive. If the verification is stopped before any results are produced, the output during that time would be nothing. Even if it is a negative result, you should record it well. Then, the company may be able to translate it into future value.

Test the Hypotheses

Verification must be rational in some way. If your hypothesis is an *existential hypothesis* (There is an X that satisfies a certain property), then the easiest way to test it is to find or create X. The hypothesis "It is possible to make a computer program that can beat a human champion in Go" is the existential hypothesis, tested by Google's AlphaGo. On the other hand, if the hypothesis is not about a specific fact but a general law, e.g., “Any object is pulled to the center of the Earth” (*universal hypothesis*), more complex reasoning is required. There are two main types of rational reasoning used in science with respect to the universal hypothesis are *induction* and *deduction*. Deduction is relatively straightforward, but the difficulty is inductive reasoning.

It is said that it was Euclid who discovered the principle of "Light travels straight". He may have observed things such as shadows of objects and sunbeams through trees, and may have arrived at the principle of "Light travels straight" as a hypothesis to explain these observations. Let us assume that H is the hypothesis "Light travels straight", and that the appearance of shadows and the observation of the sunbeams through the trees are O_1 and O_2. We believe in H because H (Light travels straight) can explain a variety of phenomena, such as O_1 and O_2. Inductive reasoning, which observes nature and finds common principles inductively from it, is one of the fundamental methodologies of science.

The hypothesis H of "Light travels straight" explains observations such as the shadow and sunbeam through the trees, but it cannot yet be regarded as a general law. It is only accepted as a hypothesis which is consistent with the phenomena observed until now. There is no guarantee that there will not be other observations, in the future, that contradict H, nor that there is no other hypothesis to better explain O_1, O_2,.... Indeed, the hypothesis H which states "Light travels straight" is not always true. Light may not travel straight in a different setting (Fermat later proposed the hypothesis that "Light travels the shortest path" instead of H).

In which cases, then, are some hypotheses found to be correct (at least for the moment) in inductive reasoning? At the end of the 19th century, a new innovation occurred in the method of inductive inference in science. It is the concept of statistical hypothesis testing. If observation O is always true under hypothesis H, only one observation of not being O ($\neg O$) proves $\neg H$ (called *proof by contradiction*). The problem is that scientific observations always contain errors. Suppose we know logically that we have a high probability of observing O under hypothesis H. Suppose that observation $\neg O$ contradicting O is obtained as a result of an experiment. If we assume that hypothesis H holds, we have to construe that a very unlikely event occurred by chance (an error). Rather, it is natural to think that hypothesis H is not valid in the first place.

What this inference approach shows is that the hypothesis H is (probably) not valid. Therefore, the statistical hypothesis test proceeds with the assumption $\neg H$ (called the *null hypothesis* because we want to reject it as unlikely as a result of an experiment), not with the original hypothesis H (called *alternative hypothesis*). As a result of the experiment, suppose that we observe the negation $\neg O$ of observation O that should be obtained when assuming $\neg H$. This suggests that the null hypothesis $\neg H$ is wrong unless a low-probability event occured. As a result, we conclude that $\neg H$ is not valid, that is, the opposite hypothesis H is valid. Of course, there are errors in the experiment, so the results may be probabilistically wrong. Therefore, considering the probability distribution of the error, the probability that the experiment result was an error under the null hypothesis $\neg H$ (called *p-value*) should be kept below a certain value. This "certain constant value" is called *significance level*, and the value used in many scientific papers is 5% or 1%.

This is a very powerful inference tool indeed. Inductive scientific reasoning was supported objectively and quantitatively for the first time in history by statistical hypothesis testing established from the end of the 19 century to the beginning of the 20 century. At present, the inference based on the statistical hypothesis test is used in many scientific fields.

Note that the statistical hypothesis testing must be in the form of "first fix the hypothesis and then plan the experiment" in order to make correct inferences. Before IT emerged, statistical hypothesis testing was a very expensive task. In order to obtain statistically significant results, a considerable amount of experimental data had to be collected. In addition, the statistical processing had to be done by hand or by using primitive computational tools (abacus, numerical table, slide rule, mechanical calculator, etc.). So scientists had to choose their hypotheses very carefully. This is because the statistical hypothesis test cannot say anything unless the result is $\neg O$ (i.e., enough evidence to reject the null hypothesis). Only well-developed hypotheses were tested in experiments and hypothesis testing.

However, the situation has greatly changed since IT became deeply involved in scientific experiments. Statistical hypothesis testing can now be performed with a single mouse click in Excel. Thus, any number of complex hypotheses can be generated and tested against a single experimental result. In such a world, what would happen in scientific experiments with statistical testing with a statistical significance level of 5%? A p-value of 5% means that you are likely to pass statistical hypothesis testing by an error about one in 20 experiments, even though your hypothesis is not valid. Alternatively, if you test 20 independent hypotheses that are not likely to be related to each other, one hypothesis is expected to be accepted as valid, even though the hypotheses are not valid, by a well-accepted "grammar of science". In fact, there are many papers in which the statistical hypothesis test was not used correctly, resulting in wrong conclusions. Therefore, the American Statistical Association issued an unusual statement[4] in 2015 calling attention to the misuse of p-values.

Model Relations

Statistical hypothesis testing is a protocol for inductive inference of the truth or falsity of a proposition. On the other hand, some laws of science are not about if they are true or false, but about relations. Suppose we want to show the law that a relationship $R(X,Y)$ exists between one variable X and another Y. How can we show this inductively from experimental results? For example, the observed values of X and Y are obtained as a scatter plot as shown in Fig. 4. From this figure, we can imagine a linear relationship between X and Y, such as $Y = aX + b + \varepsilon$ (ε is a random variable representing noise). Given the hypothesis that "X and Y have a linear relationship" what are the values a and b that best explain the data obtained in this figure? Linear regression, which you learn in the physical experiment classes, is an example of such reasoning.

[4] https://www.amstat.org/asa/files/pdfs/P-ValueStatement.pdf

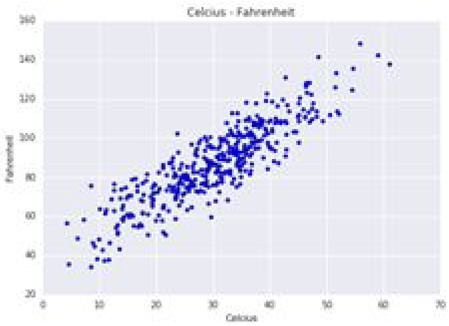

Figure 4. Scatter Graph

More generally, when considering the relation R between X and Y, we consider the relation family[5] $R(X,Y;\theta)$ where θ is a parameter used to specify a particular relation (for example, $Y = 2X + 3$) in the relation family (family of whole linear relations). In this case, $\theta = \langle 2,3 \rangle$. Under the assumption that X and Y satisfy the family $R(X, Y; \theta)$ of the relation when some experimental data $\{\langle x_1, y_1\rangle, \langle x_2, y_2\rangle, \ldots, \langle x_n, y_n\rangle\}$ is obtained, the calculation of θ_0 which best explains this experimental data is called *statistical modeling*. Linear regression with the least squared error method is a typical example of statistical modeling.

What does "θ_0 which best describes the experimental data" mean in statistical modeling? In fact, there are several ways of thinking about "explain best" but one that is often used is "Assuming θ_0, the probability of obtaining the experimental data is the highest." which is called *maximum likelihood estimation*. Note that we discuss the probability after fixing θ_0. It does not say that θ_0 has the highest probability given the experimental data[6]. Even an unlikely θ_0 may be chosen by maximum likelihood estimation as long as it can explain the experimental data at hand. Remember that statistical modeling with maximum likelihood estimation is a powerful tool for inductive relationships, but it also has limitations.

Infer Causality

Statistical modeling yields correlations, not causality. Causality is a relationship in which variable X causes a consequent change in variable Y. Discrepancy between correlation and causality can lead to incorrect conclusions. For example, if the correlation of "There are less crimes in cities with fewer policemen" should not be understood as "Because of fewer policemen, there are fewer crimes." Probably the causality is the reverse - there are few policemen because there are fewer crimes.

How can we test the hypothesis if we really want to assert causality? A technique called *randomized controlled trials* can be used here. This is a standard method used in the world of drug discovery. If a drug is shown to be effective (i.e., whether there is a causal

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[5] In statistics, a family of relations are sometimes called a *model* (e.g., as in “model selection” in AIC, or Akaike Information Criterion. It should be noted that the term "Model" may be used in various meanings depending on the context.

[6] In order to obtain θ_0 with the highest probability after fixing the experimental data, θ must be treated as a random variable and the prior probability distribution of θ must be assumed.

relationship of "Drugs improve symptoms"), patients are randomly divided into experimental and control groups, with the experimental group receiving the drug and the control group receiving a placebo. If the experimental group showed a statistically significant improvement, it can be interpreted as a causal relationship. This randomized controlled trial is similar to the *A/B testing* commonly used in advertising.

Randomized controlled trials cannot be conducted without intervention in the experimental subjects. How can we show a causal relationship for an experiment that we cannot intervene into? On rare occasions, circumstances may naturally occur that could be considered to be essentially the same as randomized controlled trials. This is called a *natural experiment*. Consider the paper [Ref. 6] on whether a causal relationship of "Working women quit their jobs when they have children." holds. Normally, in a randomized controlled trial, working women should be divided into two groups, with the experimental group instructed to have children and the control group instructed not to have children, but this is not possible. In this paper, they focused on working women who already have two children. Another study found that if two children are of the same sex, women are more likely to have a third child. If the gender of the offspring is determined at random, this is similar to a situation where women with two children are divided into two groups, one group having another baby and the other group not having another baby. This paper examined the rate at which women in each group quit their jobs and showed a causal relationship between giving birth to (the third) children and quitting their jobs[7].

It is an essential skill for researchers to draw conclusions from experimental data using the proper protocol, but it is difficult for non-statisticians to use statistical methods correctly. I, too, once misused the statistical hypothesis testing. It wasn't until I came to the Institute of Statistical Mathematics and studied statistics that I got it right. If you are not confident, ask senior researchers around you how to use statistics properly.

Make Improvements

In some cases, the goal of research is not to test or model hypotheses, but to improve existing systems, such as to improve the accuracy of sales predictions, reduce distribution costs through optimization, or improve product yield.

When conducting an *improvement type of research*, first of all, an objective quantitative index to be improved should be defined. In other words, an objective variable must be clearly defined. It is desirable that the *objective variable* is directly connected to the goal in the business (sometimes referred to as *KPIs* (Key Performance Indicators) in the business world), and at the same time, the effect must be measurable in a timely manner. An indicator that is appropriate as a business goal but whose effect is known only after the research, such as "sales 10 years from now" is not appropriate as a research objective variable.

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[7] This study does not show a general causal relationship of "Working women quit their jobs when they have a child" but shows that this can be said at least for working women who already have two children.

If the problem is not an improvement in the customer's particular environment, but an improvement in the general methodology, then there may be a standard problem (benchmark problem) that is widely used by the research community. MNIST and ImageNet in image recognition, the standard data set NTCIR for document retrieval, and the standard data set for Switchboard conversation speech recognition tasks for speech recognition are examples. Since the existence of these standard problems promotes sound research and development, it is one way to use the index of such standard problems as an objective variable for improvement.

Next, it is necessary to clarify the present baseline. What is the current factory yield? How accurate is the sales forecast? What is the latest report on the accuracy of standard problem recognition? If there is no known baseline, some assumptions must be made. In a problem setting where there are no known methods, a baseline may be, for example, randomly rolling dice to predict tomorrow's sales.

It is also important to estimate the upper bound of improvement in practical problems. Alfred Spector, former Vice President of IBM Research, has always asked the question "If this project worked perfectly, how much would it have improved?" when reviewing projects like this. For example, if we wanted to reduce distribution costs by optimizing the distribution of trucks, how much would we reduce costs if we asked God to fully understand supply and demand information and use infinite computing resources? Of course, the minimum number of trucks must run the minimum distance. Therefore, there is a cost limit that theoretically cannot be reduced further. Of course, the upper bound is not something that you can calculate no matter how hard you try. Since the estimation is based on incomplete information, it merely indicates that "There is no improvement beyond that." Nevertheless, clarifying the upper bound may be a good indication of how much improvement is likely to be made.

If the baseline and upper bound of improvement are clarified, it indicates how much improvement can be expected. I call the ratio of baseline to upper bound *Smart Factor*. When the the Smart Factor is 1.01, the problem is one that, no matter how well it works, can only be improved by 1%. On the other hand, in the case of a problem with a large smart factor (For example, a Smart Factor of 10), there is a large room for improvement, and if successful, it will lead to a large impact. I was involved with smart city projects in my IBM days and used this "Smart Factor" to make a quick estimate of how much would be improved by introducing the "Smart" concept.

In such an improvement type of problem setting, it is important to define the objective variable and to clarify the baseline and upper bound. Then, enumerate various hypotheses for the improvement, and test them.

Design Solutions

Many corporate research projects are of *design-type*, which involve building a system that has not been made before. One example is ALTO, the world's first personal computer made by Xerox Palo Alto Laboratory. Though it introduced new functions such as the bitmap display, graphical user interface, Ethernet, mouse, etc., its largest impact was the concept of a workstation for the individual.

Design-type research is more difficult than problem solving-type research. There are two reasons for that. First, it is difficult to objectively measure whether a design works. The operating system UNIX developed by AT&T Bell Laboratories was the prototype of the modern operating systems such as Linux, iOS, and Android, which are widely used at present. Whether an operating system is good or bad is ultimately determined by "How do people feel about it?", not objective indices such as its functions and performance.

The second reason is that there are infinite possibilities in design, and you don't know which one to choose or which direction to take. Prof. Makoto Nagao, former president of Kyoto University, says that this is why engineering is difficult [Ref. 7]. Science is the study which advances towards the truth, but engineering is the study of design which makes things. Therefore, the success of engineering research depends on what kind of things our society would value - which in itself is a philosophical question. That is, engineering is a discipline which simultaneously embraces the elements of science and philosophy, and the insight on both is indispensable.

Suppose you have to design a customer's system. If a laboratory is required to solve a specific difficult problem, you are better off specializing in solving the problem and letting the system integration be taken care of by a business unit or a system integrator. On the other hand, if the design itself has novelty and difficulty and is valuable, it must be designed as part of your research. If this is the case, you should admit that it would be difficult to produce a paper out of your research.

I will touch on integration-type research again in the section of technology transfer (Section 2.4), but at this point I would like to point out that a good design is usually done by a small number of people rather than with a large number of people. It may be worth noting, that even though the highly sophisticated programming languages PL/I and Ada were designed by a large number of people, neither resulted in wide adoption, whereas the lightweight and versatile language C has been widely used for almost 50 years. I don't know what the secret of a good design is, but at least you need a good sense, and good sense is developed from deep insights and consideration of people (the users). On the other hand, some researchers are good at solving problems while others are good at designing. I think it is desirable that your team has both types of researchers.

2.3 Proceed with Your Research

Now that we know the scientific methods of problem solving and reasoning, how can we proceed with our daily research? The researcher is also a human being, and it may be difficult to come up with an idea or to decide a hypothesis to proceed to verification, or maybe we fall into a so-called slump impeding progress. On the other hand, there are schedules for business units and customers and submission deadlines for papers, so even research projects must be planned in advance. I am not good at planning, and for me, it is hard to do development and experiments on schedule. It seems that there is a big difference among individuals. For example, my son (now he is grown up and a father of a daughter himself) is not a researcher, but he used to make a good plan for summer vacations every year when he was a student, and he always executed his plan. I'm always running around with deadlines. I sometimes wonder why such a difference exists between a parent and a child.

Here are two tips for "I am always running around with a deadline"-type researchers. The first is the backcast style of research -- that is, "write a paper before starting a study." Let’s look at this further in the IBM Tokyo Research Laboratory letter from 2008:

Letter "Backcast Method"

I've been conducting the annual project review for a while now. This is a great opportunity for me to meet each and every one of the approximately 160 researchers, and it helps me to understand the content of each project. This time, I would like to share with you one thing that I discovered. You should think about how to appeal the results of your research in advance before conducting the research. Many of you have very good ideas, but often only after you have results you start to think "How do we evaluate the effectiveness of this system?". As a result, you realize that you don't have enough data to prove your idea due to not having the time to do the experiments. I often wonder what if you had thought about these things before you started.

What about giving priority and allocating some time for research results in advance? I have heard that an easy way is to write a paper (or outline of it) before starting your research. I believe this is a good practice, because a technical paper has a standard structure which contains all the necessary components such as motivation, prior-arts, problem formulation, solution methods, results, and future work.

Among them, the problem formulation is important. In other words, make it clear what is the problem to be solved in the paper. Of course you have a long-term vision or goal, but more important is the problem to be solved in this particular paper. You won't be able to solve the long-term goal all at once, so you'll have to isolate the problem to a certain extent and put it into a manageable sized problem. You will also need to make a number of assumptions. How to make reasonable assumptions is one of the keys to success.

Once the problem is formulated, you have to look at previous work to see how this problem has been attacked before. If there were no previous studies, it would be necessary to know in advance what would happen with a naive approach that anyone could come up with. You need to understand this as a baseline. You should also consider what improvements you would like to make in your research compared to this baseline. What do you want to write in the last sentence of the abstract? Examples are; “We showed system X's performance,” “We improved algorithm X’s accuracy by Y points,” and “We proved that algorithm X has property Y.” Set this as your research goal.

Then consider how you are going to prove it, or at least provide reasonable evidence that many people can accept even if it's not a logical proof. In other words, think "In this paper, we will use this logic to appeal our research results, so we need this kind of evaluation experiment." In order for others to accept the results of the experiment, it is necessary to use scientific methods. One of them is to show that there is no bias in the experimental data. If you tune a system using one set of data, you must evaluate it using another (unseen) set of data. Preparing such a data set can be difficult at times, so it's important to prepare for the evaluation experiment and to do so in advance. There are many scientific evaluation techniques. If you don't understand this area well, ask senior researchers around you.

== End of Letter ==

In fact, I learned this *back-casting* approach (i.e., writing a paper first) in my consulting training. In consulting, it is called *issue-based approach*. This is a method used in consulting which makes an assumption on the root cause of the customer's problem (*issue*) and collects evidence to test the hypothesis. If the hypothesis is correct, the customer's problem can be solved with minimal effort without unnecessary detour. Of course, as you proceed with your consulting, you may find that your hypothesis is wrong, so you have to correct it in such a case. The idea is the same for research: being aware of the final form of a paper will guide your research in a most efficient way.

Another tip for "I am running around with a deadline"-type researchers is to voluntarily set and declare deadlines. For example, declare to the people around you, "I will submit a paper to an international conference XX". Declaring deadlines that you set on your own, rather than those that others have asked you to do, gives you a stronger motivation (at least in my case). In fact, when I wrote this book, I told Mr. Koyama, the editor of the publisher, "I will complete the manuscript by the end of August 2019." I knew it would make my situation worse, but that's how I could drive myself into a fiery situation to meet the deadline.

Researchers have a desire to achieve better results. Academic researchers such as those in universities may be allowed to take time to produce the best results, but corporate researchers must always produce results within a limited amount of time. You may be hesitant to hand over the results that are only about 80% complete to the business divisions and customers, but I think sometimes you have to compromise.

2.4 Make Value Out of Research

Research in a company must ultimately bring some value to the company. Of course, there is more than one exit for good research. You can present a paper at a well-known conference, come up with a product idea that can change the world even with a small number of papers, contribute to a large business by solving a customer's very difficult problem, radically reform an internal business process, create an international standard that can lead an industry, or contribute to open source.

Technology Transfer

In order to directly link research results to business, it is necessary to know what kind of business the company is doing. When I was at IBM, almost half of IBM's revenue came from services, such as consulting, systems development, and outsourcing. Therefore, the contribution of the research division to IBM services was a priority. Canon's main businesses are B2B products such as multifunction printers and consumer products such as cameras. Therefore, the contribution of the R&D division in Canon is mainly to the business units which develop and manufacture Canon products. PFN is a young company which conducts joint research and development with customers such as Toyota and Fanuc, and looks for the establishment of a new business model with various strategic partners. Both business units within the company and external entities are considered as *customers* as long as they use the research results.

What customers want is not "whether a proposition is true" in most cases, but to construct a system to solve their problems such as "build a system to automate X" and "create new production processes that increase productivity." Therefore, the research in an enterprise often involves an engineering problem.

As mentioned in Section 2.2, there is an infinite number of engineering solutions. Deciding on the solution that provides higher value to the customer depends on the customer's sense of value. What is often difficult is that the customer may say they want something different from what they really want, and sometimes there is no consensus within them.

In software engineering, the activity to extract what the customer wants used to be called *requirements definition* or *requirements elicitation*. Recently, it is often called *requirement development* instead. It is rare that customers know in advance what kind of system they want to build. The assumption that the customer has requirements and all we need is to extract them turned out to be simply wrong. They do not know what they want to build, and software engineers have to co-develop the requirements with the customer.

In requirements development, it is most important to repeat the dialogue with the customer. If your system will be used in a factory or retail store, you may want to do some field work. Recently, the concept of *design thinking* is spreading. Design thinking is a methodology to decide "What to make?", and it is better to think of it as a tool box which contains many tools (or methodologies) rather than a theory. One of the tools in this toolbox is *observation*. Instead of talking about the customer's problem by language, you ask the customer to show how they actually do business in the field. The customer does not explicitly state things that are too natural to them. However, for those who do not know the customer's business, there will be many "Why?" questions when actually looking at the process. In order to capture such implicit requirements, it is best to visit the work site.

Another important design thinking tool is *prototyping*. Customers may not be able to specify what they want at first. But if you make something that works, they will say "This is not something that I had in my mind" or "Can't you change this a little more?" The best way to capture this type of requirements is to prototype a working system with as little effort as possible. Recently, in software development, *agile development* (which is a method for creating a system that works within a short period of time, to develop requirements), is becoming the mainstream. The company mission of PFN is to "Get the latest technology to customers in the shortest possible amount of time." Thus, prototyping is essential for us.

In order to deliver the latest research results to customers, there is one more thing to be careful about. Make sure that customers understand what new technologies can and cannot do. Recently, there is a tendency to call a system using machine learning or deep learning "artificial intelligence", but I think this is very dangerous. This is because the word "artificial intelligence" is easily interpreted as "a machine that thinks like a human" and may cause customers to have excessive expectations. Deep learning is certainly a great technology and has great potential. Deep learning is enabling many things that have not been possible until now. However, it does not mean that the intelligence of the same level as human, so-called *general artificial intelligence*, can be made by deep learning. When customers use the word "artificial intelligence", you should carefully determine what exactly they meanand convey what your technology can and cannot do.

As an example of technology transfer, let me mention the PFN project which I have been involved in recently. Data Stadium Co., Ltd. is a company whose main business is to collect data from sports such as professional baseball and football, to distribute them to media and sports teams, and to offer their analysis. Hakuhodo DY Holdings Co. and PFN established a joint project for the purpose of creating new value by combining the data of Data Stadium Co. and the machine learning technology of PFN, in 2017. The initial PFN team consisted of an expert in image processing, an expert in anomaly detection, a business development person, and myself. In the first few months, we brainstormed on what kind of system to make. The customer explained what kinds of sports data analysis problems exist and what kind of data exists, and the PFN introduced the latest deep learning technology and application examples. Also, in order to observe the concrete process of data acquisition and post-processing, we went to a stadium to watch a football game and also watched the operators working on data preparation and correction (observation). During the problem search, we made some prototypes based on actual data and received customer feedback.

After this process of requirements development, we finally decided on analysis in football. In baseball, the flow of the game can generally be expressed by following individual discrete events, such as pitchers throwing or batters hitting, but in football, the play is continuous and the whole series of movements of the 22 players on the pitch, including players who do not have the ball, determines whether the situation is good or bad. This type of problem with many variables (all of which should simultaneously be taken into account, to be able to make inferences) is exactly what deep learning is good at.

After the concrete research topic was decided in autumn of 2018, the project was started by assigning researchers. There have been many statistics being collected in football. How many shots a player has made, how many meters he has run during the game, and what is the probability of a successful pass. However, these are only the information about the player who is directly involved in the ball, and the analysis of the player who does not have the ball is seldom carried out. Though it seemed to be the optimum application for deep learning, there seemed to be no example of such analysis using deep learning. The team set up hypotheses about how to use deep learning, prioritized them, and examined them one after another. Finally, a good technique was found, and a new product incorporating the technique was announced in 2019.

It took almost two years from the first meeting to launch the product. We met with the customer every week in each phase of requirements development, hypothesis generation, and hypothesis verification to confirm the current situation and mutually agree on each other's intentions. At that time, we communicated the technology correctly, and the customer understood what we could and could not do. I think this led to smooth technology transfer.

Indirect Contributions

The world of image recognition was shocked in 2012. In a contest called ImageNet, a team of researchers led by University of Toronto Prof. Hinton has risen to the top of the list with significantly improved image recognition. Deep learning was used for the first time. Toru Nishikawa and Daisuke Okanohara, who had grown a company called Preferred Infrastructure (PFI) little by little by themselves without any outside capital, decided to bet on the potential of this technology and started a new company, PFN, in 2014. In the meantime, the technology of deep learning was advancing rapidly. The researchers and engineers of the new PFN confirmed these possibilities by reviewing successive papers published on the preprint server arXiv[8].

In the spring of the following year, 2015, an engineer, Seiya Tokui, was struggling with the tools that many researchers were using at that time. The tools were difficult to use and it was difficult to reproduce various ideas. A deep neural network can be regarded as a computational graph. At that time, it was common for tools to describe and execute computational graphs in a graph definition language specifically designed for them.

[8] A preprint server is a server on the Internet that is prepared to widely distribute manuscripts of papers before they are submitted to academic journals or international conferences. https://arxiv.org/ is a famous one.

Therefore, the structure of the calculation graph had to be fixed before programming. He realized that execution process of a Python program itself can be regarded as a computation graph, and he devised the idea that construction and execution of a computation graph can be carried out simultaneously in a Python program without using a separate graph definition language. This makes it possible to dynamically change the computation graph with the given data (he called it "define-by-run"). He implemented the backbone of this idea during Golden Week that year. Three others - Yuya Unno, Kenta Oono, and Ryosuke Okuta - joined this project, and the development was accelerated. This effort resulted in the deep learning framework called Chainer.

Chainer is an epoch-making technology in the sense that a computation graph can be dynamically constructed. Internally we discussed how to align this advanced technology to the company's values. One idea was to keep Chainer in the company as a differentiation factor of products and services of PFN, another was to make it open source, in order to make PFN a player in this field. Okanohara chose the latter, and Chainer was released as open source software in June 2015. It was five months before Google released TensorFlow as open source.

Chainer has been widely used by researchers all over the world, especially those who need dynamic computation graphs, and accordingly it has gained recognition. It has become a matter of fact that researchers who participate in the international conferences on deep learning know Chainer without knowing the company named Preferred Networks. Many applications come from researchers across the whole world who carry out research and development in deep learning. Attracting excellent researchers is vital for PFN, which in turn enables PFN to differentiate by its technological strength. Okanohara's decision to open source Chainer was right as it improved the PFN brand amongst researchers.

Wrap-Up Your Research

Having experienced three research organizations with different organizational climates (IBM, Canon, and PFN), I feel that many corporate researchers do not have a habit of wrapping up their own research. To wrap up a research project is to make the findings of your study available to others, and make them the basis for the next step of research in some way. It does not matter whether it is in the form of a paper, a product, or a standard or open source. In order to do so, it is necessary to clarify under what conditions your research results are effective, what can be done at this time, and what problems remain. It is not necessarily important to publish a paper, but because a paper includes such items in its form and must be written objectively, it is easy for readers to understand. Such "communicative power" should naturally accompany a technical paper. And, by wrapping-up your work, you may be able to point out what improvements could be possible in the next steps of the research. This "communicative power" is exactly why I encourage people to get a Ph. D. In order to write a doctoral dissertation, it is required to wrap up not only a single research project but also several years of cohesive research projects grounded through a larger perspective. I think that's why people with Ph. D. degrees have this "communicative power".

Research takes time and money. Be careful not to do a research project without wrapping up the work.

Chapter Summary

- Research consists of three steps: the entry, main body, and exit.
- Choosing good problems is the first step in good research. Refine your sense of problem selection and your research approach (research taste).
- Learn how to solve problems.
- Familiarize yourself with scientific inference methods, especially statistical inference methods.
- For improvement-type research, the baseline and upper bound should be clarified.
- Consider how to allocate priority to your research results in advance.
- Actively engage in dialogues with customers so that they can accept the research results.
- Learn how to wrap up your research.

Chapter III: Communication

In the previous chapter, we discussed the importance of the ability to propose and conduct research, along with the ability to bring the research results to the impact. Whether the research proposal is adopted by your customer - be it your boss, business unit or external customer, you need to communicate with them. No matter how good an idea or research result is, they can't buy it unless they understand it. Publishing a paper or presenting at a conference is also a form of communication. In addition, discussions with other researchers and people outside the field are also important for advancing the research. Good communication is therefore an important skill for researchers. I myself have always tried to improve my own communication skills.

In this chapter, we will look at various forms of communication that are useful to researchers.

3.1 Write Papers

Writing papers is an important part of a researcher's job. A paper does not necessarily have to be presented orally or published in a journal. Some companies do not encourage the publication of research results. However, it is invaluable to summarize your research results and leave them in the form of a technical paper, even if you do not publish them with an outside academic society. Unfortunately many corporate researchers do not seem to have the custom of writing papers. I have repeatedly stressed the importance of writing a paper -- here are my thoughts summarized in my letter in January 2009.

Letter "Why Write a Paper?"

Why do researchers write papers? Some people give a very secular reason, "In order to get a Ph. D" but that doesn't give you a reason to write papers after you've earned a Ph. D and become a full-fledged researcher. There are many opinions, but I think I have at least three reasons to write a paper.

The first is to pass on the results of research to others and to allow the results to accumulate. As I said before, research is cumulative. Some studies may start entirely from scratch, but virtually all research is developed on top of previous research. Sometimes, after a certain amount of research has been done, people try to write a paper by searching for prior art and trying to find minor weaknesses in previous research to make their own paper unique (In fact, I did a lot of that when I was young). This is, of course, the wrong attitude. Science and technology advances in such a way that prior research is examined in advance, with research being carried out in the form of extending them, and knowledge being fed back to the research community.

The same applies to the accumulation of knowledge within a company. Your research does not necessarily lead to products and services immediately. Depending on your business and environment, good technology may not have applications for the moment. But don't you want to lose your good ideas? I myself had a lot of ideas that I thought "this is great," such as methods for analyzing natural language using constraint propagation algorithms, protocols for fair transactions on Web services, XML compression using DTDs, and methods for dramatically improving authentication accuracy by combining multiple biometrics. None of them became products or services, but some of them came out in the form of papers, and others developed their research using my ideas. However, many of the documents were scattered and lost because they were not properly written. Even if there is an idea in the past that fits a customer's problem today, it cannot be used if it was not properly documented. Recently somebody filed a patent titled "Encrypting an XML document" in the United States. We looked at our records in the past and pointed out that we had already published the technology for XML document encryption. This is a good example of the importance of publishing our research, although it is not necessarily an academic paper.

Because of my job, I interact with the research managers of various companies. As I have realized, some corporate laboratories are actually paying less attention to writing papers because they place too much emphasis on the technical contributions to their products. One such manager once asked me: "Not all research topics lead to product development in a timely manner. Many research results that do not lead to the product development have been scattered and lost, and have not produced any value. What should I do?" Now you know the answer. As I said, the main purpose of writing a paper is to accumulate knowledge.

Second, it is possible to send a message to society that cutting-edge research is being carried out, contributing to a positive image of the company and organization. Talented people gather around a research lab where good papers are written. This will increase the reputation of the laboratory and the image of the company from the customer's point of view. The same can also be said about individual researchers writing papers, which will increase their reputation as world-class researchers.

Third, I think this is the most important thing for you - above all, I think you should write a paper in order to develop your own research skills. In my previous letter, I said it is effective to think about "What would you write if you could get the results of this research?" before starting your own research in order to develop your research skills. A paper is a tool for objectively, logically, and scientifically asserting the results of research. Therefore, there is a standard format of a paper with elements such as the purpose of the research, scope, methodology, method of verification of results, and prior research. I think it's a good idea to plan your research while thinking about how to assert those elements in the final paper.

You can also improve the quality of your research by writing a paper to get people's opinions about your research. As I often say, even if a paper is not accepted for publication, at least experts in the field review it and comment on it - and it's free. If you make a presentation at an international conference or research meeting, of course you can get various comments on the spot or later. It is also very good to peer review within a group before submitting a paper to improve the quality of the paper.

== End of Letter ==

Once you write a paper, many people will read it. And, even if it is not read now, it may be read in the future when everybody, including yourself, forgets about it. In a nutshell, I would say that a research paper is a communication tool that scales your research across time and space.

Types and Structures of Papers

The purpose of writing a paper is to spread ideas that were previously unknown. In my view, there are three types of papers, depending on the assertion: a paper on facts, a paper on laws, and a paper on methods, each of which has a different effective writing style.

A **factual paper** indicates that a particular phenomenon has been observed or that a particular experiment has produced a particular result. Of course, this "fact" should provide new insights as it becomes clear. A typical example is the work of AlphaGo, which defeated the world champion of the game of Go. This study reports only the fact that "We built a system like this, and we did experiments like this, and the results was this" and does not claim any particular law nor indicate a methodology that can be applied to a problem domain in general. Nevertheless, it was undoubtedly a study with a significant impact.

In factual papers, the alleged fact should take the research community by surprise. A paper on facts without a surprise is ridiculed as "Yakkou ronbun (I-did-this-and-got-this paper)". The value of such a paper is low. AlphaGo's paper did say "I did this and got this" but everyone was surprised by the results, making it a good paper.

Any paper is required to be verifiable by a third party. As you can see, reproducibility is especially important in factual papers. The paper "STAP cells were found" failed to reproduce and was withdrawn. Therefore, it is necessary to describe as much detail as possible in the paper so that anyone can reproduce the situation in which the observation or experimental results are obtained.

A **law paper** asserts a universally valid relationship between one event *X* and another event *Y*. For example, Newton's law of gravitation asserts the relationship between the masses of two objects and the gravitational force between them. Inference tools for asserting the existence of a law can be derived inductively (statistically) from a large amount of data, or deductively from a set of laws already recognized as true, as discussed in Section 2.2. In a law paper, it is necessary to base the argument on a scientifically accepted inference protocol as described in Section 2.2.

A **method paper** is to show how to achieve a given objective in a particular problem domain. For example, the paper which proposed the linear programming technique showed the algorithm for solving the optimization problem with linear constraint and linear objective function.

In a method paper, it is necessary to show logically by experiment and prove that the proposed method can achieve a given goal for any problem belonging to the problem domain. In many cases, the framing of this "problem domain" is the key to a good paper. Since it is rare that a universal problem can be solved in a single paper, the problem area must be narrowed down. However, if you narrow it down too much, it looks like a useless setting in practice. What is important here is to give one concrete and practical example that falls into the problem domain. This type of example is called a **motivating example**. Articles with a good motivating example are easy to understand and read. If possible, the whole paper can be constructed around that example, using the same motivating example not only to describe the problem domain but also to describe the method. Such an example is called a **running example**.

In communication, it is necessary to always be aware of who the audience is, and the same is true for papers. It should be noted that there are shallow and deep readers, especially in the case of papers. A **shallow reader** is interested in understanding in a short amount of time what the article is arguing about, and often reads the abstract, introductory chapter, and diagrams as well, but does not seek a deep understanding of the arguments that prove the argument. A **deep reader**, on the other hand, reviews the experiments and follow proofs carefully to verify that the claims are correct. Reviewers are deep readers, and when submitting a paper, you should first focus on accurate descriptions for a deep reader and follow the proper scientific protocol. After your paper is accepted, you should give more focus on making it easier to understand, considering that it can be seen by many shallow readers.

I often hear that it is good to read a lot of good papers to write good ones. I agree with this notion. When STAP cells became a scandal, Dr. Yoshiki Sasai of RIKEN, one of the researchers involved in the scandal was highlighted as an excellent paper author, so I read his paper. I am a complete amateur in biology, and of course I am a very shallow reader, but I felt that his paper was written in a powerful, clean, and easy-to-read style. People who are not used to writing a paper tend to cram everything they have done in their research into their paper. However, stuffing in things that are not directly relevant to the claim weakens the thesis argument itself. It is not necessary to use up the maximum number of pages for the journal. I think a good paper should be as concise as possible.

By the way, even if you start writing a paper, you may find it hard to concentrate on writing because of things like surfing the web in the background, getting a Slack message and having to do something else. **Pomodoro Method** is useful for such cases. This is a way to divide your time into 30 minute intervals and focus on writing a paper for 25 minutes with no email or Slack or doing anything else. In the remaining 5 minutes, you have to deal with urgent mails, make coffee, and then start concentrating in the next 25 minutes. Pomodoro is an Italian word for tomato and originally referred to a kitchen timer shaped like a tomato.

There are now a number of Pomodoro apps that run on your smartphone, so why not try them out?

Writing in English

Even if you are based in a non-English speaking country, many papers will also be required to be written in English. In some fields, even domestic academic societies require all papers to be in English. For the Japanese, it is in most cases difficult to write a paper in English, which is not their native language, but, as we will see in Section 3.5, writing an English paper is inevitable in order to establish a presence in the international community. What should you be careful about when writing an English paper? Of most importance, is to clarify the logic of your arguments. This is not limited to English papers, but because of the language handicap for non-native English speakers, it is even more important. There are two things I am careful about in order to clarify my argument.

One is to always write conclusions first. When we write a paper, we tend to write a lot of background and preconditions. If you write the conclusion first, the sentence becomes concise. This can be said of the whole text or within a single sentence. For example, if you want to say "Because of Y, X" you write the conclusion first, as in "X because of Y."

The other is to avoid, as much as possible, the use of sentence-leading conjunctions such as "But," "In addition," and "For example," which are common in an English paper written by a Japanese author. This may be controversial, because a conjunction makes it easier for the reader to understand the structure of the text. However, if your logic flows well, the readers should have no problem following the structure of the text even if no conjunctions are used. It is not necessary to completely eliminate conjunctions, but I found that it is good practice to first compose a text without conjunctions and then later supplement conjunctions as necessary.

Long ago, Prof. Heisuke Hironaka, a winner of the Fields Medal, known as the Nobel Prize in Mathematics, came to the Tokyo Research Lab and gave a lecture. There is one thing I remember from what Prof. Hironaka said, which was that creative works are born from a laboratory that produces a lot of papers regardless of their quality. This word made a vivid impression on me. Since then, I have been trying to (at least in my mind) write papers even if the idea looks a bit odd or the work is not yet fully proven. They are not good for the top conferences but I can submit them to local workshops and get feedback. I would like to encourage you to do the same.

Once a paper is submitted, it is peer-reviewed and published in a journal (or an international conference). In some highly competitive fields, the rate of paper acceptance falls to possibly below 30%. The peer reviewers who happen to be in charge may be dissenting, misleading, or the same idea may be presented in a previous paper. Therefore, there is no need to be pessimistic if your paper is rejected. Even if not accepted, your paper still receives feedback from reviewers who are experts in the field. You can improve them and challenge the next conference/journal. If many of your papers have been accepted in succession recently, it is a yellow signal. It may be that you are not challenging enough top journals and/or top conferences. Your papers should be rejected. A good researcher submits papers continuously without being disappointed by rejections.

3.2 Deliver Presentations

We give presentations here and there, including papers presented at academic conferences, to customers, and to other departments in the company. Presentations are a form of communication, and it is important that the presenter's ideas are correctly conveyed to the audience. How to deliver a good presentation is a very profound theme, but I conveyed my thoughts to the researchers at the Tokyo Basic Research Lab in a letter in August 2008.

Letter "Good Presentation"

I think the main point of the presentation is to make it simple and easy to understand. Mr. Kakutaro Kitashiro, the top adviser of IBM Japan, always says, "Maximum three points". A magazine I had read, *President,* had a featured article on "20 million-yen annual salary presentations". It describes what the top executives of various companies are careful about during presentations. For example, Mr. Watanabe, president of Toyota Motor Corporation, explains the One-A4-page-only policy in Toyota Motor Corporation. It seems that the process of putting your contents onto a single A4 sheet encourages your logic to be streamlined and clean. Similarly, Mr. Murase, President of Canon Marketing Japan, says, "your points In five lines." The person listening to the presentation may be the customer or the person in the business department who examines the proposal. Since the audience is very busy and listens to you, you should say the most important things first. In that sense, Mr. Son, the president of Softbank, may be right that your presentation is "in the first 10 seconds". Engage your audience with the first word, and if you can do this, your presentation is a success.

The first thing I remember from the presentation training I received when I joined IBM was that "Try to direct the audience's attention to yourself, not to the screen." In other words, sell yourself rather than your contents. In the same article feature in *President* Mr. Kobayashi, president of Itochu Corporation, said that when he listens to a presentation, he watches "the level of enthusiasm and seriousness of the presenter rather than the contents." Presentations ultimately need to get the audience to act differently. In order to do this, you need your audience to understand the content of the presentation and to convince them that it is indeed a good idea. But can an hour or so of presentation really change the way people think? If you can't change their thoughts easily, isn't it best to make the listener think "I can believe what he's saying"? In this sense, "the presenter's own passionate belief in the content" is more important than the simple and easy to understand content in a presentation.

== End of Letter ==

I wrote this letter over ten years ago, but I still believe that presentations are more about selling people than the content. I think there are two types that you want to communicate: **Impression** and **logical content**. Unfortunately, presentation is good for conveying impression, not content. After my presentation, often a member of the audience would say to me, "it was very interesting," but only to discover that they really understood little about the logical content of my presentation. If you think about it, that's obvious, because the audio-visual media are not good for delivering logical content. As you can see from your experiences at Math classes with a lot of formulae on a blackboard, you must use a pencil and paper to understand the logical content. Whether you like it or not, it's impossible to get the content across in a 30 minute or so presentation.

So I believe it is ideal to combine presentations with documents. The paper presentation in the academic conference can be said to be exactly in this ideal form. It is a form in which both the presentation and the document work together, in which the impression of "This research seems to be interesting" is conveyed in the presentation, and the paper is for its logical contents. I am often asked to give a public lecture that is not a paper presentation, but I try to deliver a written document as much as possible along with the presentation. It is hard to prepare both the presentations and documents at the same time, so it is not always possible but I try to distinguish between impressions and contents.

Slides as Documentation

If you really need to convey the impression and the content at the same time in one preparation, you have to make a document-like slide. With the recent proliferation of slide sharing services[9], it is likely that your presentations that made an impression will be reviewed later to better understand the content. Document-like slides are those that allow you to follow the content correctly without having to listen to the presentation.

When I was in the IBM Consulting Group, it was a requirement to make document-like slides with a lot of text. As a consultant where the cost is measured by person-hours, the luxury of preparing the same contents proposed to the customer separately in slides and documents is not allowed. Therefore, it was required to make a document-like slide that is understandable without listening to the presentation. One technique for this is the idea of "Vertical Logic, Horizontal Logic".

Each page of the slide should contain a key message (the **bottom line**). **Vertical Logic** means the bottom line is expressed in a sentence of maximum three lines, written as the page title, and the page must contain only enough information to support the title (bottom line). Writing your bottom line in a single sentence forces you to be clear about what you want to say on the page. For example, look at the next slide I made in 2002 as a bad example.

[9] For example, SlideShare (https://www.slideshare.net/).

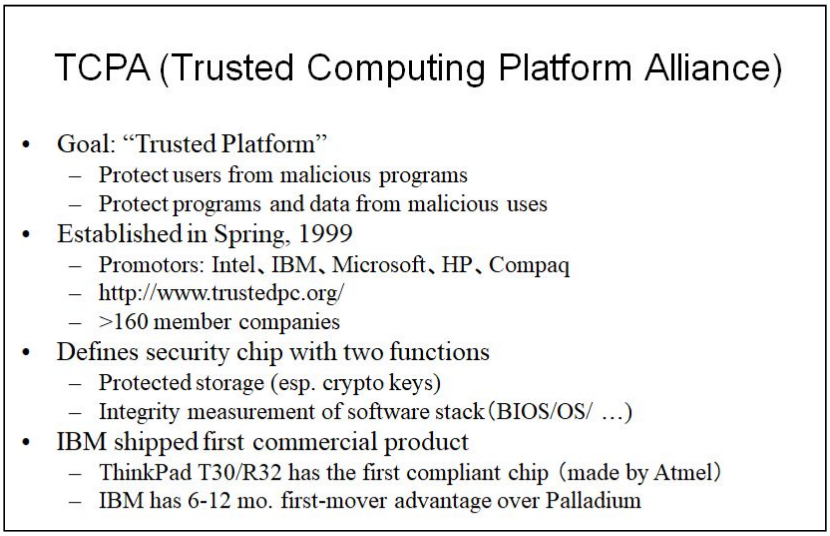

Figure 5. Bad example of document-like slide

What did I want to say on this slide? I would have argued at the time that "About TCPA". But that's not the message. The easiest way to determine if it is a message is see if the sentence has a verb. For example, "TCPA advocates a new concept of 'Trusted Platform', which is realized through a chip called TPM" or "With its first commercial offering, IBM has an advantage over the competition". In this way, the bottom line becomes clear. That is, in this example, there are two claims on one page, so I should have divided it into two pages as follows:

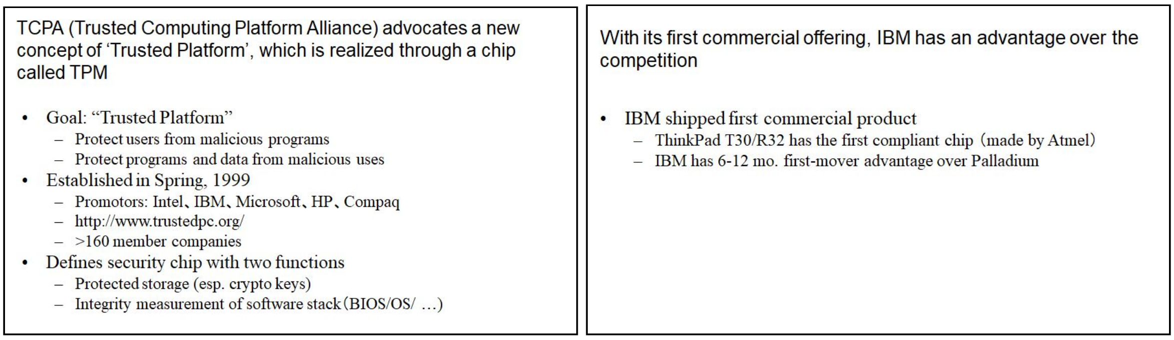

Figure 6. Better slides with Vertical/Horizontal Logic in mind

It is important to be aware that the contents of the page support the title sufficiently. If "With its first commercial offering, IBM has an advantage over the competition." is a bottom-line message, the first three items on this page do not support the claim. When information unrelated to the bottom line is included, the listener's impression of the bottom line is reduced. We tend to cram a lot of information into the slides, but the idea behind Vertical Logic is to focus on what you want to say in a single page.

So how do you combine these pages into a whole presentation? Again, the fact that the title of the page is in a sentence comes into play. All you have to do is read the titles (sentences) of the slides in order from page one to the last page, and check if these sentences are coherent. This is called **Horizontal Logic**. This way, even if the audience doesn't hear the presentation, they can follow the logic flow by looking at the slide.

Unfortunately, a document-like slide is also text-heavy and cannot be effective in making a good impression. The book *Presentation Zen* [Ref. 8] written by Gur Reynolds sums it up. I now realize that it is not a good idea to require document-like slides. So I don't make slides based on Vertical Logic/Horizontal Logic anymore. If you are required to prepare document-like slides, you should add text to the speaker notes for the slides that will help the audience understand them without having to listen to the presentation[10]. There are different ways to convey impressions and contents. You can not kill two birds with one stone.

Practice, Practice, Practice

So the contents are to be conveyed in the attached document. How can you give a good impression? I think the most important thing is to practice. If thirty people listen to your one hour presentation, you're spending a total of thirty hours of people's time. If the cost per one employee per hour is 10,000 yen, your presentation must provide value of at least 300,000 yen to the company. If you're not prepared enough to communicate effectively, you've wasted that much money. So it's your responsibility to be ready to present. The preparation of slides is only part of the preparation. The real preparation is the practice of the presentation. Noted for his impressive presentations, Steve Jobs reportedly practiced aloud over and over again. When you practice, you tend to stutter where your thoughts are not well organized. The fact that words do not come out smoothly means there is still room for improvement in the composition. If you find it difficult to do it alone, ask someone to help you. The audience can be a family member or a stuffed animal. It is important to practice assuming that the audience is there.

For examples of good presentations, listen to TED Talks. Why do all the TED Talks resonate? When a TED speaker is chosen, TED organizer (curator) Chris Anderson will take the time to work with the speaker to refine the content. In addition, a speaker practices over and over again to make such a compelling presentation. The TED-quality presentation is always my goal.

Questions and Answers

At the end of a presentation, there is often a question and answer session. Many people seem to have trouble answering questions at academic conferences, especially in English. During the question and answer session, let me tell you what I am careful about.

First, thank the questioner and repeat the question as in “Thank you for a very good question. The question was...”. The repeating part "The questions was ..." must be

[10] For example, see my slides at the 2019 annual conference of Japan Society for AI (https://www.slideshare.net/hiroshimaruyama14/jsai-162894400). All pages are annotated with speaker notes so you will be able to understand them without listening to the presentation.

addressed to the entire audience. This has two meanings. One is to inform the audience as a whole of the question, since the questioner's voice is not always heard throughout the room. The other is to pull the question into your turf when you repeat the question. Repeating the question does not need to be exactly the same as the original question. All you have to do is to tell them "I interpreted the question like this." At times, the questioner may state his/her opinion at length, and you do not understand the purpose of the question or because of the questioner’s strong accent. Even in such a case, if there is at least one keyword in the question you can pick, you can repeat as in "I understand that the question is about XX" and state your thoughts about it. It is important to focus on the questioner when the questioner is asking the question, but to remove eye contact from the questioner and speak to the whole audience when repeating the question. By doing so, you will give an impression to the attitude that you are trying to make the question and answer be a beneficial discussion for the whole audience, not just for the questioner.

The time spent on the question and answer session is very precious because opinions of researchers in the field can be heard. It's a shame to use up your presentation time before the questions and answers. Leave as much time as possible for questions and answers and make it a useful time.

3.3 Discuss

Unlike universities, where each researcher has its own research theme, corporate research is usually conducted by a team. In addition, there is communication with business divisions expecting research results, and integration with the technology of other teams necessary for the final products. Therefore, the communication in the office is indispensable for the execution of the research. Let’s consider various forms of internal communication, such as meetings and online exchanges.

Meetings

I think the culture of an organization is often reflected in the way meetings are conducted. In my experience, IBM had a culture that is flexible at best and loose at worst. There were many people who were late for meetings or who were absent without notice, and it was normal to leave the meeting saying "excuse me, I have the next meeting" even if the meeting had not concluded. On the other hand, there was a tendency to discuss important matters at the end. At Canon, the meetings were better organized, with attendees and agendas firmly fixed (at the meetings I attended, even seat assignment was fixed). Above all, it was comfortable for me that all the meetings started on time. Making someone wait at a meeting is a waste of their time. Being late for a meeting shows lack of respect for other people's time.

In the Institute of Statistical Mathematics, which has inherited the culture of the time when it was a part of the Ministry of Education, Culture, Sports, Science and Technology, the clerical staff prepared the materials for the faculty meeting in advance, and a thick bundle of papers was placed on the desk at the seat assigned to me. The length in minutes for each deliberation item was strictly scheduled, the conference proceeded smoothly, and it was rare that any substantial discussion occurred. The actual discussions had happened elsewhere prior to the faculty meeting. The duplication may seem inefficient, but the fact that government offices follow this style of meeting has historical significance. In modern Europe, when the dictatorships collapsed one after another and were replaced by the world of democracy, involving all parties in decision making and documenting every one of them was the way to prevent another dictatorship. That is **documentarism**, which makes all decisions based on documents[11].

In the start-up company, PFN, meetings are very different. Paper documents are never used (at least in the company's internal meetings). Everyone gets together in a conference room with a computer and sees the minutes being created in real time on the computer screen in Google Docs. Decisions are made on the spot and immediately executed. Everyone looks at the screen during a meeting, so it's rare to see other people's faces during a meeting. I had been taught that eye contact is important in face-to-face meetings so I was uneasy at first, but I think this is a meaningful style.

Each organization has its own style, and each has its own merits and demerits. Google's Re:Work provides a variety of tips on how to run your organization well. Of those, what I think is particularly important at meetings is that everyone feels psychologically safe. **Psychological safety** is the sense of security that you are allowed to say silly things[12] [11]. Haven't you ever thought about what you said, "Is it suitable for this place?" and didn't say it? Is there something that everyone knows, and only you don't, that you can't ask again? Don't you think everybody considers you weird if you say this now? If you feel anything like that at a meeting, it means that the meeting lacks psychological safety.

One of the things I try to do for psychological safety at meetings is the principle of **equal voice**. If there is a participant in the meeting who has not yet spoken, they should be encouraged to do so. Even if you are not a moderator, you can say "What do you think about this matter, Mr. XX?" or something like that. If the meeting is face-to-face, pay attention to the **non-verbal signals** of the participants, such as nodding, frowning, or looking up abruptly. These nonverbal signals indicate that the person has some idea about the discussion.

After all, a meeting is a place where people with different ideas exchange opinions and, in some cases, make decisions. If everyone has the same idea, you don't need a meeting. It is therefore the first step in a meaningful meeting to identify the differences of opinion of the attendees. If the attendees aren't able to express their thoughts, they won't be able to see

[11] I learned about documentarism from the book [Ref. 9] by Noriya Usami, an ex-elite career bureaucrat at the Ministry of Economy, Trade and Industry.
[12] For more information, I encourage you to watch Amy C. Edmondson's TED Talk (https://www.ted.com/talks/amy_edmondson_how_to_turn_a_group_of_strangers_into_a_team/).

where the differences lie. I want the meeting to have an atmosphere in which everyone can say something stupid and it is acceptable.

I have a memorable experience with the way a meeting went. I had a discussion about smart cities with General Electric (GE). GE has a unique meeting method called **Workout** [Ref. 10]. A Workout has an assigned "Sponsor Executive" who is responsible for implementing what are agreed in the Workout session. Sponsor executives, however, cannot participate in the discussion itself. At the end of the workout, the participant reports the agreements to the sponsor executive, who then makes a Go or No Go decision on the spot. If the decision is a Go, the sponsor executive has full responsibility for implementing the proposal.

There are a number of other points that made me think "I see," such as facilitating discussions by facilitators trained in workout techniques and careful preparation, but I was most impressed by the fact that as soon as the leader of the workout declared "I will start a workout now," all GE employees in attendance leaned forward as if the switch had been turned on and lively discussions pursued. This was good evidence that the workout methodology is firmly rooted in the GE culture. There are various methods for the meetings, but it is meaningless if you do not use them regularly. I think the meeting style is rooted in the culture of the organization.

Online Communications

The IT-based communication tools are indispensable in order to maintain a flat and open organization like PFN. As of this writing in 2019, Slack seems to be widely used as a chat tool. A lot of information exchange, discussion, and decision making are done on Slack in PFN, which helps to make quick decisions. PFN has more than 1,000 Slack channels for technology, projects, in-house general affairs, and information systems. All channels are accessible to all employees, except for certain sensitive topics such as human resources. If you are interested in a project that you are not involved in, you can follow the channel and state your ideas. When discussing a new strategy for a company, the founders Nishikawa and Okanohara suddenly appear in the channel and make a decision on the spot, saying, "Let's go with that." (Figure 7).

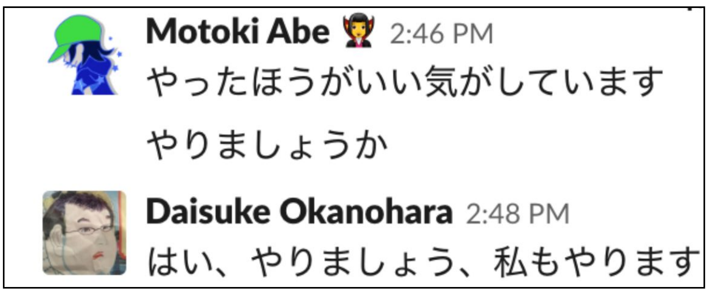

Figure 7. Example of Slack Decision Making in PFN

Slack also allows you to set up private channels with limited members. Private channels must be used for human resources topics and projects that have to limit the scope of disclosure based on confidentiality agreements with customers. However, if the number of private channels increases too much, it may compartmentalize the information flow in the company. You should watch such signals.

PFN Day

Slack is an excellent communication tool, but if time and space allow, it is desirable to have face-to-face communication. Many companies regularly hold in-house events to promote interaction among their technical communities. It was called *Innovation Day* in IBM and *Open House* in Canon and the Institute of Statistical Mathematics. In PFN, as the company grows, some forms have been tried, and what is being done at present is a whole company event of 1 or 2 days called *PFN Day*. It includes keynotes, several parallel sessions, and poster presentations. Poster presentations are my favorite style of discussion, and it's great to be able to discuss them directly with researchers. New ideas are born out of these interactions (Figure 8).

Figure 8. Discussion on PFN Day

PFN Values Week

In April 2018, PFN held an intensive one-week discussion by all employees to determine the values shared by them (*PFN Values*). PFN Values is an attempt to express in our own words what PFN is, which, in turn, determines our code of conduct at PFN. From a week-long discussion, four values "*Learn or die", "Proud, but humble", "Boldly do what no one has done before",* and *"Motivation driven"* were defined. Let me tell you how this discussion was designed and implemented.

First of all, there was a principle of everybody's participation. By building the values from the bottom up, employees can see the resulting values as their own, not something that is given by the top. An example of a bottom-up discussion in which all employees participated in determining a company's values was *IBM Values Jam,* in which IBM determined its current *IBMer's Values* through a 72-hour online discussion, in 2003. The situation at that time was described in detail in *Harvard Business Review* [Ref. 11], but thanks to almost 50,000 employees worldwide exchanging their true feelings, IBM was able to delve seriously into the question of "What are our values?". We designed PFN Values Week by looking at similar examples of other companies.

As is the case with this kind of argument, the process consists of two phases, divergence and convergence[13]. In the **divergence phase**, it is required to gather as many ideas as possible. It is in this phase that everyone needs to participate. On the other hand, the **convergent phase** is the process of assembling a large number of emerging ideas into a small number of messages, which must be done intensively by a small team.

In the divergence phase of *PFN Values Week*, the following three activities were performed for one week.

1. Face-to-face discussions with randomly assigned six person teams.
   As a mechanism to ensure the principle of everybody's participation, we assigned all employees to one of the six person teams and asked them to have at least one 1-hour discussion during Values Week.
2. General discussion by Slack
3. Brown-bag lunch meetings

A moderator was assigned to each discussion with the following instructions:

- Extract as many ideas as possible from the participants. To do so, do not speak yourself, but concentrate on listening.
- Don't criticize the comments that come out. Don't allow criticism.
- If a participant is unsure what to think, ask questions such as:
  - What is the value of a PFN? What is the value to society, yourself, and your customers?
  - How is PFN different from other companies?
  - How did the PFN look from the outside? (this was especially useful with recently hired employees)
- Share the discussion summary with the main Slack channel immediately after the discussion.

This was followed by the convergence phase, where the core team took a bird's-eye view of all the opinions, picked up common themes, and expressed them in writing. In particular, we paid close attention to the wordings so that messages could be conveyed clearly. Finally, after review by Nishikawa and Okanohara, four values of PFN were decided.

[13] It is similar to the fact that research has two phases: hypothesis generation and hypothesis verification.

This kind of discussion leads to thinking about "What should the company do?" across teams. I felt that this process was also an important exercise in addition to defining the PFN values themselves.

3.4 Negotiate

Negotiation is an important skill for corporate researchers. Whether the proposal of the research is adopted or the research result is accepted by the business division and the customer is often decided not only by the content of the proposal but also by negotiation.

In this context, "Negotiation" is not about imposing your ideas on others. It refers to the joint work with your partner to find a better solution *C* than *A* or *B* which are the original proposals from the individual parties. In other words, it is a creative process to find the best choice for your customers or divisions by incorporating your own proposal into it. If you don't stick to your original idea and are able to come up with a proposal that the other party didn't expect but is more advantageous to them than their original ideas, your proposal will be more likely to be accepted.

What is important in negotiations is to understand the other party's true goal (unstated in many cases). Do they really want what they're saying? When the customer says "Your proposal lacks the function A, so I cannot adopt it.", what the customer really wants is the function A? Or is it because the customer's boss has a personal connection with the sales of company B and has already decided to use the solution of company B, so he just uses function A as an excuse to refuse? If the latter, then negotiations such as "If you give us three more months, we can implement function A" would be meaningless. In that case, it is better to consider other ways such as negotiating with company B and asking them to use your proposal as a part of company B's solution.

Another important aspect of negotiation is to always be aware of **BATNA** (Best Alternative To Negotiated Agreement), which is the best alternative if the negotiations break down. If you understand your BATNA and your party's BATNA, and if you can make a better suggestion than either of them, you and your party will be happy. It seems to be a simple matter, but it is difficult to think about BATNA calmly at the negotiation table. If negotiations get bogged down, take a step back and think again, "What is the opponent’s BATNA?" or "What is my BATNA?" In the above example, your customer’s BATNA is to use the solution of company B. Therefore, if we can add some value to the solution of company B, there is a possibility that the proposal will be accepted.

For more on negotiation, see George Kohlrieser's book [Ref. 12]. The author is a former hostage negotiator, a professional negotiator in hostage crisis management for the Ohio State Police (now, as a business school teacher, he teaches a leadership course to corporate executives). In a hostage situation, the negotiator makes a mental connection ("bond") with the criminal through dialogue and finds out the true feelings of the criminal. The negotiator then gradually turns the criminal's attention away from hurting the hostages toward achieving his real goal. It is said that the author himself became a hostage four times as a result of exchange with a hostage, and succeeded in negotiation each time. In hostage cases, professional negotiators have a 95% chance of rescuing the hostages without damaging them.

It's hard to turn someone's interest toward achieving a real goal when they're emotional and stick to their point. One of the techniques in this book is to give the other person a choice. Instead of saying, "Go without delay" when your child is fretting over going to school, ask, "Do you want to wear light blue today? Pink?" By giving the option like this, your child pays attention to the independent judgment of "What clothes do you wear?" from the passive subject of "I don't want to go to school." which is the subject of the emotion. This technique may be useful not only for work but also for parenting.

Let me tell you more through my letter on practicing negotiation from my own experience.

Letter "Understanding Your Party"

What is the purpose of communication? Ultimately, you want the people you are communicating with to behave differently as a result. If so, isn't it extremely important to think about how the other party thinks when communicating? I received training from a consultant, Kanji Okubo, last year (note: this training will be discussed again in Chapter 5). Among the things that Mr. Okubo taught me, what impressed me the most was the difference between persuasion and acceptance. No matter how much you try to **persuade** people by reason, if they don't **accept** what you say, they won't be able to act. Are we researchers trying to persuade others with logic? If your suggestions don't work, they probably won't. If so, wouldn't it be helpful to put yourself in the other person's shoes and think about what the problem is?

One technique I sometimes use is to let someone to come up with the idea that I want them to do. It's not difficult. Instead of saying "Please use technology X to solve this problem", ask "What would happen if you use X in this problem?" Your party will then wonder if he can solve the problem this way. When he starts to accept, "Yeah, maybe I can" even though the first hint came from me, he'll think it is his own idea. In many cases, researchers are obsessed with their own idea so they tend to accept ideas if they think that it is their own idea. Sometimes you can get something bigger by giving away your idea.

Another thing I try to do is to make the other party underestimate me, especially when I first meet them. If they think "this guy knows nothing," they will lower their guard and tell you a lot. I think it's important to ask questions innocently and get as much as you can from them. This is, of course, a double-edged sword, and I don't want people to underestimate it and think, "It is no use talking to him."... I think the secret to communication is to have an intelligent conversation and extract as much information as possible to understand the other person.

== End of Letter ==

3.5 Cross-Cultural Communication

Speaking of communication, cross-cultural communication is especially a big problem for us Japanese. The average TOEIC score of IBM Tokyo Research Laboratory exceeded 800. I think this was very high for an organization of nearly 200 people in Japan, but it is not always enough. In PFN, about 10% of the employees are foreign nationals, and the main communication including the general meeting is carried out in English. I myself have a hard time in English every day. Recently, the number of researchers who have had overseas experience while young has increased, and the younger generation seems to have less trouble than us, but there is still a long way to go. However, it is not only the English ability that is important. The key is whether you can make your presence felt in cross-cultural communication. To do so, it is necessary to appeal with who you are first.

Looking at many Japanese researchers, I often feel that you are nothing if you are invisible. Humility is a virtue, but silence produces nothing. If you have valuable information or ideas, it is your duty, your job, to inform others of them. It is not a matter of personal character or preference. At the same time as being themselves, they have to work to be "visible to others" in order to gain recognition in the technical community and to become high-level engineers in the future.

There are two things you can do to get started. One is to put a photo and an English profile on a page in the company's web directory. I think every company has such a system now. When someone in your company wonders "Oh, who is this?", she looks up the directory first. The content should of course be in English. The other is to use a public directory for external reference, such as LinkedIn. If you are a Japanese researcher, I also recommend putting your profile on ResearchMap. Researchers must keep track of their work. LinkedIn and ResearchMap are good tools for that.

It is true that "Invisible means non-existence" but I think the challenge for Japanese in particular is communication with foreigners. I have a lot of experience of communication difficulty between different languages and cultures. Even if I understand the words, it is still a big problem for me to insert my words in a heated discussion in a timely manner. Perhaps the best way to overcome these communication problems is to be immersed in the culture. Regarding my own experience, in 1993 (ten years after joining IBM), I spent four months at T. J. Watson Research Center, in New York. I think that is when my English improved the most (I had no chance to use Japanese every day because I was away from my family and there was no Skype available at that time).

If you are still a student and have the chance to go abroad for a few months, please seize the chance and experience cross-cultural communication.

How can we make a presence in cross-cultural communication? I often hear people say, "The Japanese are so modest that they are always at a loss". I don't necessarily think so.

Being modest isn't all bad. Dr. Makoto Murata, who is famous for standardization of XML, is generally modest but highly trusted in the international community of standardization. Especially, when problems of international character set and encoding appear, foreign experts ask, "Mr. Murata, what do you think of this?" He has more presence than foreigners who tend to talk a lot of thoughtless opinions. First, you should understand that if you have a good idea or important information, someone who can't get it out of you is losing. So being modest is never a bad thing. What is important is to make a presence by giving opinions that make people think so much and providing information that everyone needs. It is not achieved only by improving English. Being talkative is one thing. I think it is more important to be able to provide useful insights and to be trusted. That leads to a presence.

On cross-cultural communication, I will conclude this chapter with a blog I wrote in 2010.

Blog *Cartel of Mind*

Professor Emeritus Hisashi Kobayashi of Princeton University wrote a message on his blog that Japan's intellectuals should be more internationalized. It introduces the book [Ref. 13] by Ivan Hall titled *Cartel of Mind*. It is so critical that it made me feel bad about how closed Japan's intellectuals, especially those in the law, journalism, and higher education (academia), are to foreigners.

From the Japanese point of view, the content is very shocking. I worked for a long time in an American company that is at the forefront of globalization, and now I work for one of the leading global companies in Japan. However, in both organizations I often felt the closed nature of Japan and it still exists today. It seems that the world is definitely moving toward globalization, but unfortunately, Japan may be neglecting its efforts towards globalization these days.

Why did it happen? First of all, I have to raise the language ability of the average Japanese person. Like it or not, the common language in the world is English. Therefore, communication skills in English are indispensable for us to be active on a global basis. Professor Kobayashi points out in his blog that Japan's TOEFL score is the lowest among 27 Asian countries. According to the 2008 TOEIC Newsletter, the average TOEIC score for Japanese companies in 2008 was 456 (909 companies and 55,375 participants). On the other hand, large Korean companies, such as Hyundai and LG Electronics, require a TOEIC score of 800 when they hire, so we have to realize how poor our English is.

For many Japanese, speaking English is not easy. This includes me. I think I speak English better than the average Japanese person, but I still consume a lot more energy when I speak English than when I speak Japanese. Like many Japanese, my English was not acquired when I was a child. For example, I cannot tell the difference between the sounds made by pronouncing L and R. Try listening to "right" and "light" or "ray" and "lay" in an electronic dictionary with speech synthesis. They sound exactly the same to me. I have to use the context to infer the right words while concentrating on the discourse. For those of you who have been listening to English since childhood, these sounds are naturally different. English is a big handicap, but we have to get over it.

However, there seems to be a bigger problem beyond our language ability. It's a matter of the decision-making process. In *Cartel of Mind* Ivan Hall repeatedly pointed out that foreigners who speak Japanese fluently are intentionally blocked from making decisions. Foreigners are very welcome when they come to Japan as customers, but they are not perceived well as soon as they step into the decision-making process. Why is this happening?

Recently, a young engineer asked me, "Why do higher managers try to avoid having many stakeholders at the beginning of the argument?" Often in Japanese business scenes, decision making is first done by a small group of higher managers and then it is explained to other stakeholders by saying "This is what has been decided." The rationale behind it is to avoid unnecessary complications with having too many stakeholders in the decision making process. Haven't you experienced a similar argument before?

In fact, I believe that it is good and necessary to have a variety of stakeholders in the decision-making process from the beginning. More diverse ideas mean that better ideas can be drawn, and the involvement in the decision-making process makes it easier for the stakeholders to accept the decision. Unfortunately, in Japan we're not well trained on how to make an agreement out of multiple conflicting opinions. I suspect that such training was lacking both at school and at home.

Mr. Shintaro Nezuka, former president of Computer Associates of Japan, is a friend of mine. He argues that in addition to language ability, "argument ability" is necessary for globalization. It seems to me that this "argument ability" is nothing more than welcoming new opinions from different perspectives and incorporating them to create a higher level of consensus.

How can we get out of the “Cartel of Mind” situation and become a responsible society that is recognized as equal by the international community?

Of course, first of all, we have to improve our English communication skills. There are now many English teaching materials and new mediums for learning such as podcasts in English, so you have a number of opportunities available to improve your English ability. Still, the most effective method is to increase the chances of actually using English in business. When the opportunity arises, it is important not only to listen to the other person, but also to have two-way communication.

When it comes to business communication, the other party should be talking because they want to get something from you. They may want to get some information from you, such as whether you have understood what they have said to you or how likely it is you will accept what is proposed. If that is the case, the first thing you ensure is that you have something valuable to offer.

Mr. Nezuka warned in the past that "a fluent English speaker without business skills can sometimes be mistaken by the Human Resources department as a global talent just because of his English ability." Global communication skill is a combination of English ability and business ability - at least this is the case in the world of business.

For the second point about the closed nature of the decision-making process mentioned above, I believe that the concept of diversity is most important. **Diversity**, in my view, means accepting and respecting differences. If you're willing to accept different ideas, you won't be afraid of complicated discussions with many different viewpoints and you will be willing to involve many stakeholders into your decision-making process.

Accepting diversity in the decision-making process, however, requires effort. It takes me time to understand different viewpoints, and often I can't evaluate the risks with confidence. Let's say you have two suggestions, *X* and *Y*, at a meeting you host. *X* is an idea from someone close to you, and you know its content and value. Proposition *Y*, on the other hand, is from an unfamiliar person in a different community (a foreigner, for example) and sounds reasonable, but you don't really understand it and you don't know any risks of the proposed option. Should you consider Proposal *X* first as a conclusion of the meeting? Or should you consider Proposal *Y*?

As an approach to diversity, I think we need to put a temporary assumption in place that what we don't know is better than what we know. In a sense, this is a generalization of **affirmative action**. Affirmative action is used to actively favor a group of minority people, for example, to give priority to the promotion of women for the same grades. But I want to define it here as "People who are not members of your own group may have something that you do not understand, so positively evaluate them by assuming that these unknowns are something valuable." The idea is to minimize the risk of missing something valuable because you don't understand it.

If we're willing to accept ideas from different perspectives in our daily discussions, and if that practice is used in our decision-making, the Cartel-of-Mind situation will improve. In order to achieve this, I think each and every one of us should be mindful of taking "affirmative action" as I have defined above.

In the 1970s and 1980s, when we were young, Japanese companies entered the world, and globalization was rapidly advancing. We expected that globalization would further advance and Japan would have soon occupy an important and responsible position in the world. It seems that we have abandoned our efforts to do so. Instead of waiting for advice by Ivan Hall, each of us has to make an effort to globalize.

== End of Blog ==

Acknowledging diversity is by no means comfortable. We need to mobilize our willpower, contrary to the natural feeling of fear of the unknown. We should attempt to "put in place a temporary assumption that what we don't know is better than what we already know".

Chapter Summary

- A technical paper is a tool that scales research across time and space.
- Consider whether your paper asserts facts, laws, or methods. Your thinking structure and research type will accordingly be set up differently.
- Let's get in the habit of writing English papers.
- Presentations alone are not enough to convey the content. Provide a secondary channel to communicate.
- Questions and answers are an important opportunity to receive diverse perspectives and make a good impression with the audience.
- Be psychologically safe at meetings.
- Combine the strengths and weaknesses of online tools and face-to-face communication.
- Make a presence when communicating in English.
- Respecting others is the most important point of communication.

Chapter IV: Career as a Researcher

If you are a graduate student in science and engineering and you are thinking of working as a researcher in the research and development department of a company, what kind of career do you think it will be? It is a long journey if you get a job after graduating from a master's degree to the time of reaching retirement age after 35 years of work (or 40 years if you consider employment extensions). Will you continue to pursue research in the same field? Additionally, is it possible to spend 40 years as a professional researcher? Or will you aim to become a manager? It would be difficult to continue working for the same company from graduation to retirement. Then, how should you consider changing jobs? Is there a career path from business to academia? In this chapter, we consider the careers of researchers.

4.1 Change Research Fields

The motto of Chieko Asakawa of IBM Research is “Make the impossible possible by never giving up.” In her nearly 40-year research career, despite being completely blind, she has made a number of achievements in the field of information technology support for the visually impaired with her strong will.

Strong conviction and a willingness not to give up are important elements of a successful researcher, and I am awed by a researcher with such qualities. I can never be such a person, but it doesn't mean that every researcher has to be like Chieko Asakawa. It is rather rare for researchers in companies to continue research in the same field for the period of their professional career. This is because, in the world of fast-moving technology, what is valued by customers changes with time. Nick Donofrio, IBM's technical leader, retired in 2008 and he reminded IBM engineers of his own career by saying "Don't settle."

Letter "Don't Settle for Anything"

As you may already know, Nick Donofrio is retiring on October 1. Nick has been with IBM since 1964, so it's been 44 years. It was a big shock for me. Since I joined the company 25 years ago, I have met a number of people who I feel like "I want to be like this person," "This person is supporting the company IBM," and "I want to follow this person." Nick is the leader of IBM's 200,000 technology employees and I suspect he's the most admired person by the IBM technical community.

In April, an IBM Technology Leaders Meeting was held in Orlando, Florida, attended by 4,000 technical employees. In the last session, Nick talked to us.

The theme of his message, as well as the theme of this whole meeting, was "Change!" It means that we have to change along with the change of the market. Do you remember listening to some of his talks last year at the last plenary meeting? When asked by an American engineer about globalization ("Are we going to lose our jobs?"), Nick said that values migrate, so you have to change your skills accordingly. "

I heard that Nick joined IBM 44 years ago as an internship student. He studied vacuum tube circuitry in college. Of course, many of the vacuum tube circuit techniques are now of little use. As the leader of IBM's technical community, he spent 44 years studying information technologies such as semiconductors, computer architectures, software, and services on an as-needed basis. Nick wants us to do the same. As the market changes, so does the technology customers demand. We have to lead that change.

At the end of his eighty minute speech, which included 20 minutes of questions and answers, Nick said, "Don't settle for anything!" This was his last word as the technical leader o f the entire IBM company. You've been in the company for so long, and you've become more and more used to the status quo. I think Nick warns you not to be satisfied with the status quo. Constantly improve yourself to be better. That was his last word.

== End of Letter ==

The term "Don't settle" is coined by Steve Jobs in his famous speech at Stanford University. “Keep looking, don't settle” was what he said. As these words were said to Stanford graduates, I understood that the same also applies to non-researchers. For researchers, "keep looking" means not only pursuing new ideas in your field, but also exploring new fields.

There are ups and downs in the research field. In the postwar period when shipbuilding was the top industry in Japan, marine engineering was a popular research field for science and engineering students. Since the Great East Japan Earthquake and the subsequent accident at the Fukushima Daiichi Nuclear Power Plant, the budget for research and development in nuclear engineering has decreased significantly. In the field of computer science, research on databases reached its peak in the 1980s, but the research and development in databases itself has not been seen so much since then. It is inevitable that many database researchers shifted their research focus to data mining and distributed processing. Don't get me wrong, this is not to say that database research is of low value. The database is one of the basic technologies of the information system, and it is natural that the technology must constantly be improved. The point is that any technology has to lose its value relative to other new technologies as they emerge. You should therefore keep in mind that clinging to your field of technology may be a relative devaluation of you.

Of course, it is important to have an obsession with your own research field as a researcher. At the end of the 1980s, people had great expectations for neural network technology, but many researchers were left without the prospect of a practical application for neural networks at the end of that particular phase in technology development. However, Professor Hinton and others at the University of Toronto continued their research without giving up on the technology. They finally achieved breakthrough in 2012 with the help of the improvement of computer performance and the tailwind of big data. After nearly 30 years of pursuing the same technology, he achieved great results.

Unfortunately, it is difficult for commercial companies to continue investing in R&D that does not produce customer value for 30 years. If your research has not produced any customer values in the past 1 ~ 2 years, you should consider changing your research theme. Given your 40 year career as a corporate researcher, your area of expertise will almost certainly change.

Changing areas of expertise can be painful. Your organization may have to terminate your project due to changes in company policy or business conditions. If you have an idea about a new area, make a research proposal for the idea, and if it is approved, you can start in a new research area with high motivation. On the other hand, it may be difficult to change the field of specialization if you cannot find something interesting to you. Unfortunately, as the lab director, I had to cancel many research projects. How would you feel if you were told to stop your project that you had been interested in and were about to produce results? As a corporate researcher, following company's orders is inevitable, but it can be frustrating. There are two things I would recommend for those of you in this situation.

First, as discussed in Chapter 2, always keep a written summary of current research. Even if the research is stopped by the current company policy, the company's situation may change in the future and the technology may be needed. Or, a business that had not been clearly seen before may suddenly come into the limelight at the request of a particular customer. In these events, the only way to ensure that a research project that was discontinued years ago can be restarted immediately is to keep track of the progress of the research. As mentioned in Section 3.1, technical papers are the best tool for this. Technical papers are supposed to be written so that researchers who read them can continue their research by reproducing the results. A paper also has the advantage of becoming an individual's research achievement in the form of publication. Even if it is not a paper, it is possible to record research results in various forms such as internal reports, invention disclosures, and contributions to magazines. In any case, we want to make sure that a sudden cancellation of a research project does not result in the loss of research results that has been accumulated over many years.

If your research project is canceled, you must move to a new research area. The second piece of advice is that in such cases, you should try to broaden your horizons as much as possible so that you can become interested in anything. When researchers looking for new research areas are told "Please do this research project," some say, "I'm not interested in that research project." Corporate research is not about interests, but about necessity. That is what "Research that matters" means. Therefore, when a new field of research is presented, unless there is a specific reason, you should say, "Please let me do it." Of course, the research should be interesting, and as Professor Kanade said, all good research IS interesting. And in any research area, I think there are a lot of opportunities for "good research".

When I was working at IBM in 1997, I was asked to help develop security products that had been showing some schedule delays. Until then, security was completely out of my domain, but I thought it was an interesting opportunity and said yes. When you enter a new field, you get to learn something new with a steep learning curve. Everything I saw and heard was new to me. Among them, I was fascinated by the mechanism of public key cryptosystem, and I entered the world of security research after returning to Japan. Of course, the world of security is wide and deep, and it is not a research area where newcomers can suddenly produce results. However, I continued to learn and was able to find my own new research area, the intersection between XML and security.

In 2011, I unexpectedly started working at the Institute of Statistical Mathematics. I have been researching and developing (broadly speaking) software technologies for about thirty years. Of course, I learned probability theory and statistics in school (and achieved good marks), but I never really understood the contents. I had never written a paper on statistics. The reason why the Institute of Statistical Mathematics invited me to work with them was because big data was becoming a big keyword at the time and they wanted to include people who understood software well. When I came to the Institute, I found that the methods and considerations of software researchers and those of statistics researchers were quite different. At the institute, there were many leading researchers in statistics, such as the then director, Dr. Tomoyuki Higuchi, and the current director, Dr. Hiroe Tsubaki, and I could learn many things, including the probability theory and random processes. In fact, the deep learning which has rapidly evolved since 2012 is based on **statistical modeling**. At the same time, it can be regarded as inductive programming, which means making a program inductively from the training data. Based on this awareness, I proposed a new research area called machine learning engineering after moving to PFN. At present, machine learning engineering is an actively researched field by many researchers and practitioners in the Machine Learning Systems Engineering Working Group of the Japan Society of Software Science and Technology.

One of the PFN values discussed in Chapter 3 is "Learn or Die." The bleeding-edge technology that PFN is challenging is a rapidly changing field, and we recognize that the only way for PFN to remain at the forefront is for each employee to keep learning. This value is a strong obsession of Vice President Okanohara. If each person has a desire to learn at all times, I think the organization can become a very flexible organization that can adapt to changes in the outside world.

I do not think that the encounters with new fields are always successful, but I want to have the spirit to challenge new fields and always look to broaden my horizon.

4.2 Change Job Types

In academia in Japan, many doctoral graduates stay in academia after completing their thesis and remain there for life. However, in private companies you cannot do the same. In some cases, your research success promotes you to a management position, or the R&D department may be downsized and you are transferred to a business division.

Many researchers seem to be obsessed with being a researcher. Some people may be genuinely interested in research, while others may feel some sort of status in being a researcher. It is of course important to study with pride as a researcher, but I think it is also an important choice to experience other occupations. There are two reasons that come to mind.

One is that there might be a job category where you can use your skills more than you can in research. It is not unusual that a person who has not achieved much as a researcher is transferred to a business division and becomes successful. Researchers generally have strong intellectual ability so they can play an active role in any type of occupation. Sometimes research results are difficult to quantify. If you want your achievements to be more visible, becoming a sales representative may be a good choice because your weekly sales are very clear and you can feel your contributions to the company more directly. If you become a consultant, you can go in and help customers solve real problems - that may be more challenging than solving mathematical problems that can be more simply formulated. Sales and consulting may be more challenging for you than research. Keep an open mind about opportunities. You may find a job that is a good fit for you when you try it.

Employees, like capital and equipment, are management resources for companies. The company wants to assign each and every employee to the most valuable job available. In addition, as everyone ages, the strengths and weaknesses of their skills change. Considering your long company life, you should try various jobs if you have a chance without being too anchored to your own research.

The second reason why you should not hesitate to experience other types of work is that even if you continue to build up your career as a researcher, experiencing the world outside the laboratory is always a plus for future research. I myself was assigned to an US-based product division from 1996 to 1997 and also spent one year in a consulting business unit from 2003 to 2004. This kind of out-of-laboratory experience was invaluable to my career. An important part of a researcher's job is to generate ideas. But it's hard to come up with an idea just by thinking alone in your office. Many people say, and I agree, that interacting with people from other fields and industries creates innovation. In addition, it is very important for researchers of the enterprise to see the field where their technology is actually used. If you know the field, you can have confidence in your skills. Even when making a new proposal, the impact of the proposal is totally different between knowing the field and not knowing it. A letter I wrote when I was transferred to the consulting department in 2003 sums up my thinking.

Letter "Transfer to the Consulting Division"

As you may already know, I will be on loan to IBCS (IBM Business Consulting Services) from next month under the new loan program. You may have noticed that the management of the Tokyo Research Laboratory has been discussing this issue. However, I don't think there is much understanding of what this transfer program means for researchers at the

Tokyo Basic Research Laboratory. There are also subtle differences in opinion among the management. I hope this letter will help you understand what led to my decision.

What is the purpose of this loan program? I believe it is about finding out what kind of research we should do to have an impact on IBM's service business in the future.

From 1996 to 1997, I was on a one year business trip to the Internet Division of the U.S. Software Group. Originally, my mission was to promote IBM Japan’s technologies, which I failed in achieving. On the other hand, during the year, I was able to experience how the software industry works. This gave me a sense of where the fast-moving IT world is heading. I think it is because of this sense of "What new technology will IT need in the future?" that I started research and development in the area of XML and security that I had never experienced before. And thanks to the year I spent with the software group, I was able to build valuable connections within the company. As a member of the software group, we have shared a common sense of purpose and are proud to be recognized as a trusted member in the role of researchers among key architects in the company. This network is still a valuable asset to me six years later. By creating an "internal network" and "Sense of IT industry trends" through this long trip, I think I have acquired one of the most important skills I have as an IBM researcher: to understand how to make the largest impact to IBM’s software business.

I signed up for this loan program to the consulting business because I'm hoping that the same can be done for IBM's consulting business. In other words, by developing a sense of "What technologies do customers need in the future in the security area?" and then becoming a trusted researcher through networking in the IBM service business units, I will be able to smoothly contribute the research results in the future.

I am currently a sub-strategist in the security and privacy area of IBM Research. A major focus of this year's research strategy discussion is the impact of IT security on business strategy. In other words, because the business is more dependent on IT, IT security is not just an operational issue but a topic that is directly linked to the enterprise's risk management strategy. However, the current research agenda in IBM Research focuses on individual technologies such as cryptography, protocols, and intrusion detection, and there is no research on business risk management. This is partially due to researchers not seeming to understand what is needed in this area. However, my gut feeling is that this area is a large white space and represents a great opportunity for us to contribute to the service business.

Since I joined the company in 1983, I have been working in areas such as artificial intelligence, natural language processing, and machine translation for 10 years. These studies may have yielded some results in the academic world, such as receiving a Ph. D. from Kyoto University. However, my frustration was that the results of my research had little impact on the IT industry. I went to a customer’s site for half a year and tuned the machine translation by sitting next to human translators, but it didn't work. After that, I started working on handwriting recognition, video on demand, and information retrieval, but it didn't turn out to be very successful.

XML and security were the areas of success for me. My motivation is to find another success for the service business.

Even if you start your career as a researcher, it doesn't mean that you have to limit your experience to the world of researchers. If you have a chance, enjoy your work outside of research.

4.3 Change Workplace

In Japan, there is a practice of lifetime employment, and in the high-growth period, it was common to work for the same company until retirement. However, employment today is more fluid. You don't even know if your company will last for another 40 years. Rather, you should expect to change jobs at least a few times in your career.

Growing up in a high-growth period, when I joined IBM Japan in 1983, I assumed I would stay in IBM until retirement. I decided to leave IBM at the end of 2008. It was difficult to leave a company after more than a quarter century. Looking back, I think I made the right decision. After spending a quarter century in the same company, all that you know is about the company, and you don't know if what you think is right is also right in other organizations. When you change companies, everything is fresh and there is a steep learning curve. There are so many things I didn't know until I changed jobs.

One of the things that surprised me when I moved from IBM to Canon was the handling of labor costs. The largest part of cost calculation in IBM Research is personnel cost. So when departments were asked to cut costs, people were the first to be cut[14]. On the other hand, in a traditional Japanese company, personnel costs are fixed costs and cannot be freely controlled by the department. This makes sense provided that lifetime employment is the norm because it is not easy to adjust the number of people every year. It was a fresh surprise for me, who had always considered labor costs as a variable cost.

The start-up PFN was even more surprising. PFN has more than 250 employees as of August 2019, but there is no hierarchical organization structure. Unlike ordinary companies, there is no such thing as a section chief or manager. Then, when asked who your boss is, you have to answer that it is Nishikawa and Okanohara, the two founders and nobody else. So who makes business, personnel, and budget decisions? At present, it is not always clear, and in many cases it is decided by agreement of the parties concerned. Why is that possible? Frédéric Laloux's *Reinventing Organizations* [Ref. 14] introduces various forms of organization without a management hierarchy.

Why does an organization need a hierarchy in the first place? One of the reasons is to clarify "Who is responsible?" When you ask another department in the company to do something, or when you need to get the company's consent about a new policy, you can easily see who is responsible by looking up the organizational chart. If your company has

[14] In many cases, the cost per employee is more than double the salary paid to the employee, because it includes the office space cost, the cost of support departments, etc.

10,000 employees, you'll be at a loss who to ask. But now we have IT. If the person responsible for each task in the company can be searched, there is no need for an organizational hierarchy, at least in the sense of identifying responsible personnel. The hierarchical organizational structure is easy to understand, but there are some adverse effects such as the bottleneck caused by busy managers, the concentration of authority to specific managers, and the existence of managers itself becomes a cost in the first place. Whether or not the rapidly expanding organization of PFN can continue to use the flat structure is uncertain, but I think that it can be understood that PFN is searching for a new mechanism in the aspect of organization management.

Again, changing jobs is a great learning opportunity. If you don't change jobs at all in your life, you're missing a big opportunity to grow. Even if you get a job at a big company, it cannot be a stable place of work for your life. You should always consider changing jobs as an option in your career.

Which company to choose?

If you are a student and want to find a job, or are already a member of society and are considering changing jobs, which company should you choose?

<u>The most important question for researchers who choose a workplace is whether they can grow in the workplace</u>. The environment where researchers can grow is where there are many excellent researchers. In that sense, well known laboratories would be good candidates, such as NTT's lab, IBM's Tokyo Research Laboratory, and PFN, at least in Japan.

How can you select a good company? I don't know about other industries, but if you are thinking about the IT industry, a company with its own services and also an internal research organization would be the best. Google and Facebook are examples of these types of foreign companies, and DeNA and Mercari in terms of Japanese companies. Why not recommend IT vendors such as IBM, Microsoft or Fujitsu? One of the reasons is that the development of IT systems has become more iterative, exploratory, and continuous.

In the early days when large mainframe computers were introduced into enterprises in 1960's, enterprises employed COBOL programmers themselves to build systems such as payroll and inventory management. However, it is inefficient if several companies develop the same system individually. That is why computer manufacturers increasingly developed software packages that are common to certain industries, and large user enterprises who had already developed their internal systems detached their information system divisions as subsidiaries to sell these internal systems to others in the same industry. In this way, most user companies lost the ability to develop their own systems. When the specification is relatively clear, as in the case of payroll and inventory management, the sharing of roles, in which the user enterprise presents the specification and the vendor enterprise implements it, works well. However, in systems with complex and dynamic user interfaces and systems that are developed inductively using machine learning, projects become iterative in the first place because they do not know what kind of system to develop, and they do not know whether they can satisfy given specifications, so development becomes exploratory, and even once they are completed, they need to be continuously modified during operation. Thus, in iterative, exploratory, and continuous system development, a two-party model in which a user company places an order and a vendor company undertakes it often fails. Each incentive does not point to the same vector. This is particularly evident in data analysis projects using machine learning.

Many of the enterprises applying machine learning (Google or DeNA, for example) have developed these systems by themselves. At the 2018 Machine Learning Engineering Symposium, Takuya Kudo, who leads the global data analytics team at Accenture, made it clear that a joint venture would be needed for a data analysis project to be ultimately successful. This means that data users and technology vendors must work together to form new joint ventures and share the revenue.

Another thing to keep in mind when choosing a company is whether or not the company has fallen into "process fundamentalism". We will discuss process fundamentalism in more detail in Chapter 5. If a company puts too much priority on observing rules and compliance and interferes with the free and vigorous activities of its employees, researchers should avoid the company. Just because a company is famous doesn't mean it's a good place to work for researchers.

It should be noted that the salary offered to you does not necessarily accurately reflect your market value. The salary offered varies for various reasons. Maybe you just happened to make a good impression during an executive interview, or maybe a misspelling on a resume caught the eye of someone in human resources and lowered your rating. If the job market is efficient, i.e., it accurately reflects the market value of a candidate based on the supply-demand relationship, then if your true market value is Y, the salary offered will be distributed stochastically around Y. If this is a normal distribution of mean Y and standard deviation σ, about one of the seven firms will offer $Y + \sigma$ salary to you. But don't take $Y + \sigma$ as your current market value. If you assume so and set your lifestyle based on $Y + \sigma$ income, you will be in trouble the next time you change jobs[15].

Financial rewards are a typical **hygiene factor**, a condition that needs to be met at the minimum level, but additional quantity beyond the minimum level does not contribute to increasing motivation. If you're being offered a level of salary that doesn't hurt your life, it's better to choose a job based on what you want to do, whether the company has people you want to work with, and whether you can grow in the environment.

Academia?

Academia, such as universities and government research institutes, is a promising candidate for consideration when considering a career of researching. I spent three years as

[15] This situation is typical in sports events. A player who performs well in one game (by random factors) often ends up with the average performance in the next game. This is called **mean reversion**.

a visiting associate professor at Tokyo Institute of Technology and five years as a professor at the Institute of Statistical Mathematics. One of the things that I strongly feel is that young researchers in academia think too narrowly about their careers. Staying in academia after graduation for life, waiting for tenure positions to become available while doing postdoc research and then becoming an associate professor and eventually a full professor, is one career. It's a viable option, and I think it is a path for a strongly determined researcher, but if you look a little wider, there are many opportunities in society. It takes courage to change your environment, but it is also a chance to learn. I hope you have a broad horizon and seize the various opportunities available to you.

In the U.S., people move more casually between academia and the private sector. Recently, more researchers are moving from academia to the private sector in Japan. I myself changed my job from the Institute of Statistical Mathematics to PFN in 2016. In PFN, there are many researchers who have moved from academia, including Akiba who I mentioned in Chapter 2. I expect this trend for mobility of talent to continue in the future.

4.4 Things to Learn in School

I advise you to study core subjects while you are in school. Prof. Hisashi Kobayashi, the founding director of IBM's Tokyo Research Laboratory, used to say, "you must learn Mathematics and languages while you are young." I think he is right. Mathematics is a basic tool used in all science and technology. Whether it's Calculus, Algebra, Statistics, or Probability Theory, there will always be a time in your research life when you need it. I originally specialized in natural language processing which was based on formal language theory at that time, but after circa '90, statistical methods have become mainstream in natural language processing. I was good at Discrete Mathematics (which includes formal language theory), but later I found that I lacked skills in Linear Algebra and Statistics. As long as you are in a research position, you cannot avoid Mathematics. I want you to acquire core knowledge of Mathematics while you are in college.

The same applies to English. Like it or not, English is the official language of the world in science and technology. If you want to become a world-class researcher, English is inevitable. I would like to give you three pieces of advice on English.

The most important thing for you who aim to be a researcher is the ability to read English. Of course, English conversation is important, but it doesn't matter if you don't have the ability to read technical books or papers. You have to read a lot of books and papers to become comfortable in reading English. Some people try to read papers by looking up every unknown word one by one, but this is not a very efficient method. Even if it is written in Japanese, you may not know what the first few sentences of your paper are going to say until you have finished reading it. Rather, it is better to try to understand the structure of the whole paper by skipping over the parts you don't understand in the first pass. Once you vaguely understand the paper's structure, including what the research problem is, along with the assumptions, and main results, you should be able to read each English sentence more easily. Then, start looking up words that are most frequently used.

The ability to read and write emails in English is also important. In some research areas, the primary information may come from mailing lists. The English used in email and mailing lists is a little different from the English used in textbooks and papers, and there are some informal expressions, so it is necessary to get used to them. For example, a common expression used in discussions on English mailing lists is "IMHO," which stands for "In my humble opinion." To learn such "mail English," you should be exposed to such expressions multiple times. To achieve this type of fluency doesn’t necessarily require you to join a mailing list focused on your research area; instead, it can be anything that you are interested in. One of my hobbies is to fly radio-controlled airplanes so I joined in a mailing list for this hobby which in turn allowed me to acquire a lot of such informal expressions. Once you get used to reading English mails, you can also write one in English to the mailing list by yourself. In this way, it is possible to make a presence in the international community as I mentioned in Chapter 3.

The third, of course, is conversational ability. You'll have to use it. If you have a chance to go abroad when you are a student, you should seize the opportunity. Even if that opportunity does not present itself, there should be many foreign students in your university. If you have a foreign student who is still not good at Japanese in the same laboratory or in a nearby laboratory, I strongly recommend that you become friends. As you get closer to each other and speak almost every day, soon you should be able to understand each other even in broken English. Then English will come out unconsciously. I've heard that the best way to learn conversational English is to have a boyfriend or girlfriend who speaks English, though it may not be always possible.

In addition to Mathematics and English, you must study your domain subjects well. In addition, it is also important to acquire common knowledge to understand enterprise activities such as sales and accounting. In order to do so, I want you to be interested in liberal arts as well [a]. Why don't you take subjects such as Sociology, Economics, and Management at an opportunity? Also technology schools today often have courses on Technology Management (MoT), Learning subjects such as R&D Processes, Commercialization, Financing, Human Resources, Intellectual Property and Patent Strategy, and Innovation will eventually be useful for you.

In university classes, students are often asked to submit reports instead of getting grades by exams. This is a good opportunity for you to build your writing skills. A report on a subject like "What do you think about XX?" is a short but real paper. Why don't you use it to practice your ability to write papers, pretending that you are actually writing a paper to be submitted to a technical conference? There are a lot of books and web pages like "How to write a good paper" that you can refer to improve your writing skills.

When you are assigned to a laboratory in the fourth year of undergrad, it is also important to learn how to conduct research as described in Chapter 2. There is no good textbook on this. You have to learn it from your supervisor and senior researchers in the lab.

From the hiring point of view, the research ability of a student seems to greatly differ depending on who the supervisor was. As a result, our hiring decisions tended to be biased toward those from specific laboratories. Because of this, it may be a good strategy for you to select a laboratory that has produced many researchers working in your target company.

Should I Pursue a Doctoral Course?

When I talk to students who aim to become researchers in companies, I often hear the question "Should I go the the doctoral course?" My short answer is "Yes." As described in Chapter 2, a Ph. D. proves the candidate who has the "research ability" of being able to propose, conduct, and organize research - or at least he/she should. Therefore, it is the right strategy to obtain a doctoral degree (and basic research skills) before joining a company.

When entering a doctoral course, it is even more important to select the right supervisor, at least in the case of a Japanese university. In some laboratories, Ph. D. students conduct their research under close direction of the supervisor. Such a student tends to have narrow views and are not flexible even after they get their Ph. D. It is difficult for a company to hire this type of candidate who has limited perspective no matter how impressive their publication list is. On the other hand, under a good supervisor, students who have a wider view and are able to explain the positioning of their own research. This type of person is more flexible and easier to fit into any research project in the company.

It is said that there are superfluous postdocs in Japan so you may well be worried that you might not have a job after getting your Ph. D. Until about 10 years ago, many Japanese companies tended to prefer hiring master's students who are considered to be more flexible (and the companies were willing to spend years to train them). However, the situation is changing. The labor market for future researchers will become more fluid and a company no longer has the luxury of training new hires for a number of years. Therefore, as a Ph. D. holder, you have a higher chance of getting a job in a private company in the current market. Even if you do not have an immediate position, you do not have to worry too much because there are many postdoctoral positions in academia so you can take your time to find a suitable position. I think it is more important to build up achievements that can be listed on your resume and thus increase your market value.

Obtaining a Ph. D. is important, but it can be later in your life, because you can go back to school after working for a company for several years. Recently, many universities welcome such experienced Ph. D. students. I myself received my Ph. D. from Kyoto University after 12 years of work at IBM.

Based on my experience in academia it seems that experienced students are generally more motivated on their research. This may be because they know the real business problems that need to be solved. The same can be said for those that graduate from a technical college and continue their studies in a graduate school. Japanese technical colleges teach practical engineering but not much theory. My impression is that students who are experienced in practical engineering are more motivated about learning the theory behind the practice. In my own experience, I found that studying the basic theory when I really need it was the most effective way to make it my core knowledge.

If you have a chance to go for a doctoral program, you should think about it. There are many professors in many universities, so don't think too narrowly about where to go to, and ask people around you about their reputation. Also keep in mind that there is also an option to get a Ph. D. after joining a company. Check if your company allows (and encourages) their employees to get Ph. D. qualified.

Know About the Company

The number of researchers in the private sector in Japan is said to be 450,000. The number of companies with such researchers must be enormous. You have to think about which companies to apply to.

I think the best way to learn about a company is through an internship. Many companies now offer internship programs for a few weeks, mainly during the summer vacation. PFN also accepts dozens of internship students from home and abroad every year. In the internship in the laboratory of the enterprise, the research is carried out in the actual research environment with full-time researchers, so that the student can understand well what is happening in the lab. On the other hand, companies can use the power of motivated students like yourself, and if you are really good, they may start recruiting you. It can be said that this mechanism has merits for both students and enterprises.

Since internships are usually offered only once a year, it is impossible to get to know many companies. To learn more about the research in other companies, it is better to participate in academic conferences. Listen to the research presentations from researchers in companies you are interested in and ask questions if possible. In some cases, you may be invited to the lab for further discussions. In the same manner, it can be effective for you to present your own paper. A researcher from your target company might speak to you after your talk.

I am sometimes disappointed because students tend to judge a company by the popularity of its name. Companies with consumer-oriented products and services are more popular, while so-called business-to-business (B2B) companies and start-up companies whose main customers are other companies are less known to students (or their family members). In modern industry, a very complex value chain is hidden behind the consumer-visible form of the final products or services. This value chain consists of suppliers, equipment manufacturers, and engineering companies who have very advanced technology but do not manufacture end-user products by themselves. In the IT industry, system integrators and consulting companies, among others, comprise the B2B value chain. Many of these non-consumer-facing companies have their own R&D departments, conduct world-class research, and are active in markets around the world. I think it is important to understand the modern industrial structure well to allow you to look for these good hidden companies.

4.5 Enhance Your Market Value

As a researcher, how can you increase your market value? In academia, your performance is measured by the number of your publications, but in the private sector, the measurement is more diverse. There are many researchers who have a large number of papers but not enough ability to carry out good research; on the other hand, there are some who have produced excellent results in open source software even though they have few papers.

To increase your market value, it is important to ensure your reputation in the research community that transcends organizational boundaries. Writing high-profile papers is one of them, but it's not the only way. There are good researchers who are active in open source software, a good example of this would be Yukihiro Matsumoto, the founder of the programming language Ruby. Additionally, there are people who have a presence in standardization activities, people who are active in the academic society, and people who are known in Internet communities such as the blog of Qiita, etc. These people are valuable from the viewpoint of the enterprise. Therefore, you should make every effort to increase your presence in these communities.

The academic society is one of the places you can make a presence in the research community. Activities at academic societies include participation in workshops, peer review of papers, and contribution to the various roles of societies (secretaries and supervisors of study groups, directors and officers of academic societies, program members of international conferences, etc.). Through these activities, if you can make people think that they want to work with you, you can increase your value to the market.

Even if you do not involve yourself with an academic society, you could get involved in a community of engineers. In Silicon Valley, it is common to hold "meetups" where engineers from various companies get together to learn. In Japan, engineer meetups are held almost every day[16]. PFN also supports such activities by organizing various meetups and providing venues.

Some Japanese companies are reluctant to let their employees participate in academic or community activities. One reason is to fear the loss of confidential information through interactions with outside people. Unfortunately, such companies cannot attract good researchers. Academic and community activities are one of the driving forces of research, and limiting these activities deprives researchers from reaching their market value.

Academic and community activities are important tools to increase your market value. I myself always try to be a researcher with whom many people want to work with.

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[16] For example, https://connpass.com/ is a Japanese hub for IT engineer events.

4.6 Think About Your Life

At one point, a female researcher who had just given birth approached me and ask "Should I prioritize work or family?" I answered without hesitation: "family." She is now the mother of three children and is active as a frontline researcher.

Life events such as marriage, childbirth, child care, and nursing care for elderly may come in from time to time in a long researcher's life. Parenting, in particular, often overlaps with the ages when you are most productive, so you may find it difficult to balance work and life. Pregnancy and childbirth impose a heavy burden on women, but childcare should be shared equally by the mother and the father, so the issue of work-life balance is not only for female researchers but also for male researchers.

Childcare is probably the hardest when the child is under three years old. For a few months after birth, parents can't sleep well at night, and even if they start going to preschool, they quickly develop a fever after getting sick from another child. It's natural that you may feel impatient when you are feeding milk or changing diapers, while your colleagues are conducting experiments and writing papers.

You don’t need to be impatient. The time you spend with your children is so precious. There is a saying "a child shall have given a lifetime's share of filial piety by the age of three." I completely agree. Of course, research is also important. "Research is my life work" you might say. If so, what lingering affection do you have for spending a few years out of your 40+ years of research career on parenting?

What is your life for? Here is an interesting paper written by an authority in business administration.

Evaluate Your Own Life

Clayton Christensen is a professor at the Harvard Business School, who made a name for himself with the book *Innovator’s Dilemma*. In 2012, he wrote a paper titled “How Will You Measure Your Life?”, which is not about innovation or management. It is a theory about life. He wondered why so many of his Harvard Business School classmates were so distressed by the unfortunate lives they were having, such as divorced or jailed after the Enron scandal. Christensen asks; If you can't manage your own family and life, how can you manage a company?

This paper was later revised and published as a book [Ref. 15]. He argues with the viewpoint of being a business management expert that the theory of business management should also apply to the management of personal life. Life should have goals, just like corporate management. What is your goal in life? Will you be richer than anyone else? Will you demonstrate leadership as the president of a large corporation? Or will you build a happy family and become someone loved by everyone?

You may not take this "What is your goal in life?" question seriously. If "the goal of life" is too heavy a question, instead you may ask "What is the most important thing for me?" As a researcher, you might think that your goal in life is to achieve great results in your own research. But is that a goal that you can sacrifice for everything else, including your friends, your family, and your integrity? Please think about it.

Once the goal is clear, you can see what you need to do for it and where you need to invest your "management resources," i.e., your time and attention. Christensen argues that various ideas of management theory can be used for personal time management.

It is very interesting to see what your life looks like through the lens of business administration. Then, going back to parenting, what do you see when you look at parenting through the lens of a researcher?

Parenting and Research

Alison Gopnick is a developmental scientist at UC Berkeley. She herself raised three children and now has three grandchildren. In her book *The Gardener and the Carpenter* [Ref. 16], she analyzes the role of parents in parenting.

Parenting is a strange word. The noun “parent” is forced into a verb, which, if anything, means "doing what parents should do." It seems to indicate the how-to like "how to raise a child with such and such ability." According to Gopnick, however, children grow up receiving a much more diverse range of external stimuli than their parents provide. By intentionally being distracted to any external stimulus, young children receive all external stimuli equally and learn what is important to them. This may be similar to the search phase that researchers perform. As children grow up, they become more focused on what's important, and as a result, they self-optimize towards spending their time more effectively. This may correspond to the utilization phase.

So, probably it is wrong to put your children who are performing learning-by-searching within a predetermined frame. She argues that it is the role of parents to provide a safe environment for their children to explore. What the modern developmental science teaches us is that we can't control how your children grow up. In other words, we should forget about "parenting." If you're a parent and think "Is my parenting right?” or “Am I obsessed with my research too much and I'm not giving enough love to my children?" you should read this book. Looking at parenting from a researcher's lens, you can see that concepts like "correct parenting" does not make sense in the first place. You do not develop your children. They grow by themselves.

My wife, Naoko Maruyama, is a university professor and a researcher. We had two children and they grew up with the help of our parents and the local nursery school. Naoko and I both continued to work full-time, and in that sense we might not have paid enough attention to our children. But they both grew up well, and now our son is an editor working for a publisher and our daughter is a start-up founder, both of whom have a child and are nurturing the next generation.

4.7 Yet Another Research Career

To conclude this chapter, I would like to look back on my own career as a researcher. The following is an article I contributed to the *Journal of the Japan Society for Artificial Intelligence* in 2013[17], this version of the article including some updates. If you include the four years from my undergraduate course, it was exactly forty years of research life as of this writing. It's just one instance of a researcher's life out of infinite possibilities, so I don't know how much helps you to design your career. It's a bit long but be patient.

Article "Yet Another Research Career"

Many of the researchers who have made excellent achievements in the history of science, such as Maria Curie and Professor Yamanaka of iPS cells, have made brilliant achievements by working with strong conviction and patiently accumulating results for many years. Strong conviction and a sense of not giving up are important elements of a successful researcher, and I am left in awe by a researcher with such qualities. I can never ascertain that level of dedication, but it doesn't mean that every researcher has to be Mrs Curie or Professor Yamanaka.

Researchers have different styles and different careers. I hope that the valuable encounters and opportunities I have gained in my research life described in this article will give readers a clue on how to think about their own research life.

How I became a researcher

My first encounter with a computer was in high school. At that time, I think it was uncommon, but my high school had a minicomputer called OKITAC 4300C in the Math Club of which I was a member, and I could use it freely. I had been a science boy and had an interest in electronics, but after programming FORTRAN and assembly language with this computer, I began to want to learn computer science. Mr. Fumito Nishino (now in Fujitsu), who was a senior member of the Math club at that time, said that "if you want to study computer science, you should go to the Tokyo Institute of Technology", so I followed him to the school taking the Information Science major. At that time, there were few universities that had a department of Information Science.

I joined the laboratory of Prof. Izumi Kimura, but I was lucky because Dr. Aki Yonezawa, who had just returned from Hewitt's lab, was also in the lab as an assistant professor. Dr. Yonezawa gave me a book titled *Understanding Natural Language* [Ref. 17].

[17] Vol. 30 No. 5 (September 2015).

This book is the doctoral dissertation of Terry Winograd of MIT and covers the SHRDLU system, which is a monumental work of early natural language understanding. SHRDLU understands English such as "Pick up the big red block" and executes it on the computer graphics display screen as shown in FIG. 9. There are 2 red blocks in the scene, and the robot has to figure out which one is the "The big red block" and then to plan how to move the top green block to another location before picking up the big red block.

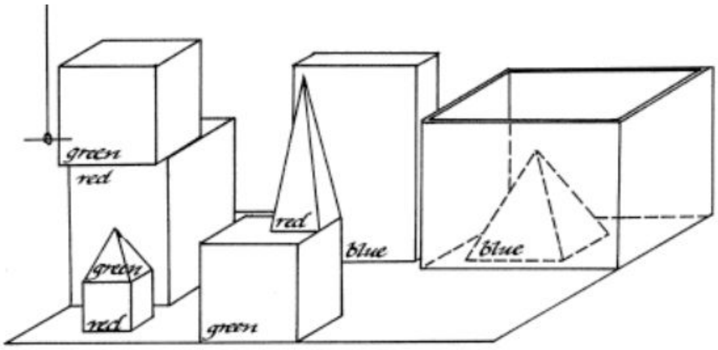

Figure 9. Screen capture of the natural language understanding system SHRDLU [Ref. 17]

SHRDLU was a dream system (at least for me) that integrated parsing, semantic analysis, contextual analysis, and planning.

When considering the theme of my master's thesis, Professor Yonezawa presented me with the task of reinventing a SHRDLU-like system with the latest technology of logic programming which would modernise SHRDLU as it had been developed ten years earlier. To meet the the research goal I constructed a natural language understanding system in Prolog.

At that time, Japan's Ministry of International Trade and Industry initiative on 5th-generation computer systems was about to start, and I would come to know well-known researchers in natural language processing, such as Yuji Matsumoto from the ElectroTechnical Laboratory (now at Nara Advanced Institute of Science and Technology) who came to lecture on BUP, a bottom-up parser using Prolog, and Takashi Chikayama and Hideyuki Nakajima from the University of Tokyo who were kind enough to give me the magnetic tapes of the Lisp and Prolog processing systems they were developing.

The research was interesting, but I didn't expect to become a researcher. As for job hunting, I applied to IBM Japan to become a systems engineer, thinking that a company with no laboratory alumni would be interesting. It was in 1982. I didn't know that IBM Japan had just established a research organization (Japan Science Institute) in Tokyo that year and was expanding its lineup rapidly under the founding director Hisashi Kobayashi. When the human resources manager at IBM Japan found out that I was a master's degree student in Computer Science, which was rare at the time, he told me to go to Japan Science Institute for an interview instead of becoming an SE. This was how I became a researcher.

Natural Language Processing

When I joined the company, I started working on Natural Language Processing as a continuation of my research. At that time, there were three major research centers in IBM Research - that is, Watson Research Center in NY, Almaden Research Center in CA, and Zurich Research Center in Switzerland, and several more satellites called Scientific Centers in Germany, England, France, and other countries. I think the Japan Science Institute was positioned somewhere between a research center and a scientific center.

One advantage of working in a laboratory of a global company such as IBM is that you can easily talk to world-class researchers. There was no Internet at that time, but there was a system called VNET which connected the mainframe computers around the world with the company's internal network, and I could exchange e-mails with IBM researchers abroad. Karen Jensen and George Heidorn, who had been developing a famous English parser called PLNLP (Programming Language for Natural Language Processing), and Michael McCord, who, like me, developed a natural language processing system in Prolog and published a paper in the *AI Journal*, the best journal for Artificial Intelligence, were among them.

SHRDLU succeeded in treating a subset of English in a limited situation, but it was around this time that I realized that understanding natural language in general was an impossible task, at least for many years. So I decided to focus my research on machine translation, especially in the area of syntax analysis of Japanese. At that time, the analysis of natural language was mainly based on Chomsky's Phrase Structure Grammar, and it was difficult to find the grammar theory suitable for the language based on the relations among words like Japanese. No formal language theory was known for such dependency analysis, so I proposed a formal grammatical system called Constraint Dependency Grammar.

I was originally better at programming than theory. However, in order to get a new grammar theory to be accepted by the research community of Natural Language Processing, I understood that it was necessary to prove theoretical properties such as weak generating capacity and parsing computational complexity. At the Tokyo Research Laboratory, there were several excellent researchers with strong knowledge of theory, and I owe a lot of theoretical support to my colleagues at that time, such as Shinichi Morishita (now at the Medical Research Institute, University of Tokyo) and Kazuo Iwano (currently at Mitsubishi Chemical Holdings).

In 1990, 7 years after joining the company, my paper on the Constraint Dependent Grammar was accepted to the annual conference of ACL (Associations for Computational Linguistics), and since then my papers has become consistently accepted at top conferences. At one of these international conferences, I met Professor Makoto Nagao (former president of Kyoto University). Professor Nagao liked my presentation so much that, when we got together at a Chinese restaurant in Montreal where we had dinner with other researchers of natural language processing, he offered me to write a doctoral thesis under his supervision.

XML Security

I received my Ph.D. from Kyoto University in 1995, 5 years after Professor Nagao called on me to do so. However, I eventually became frustrated with my research on Natural Language Processing. Natural language is full of exceptions, and grammar-based Natural Language Processing, which many researchers had tried at that time, had to have very complex hand-written rules. Statistical Natural Language Processing and case-based methodologies were beginning to emerge, and I tried them myself, but they didn't lead to much improvement, and most of all, I couldn't find any useful applications with the low accuracy of the resulting technology at the time.

Although my degree in Natural Language Processing was given due to my work on this field, I tried to work in different areas, including information retrieval, handwritten character recognition, and multimedia. In the meantime, Dr. Iwano became the director of the Tokyo Research Lab. Dr. Iwano knew me well through my research on Natural Language Processing, and thought I would be a good fit to lead the planning department of the laboratory. It was my first management position.

The work of the planning department in the laboratory often involved miscellaneous work such as arranging meetings, preparing presentation materials, etc. On the flip side, I could learn about the latest research topics in a wide range of fields from physical science, semiconductors, compilers, graphics, speech recognition, algorithm theory, to software engineering from the 200 researchers that were actively researching at IBM Tokyo Research Labs at the time.

After about a year of being the manager of the planning department, Dr. Iwano sent me to work overseas, which I had wanted for many years. I was assigned to the Internet Division, a product division headquartered in upstate New York. This was not a research department, but a software product development business unit, where I did Internet-related technical evaluations of Silicon Valley start-up companies and was also involved in product development. This is where I encountered two technologies: XML and public key cryptosystems.

After two years of management and international assignment, I returned to the role of researcher. I was leading a small team of talented people consisting of Naohiko Uramoto (currently Mitsubishi Chemical Holdings) and Taketo Tamura (now at Google) where we started developing XML technologies. At the same time, I received a position as a visiting assistant professor at Tokyo Institute of Technology. Before leaving for the United States, Prof. Hozumi Tanaka of Tokyo Tech (he passed away in 2009) suggested to me: "Why don't you come to university - busy days at IBM will ruin your health?" Thus, I started a double life, spending three days a week at the IBM Tokyo Research Lab and the other three days at the university.

At the university, I decided to work on Internet security, especially on the security of downloaded code and code signatures. In the process, we discovered code vulnerabilities in popular browsers (Figure 10).

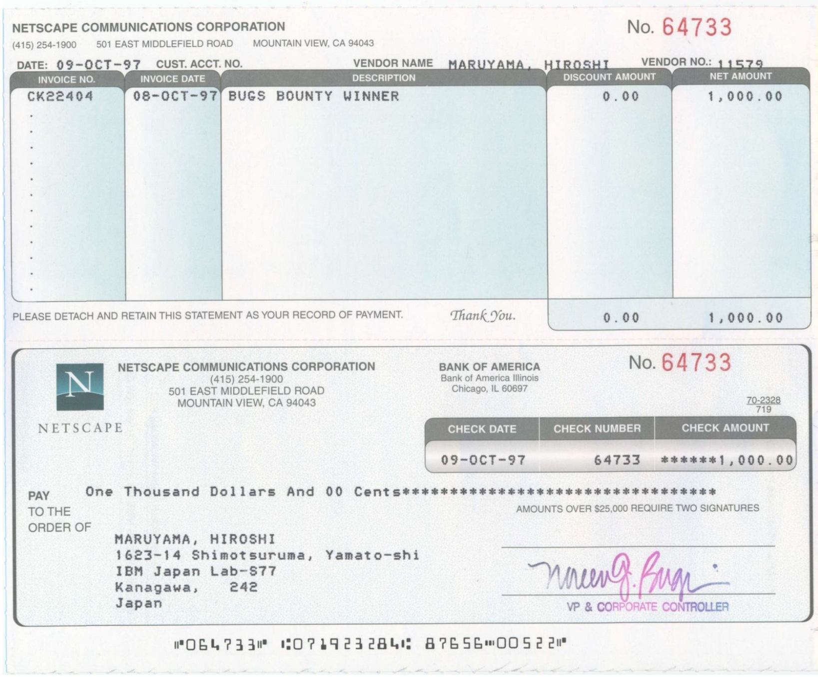

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Figure 10. Bug Bounty for Netscape Navigator

This double life gave me a great opportunity. XML and security were both hot research topics, but only a limited number of researchers were familiar with both. Our team, together with Anthony Nadalin, Maryann Hondo, and others from the Software Division, were able to develop and standardize technologies for XML and Web services security on behalf of IBM.

It was a great pleasure to have a presence in both XML and security in IBM's global team during this period of time. Though I could not publish many academic papers, I felt that what I was doing had an impact on the world. Needless to say, it was the result of a network that transcended organizational boundaries through collaboration with international researchers such as Bob Schloss, Noah Mendelsohn, and Makoto Murata in XML, and in security, Charles Palmer, Matthias Kaiserswerth, and Michael Weidner. I really enjoyed working in a global team that would have only been possible in global companies like IBM.

On the other hand, private companies were required to contribute to the business even in the research department. In response to the shift in the focus of IBM's business towards services such as consulting, system integration, and operations, IBM Research was given increasing opportunities to be directly involved in projects with customers. At the Tokyo Research Lab, the director at the time was trying to send researchers to the consulting business to directly contribute to IBM's income. I objected to sending young researchers who had just started their careers as researchers to the consulting business unit. My boss said, "if not young researchers, do you volunteer?" And I said, "Yes."

Consulting

Thus, I was transferred to the IBM Business Consulting Service. I had never done consulting before. At that time, the IBM Consulting Service had a very good training program, and I went through a one-month online training course and one week camp training. I learned many things about the basics of consulting.

Consulting is a strange business. You do not necessarily provide the best solution for the client. Sometimes, even if you don't think it's the best, it is the solution that the client is satisfied with. In one point of view, you may say that the consultant's job is to extract a solution that the client already knows subconsciously, make it explicit, and produce a rationale behind the solution. It is fundamentally different from researchers who always have to pursue the best solutions in objective measures.

The final one-week training camp was all about communication skills -- facilitation, presentation, negotiation, etc., which turned out to be a very useful skill even for a researcher, and that fact alone makes me think that my transfer to the IBM Business Consulting was a good thing for my career as a researcher.

I was placed in a security consulting team. At that time, Eijiro Oki (currently Kogakuin University) was starting a new consulting practice on Information Security Management based on the ISO standard of Information Security Management System (ISMS), and I learned a lot from him. Security cannot be achieved only by individual technologies such as cryptography. I learned that it is important to consider the management process of setting policies, designing the whole system according to the policy, and always running the PDCA cycle for new threats and vulnerabilities. It is difficult for researchers who study individual technologies to see the whole picture.

In actual projects with clients, we often interviewed various departments to understand the actual state of the client process. I found that every company is different. For a researcher who had only known IBM, every interview was a series of surprises.

Some clients wanted to introduce the security management process. Other clients had urgent needs to respond to security incidents. I was called by one client who had lost a half million records of their customers' privacy data. We didn't know where the attack came from, but one thing we found was that no one knew exactly where the customer records were stored. Of course, they were in the master database, but there were many partial copies of the master database scattered around the company because sales representatives had made their own copies to their PCs. How can we find all these copies? I came up with an idea. I asked the Natural Language Processing researchers at my old home, Tokyo Research Laboratory, to create a tool that scans the file system of a PC and lists files with a lot of personal information. It is not so difficult to extract proper noun information such as names and addresses using the natural language processing technology. We had been struggling with real-world applications, but I realized that I could find new applications relatively easily. I only needed to see clients.

I became a senior manager when I returned to the Tokyo Research Lab after about a year assignment to IBM Consulting Services. One Sunday morning in February 2006, Paul

Horn, then head of IBM Research, called me at home. He said, "Are you interested in becoming the director of the Tokyo Research Lab?"

Research Management

When I applied for IBM in 1982, I didn't expect to be a researcher, but it so happened that I became a researcher. Since that first role as an IBM researcher I had a vague idea that I would be a researcher for the rest of my life. Although I became a senior manager, in my mind I thought I was still an active researcher. So Paul's remark about becoming the director came as as a surprise. With that being said, my motto is "If you're given a chance, you grab it." So I said without hesitation, "Yes."

At the time, there were about 3,000 so-called "executives" in IBM around the world. The director of the lab is considered to be an executive, so as a member of this "executive" community, I received education such as how to build an organization, how to form its strategy, how to protect the organization, and how to develop talent. One of the advantages of being a member of the IBM executive team is that information such as what kind of trends exist in business and technology, and how the top leaders of IBM think about these trends is available to you in real time. For example, IBM Research produces a strategy document called Global Technology Outlook every year, and I was deeply involved in the process as the director of Tokyo Research Lab. Through such activities, I have learned how to look at the whole IT industry from a bird's-eye view and how to predict what kind of technologies will become important for IBM's business.

Among IBM's top executives, I most admired Nick Donofrio, who is the highest-ranking officer managing the 200,000 technical population around the world. Executive orders that came down from the top were sometimes hard to understand or follow, but Nick always spoke in his own words, and Nick himself hesitated, sometimes distressed, but still told us what to do for the company. I think I learned from Nick what true leadership is.

At the time, IBM Research was organized into 5 areas: Technology (mainly semiconductors), Systems (servers and system software), Software (middleware), Services, and Solutions. The head of the Software department was Alfred Spector (he went to Google and eventually left in 2013.), a former associate professor of CMU and an expert in distributed systems. Alfred was a specialist in distributed processing, but he was well versed in all fields of computer science and excelled at understanding the nature of the problem. He is one model of a computer science researcher that I still admire.

Alfred wondered why a total of nearly 200 researchers at IBM Research were working on a variety of disparate natural-language processing projects and directed the development of a unified architecture for these many technologies: the Unstructured Information Management Architecture (UIMA). This UIMA became the basis of the question answering system Watson which went on to win the TV quiz show, Jeopardy.

At that time, Tokyo Research Laboratory had about 180 world-class researchers, so it was exciting and fun to have a technical discussion with them. 180 people were just the right size, and I could follow what each individual researcher was doing, while 180 people covered almost all areas of computer science, so for any problems in computer science, somebody could immediately come up with something as experts in that area.

Being the head of an organization is not always fun. At that time, the emergence of so-called BRICS was intense, and the position of the Japanese market was - in relative terms - declining. In conjunction with this, a shift in resource allocations was defined for us in the IBM research department, and along with some other labs, Tokyo Research Laboratory was forced to reduce our number of researchers. When the financial crisis occurred in 2008, I reluctantly had to ask a considerable number of researchers to leave the laboratory. When I reflected on this experience, I felt that I should not remain at IBM. I decided to leave IBM after 26 years of service.

Statistics and Big Data

After IBM, I worked for Canon for about one year before quitting, and I was unemployed for a while[18]. In the Japanese business scene, being unemployed is quite a handicap. For example, when I made a phone call, it was very awkward that I could not tell my affiliation such as in "I am Maruyama from Canon Digital Platform Development Division." or "This is Maruyama from the IBM Tokyo Research Laboratory."

One day at a party, I met Dr. Tomoko Matsui of the Institute of Statistical Mathematics, and she asked me, "Are you interested in the Institute of Statistical Mathematics?" I submitted an application as if it was my last hope for my career. I was interviewed, and was hired by the Institute of Statistical Mathematics in April 2011, right after the great earthquake hit Japan. The big data boom was just beginning to blossom.

I was never an expert on statistics or big data. However, I could learn a lot from Dr. Tomoyuki Higuchi, who was then the director, and Dr. Hirokazu Tsubaki, who is the current director, and I was able to form my own view of the world with regard to big data and analytics, I gradually integrated that knowledge to my background in software.

Machine Learning Engineering

While I was conducting two research projects, namely Data Analytics and Systems Resilience at the Institute of Statistical Mathematics, I was often called to government committees such as the Ministry of Economy, Trade and Industry and NEDO (New Energy and Industrial Technology Development Organization). In these committees, one topic of discussion was "What is the next computer architecture after cloud computing?" I had never thought about this before, so I thought this was an interesting question and asked various people for their opinions. One of them was Daisuke Okanohara, the current vice president of PFN.

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[18] I applied for Google, but I got a rejection notice just after I quit Canon.

I asked him at a Starbucks in Yurakucho and he said, "In the future, most data will be stored at the end of the network, not in the cloud." In cloud computing, data is said to be gathered in a data center called the “cloud.” In the 1970’s when databases were first invented, the idea was that data was valuable and should be managed independently from applications - that is, the data should be stored in a central data center. I had not questioned the concept, so I was surprised when Okanohara said something to the contrary.

But if you think about it, it is natural. In the IoT (Internet of Things) world, where many sensors such as video cameras are connected to the network, a lot of data is generated. Even if you send all the surveillance camera data to the data center, it's unlikely the data will ever be used. That's why surveillance camera data is stored at each location. The problem is the balance between the value of the data and the cost of transferring and storing the data. In many cases, it is enough to store the data at the edge of the network. We named the idea *Edge-Heavy Computing*. Edge refers to the periphery of a network. Okanohara and I jointly publish papers on the idea.

I think Okanohara remembered our discussion. The year after they founded PFN in 2014, they invited me to join. Unfortunately, I couldn't change jobs right away because I was leading two multi-year projects including a project funded by the Ministry of Education, Culture, Sports, Science and Technology, so I became an advisor to PFN for that period. I officially became a full-time employee of PFN in 2016 after these projects were completed.

PFN is specialized in research and development in deep learning, and it was already a top-class research group in Japan in the field. Deep learning can be considered as a branch of statistical machine learning, and its base is statistical modeling. I suddenly felt that my five years at the Institute of Statistical Mathematics had great value. With my consulting experience at IBM and my knowledge in statistics, I was able to help a number of customer projects and was also able to write papers and patents. To me deep learning is a way of doing programming, which we call *inductive programming*, where existing software engineering practices are of little help. With this in mind, I worked with other researchers to launch a new research area called "machine learning engineering" which combines the best of software engineering and deep learning.

Conclusion

I did research, but I also experienced product development, consulting, and management in business departments. Clearly, I am not a "a determined researcher" like Marie Curie and Professor Yamanaka. It may be wrong to call someone like me a “researcher.”

Nevertheless, I think it is yet another life of a researcher.

== End of Article ==

The important message is that my life was not preplanned. As with parenting, your life doesn't go the way you want it to. I think that's fine.

Chapter Summary

- Time goes by. Things change. Don't be complacent with your field, and make bold changes in your research field.
- Don't stay in the lab too long. Experience the outside world.
- Changing jobs is a great opportunity to learn. Always consider the possibility of changing jobs.
- When you are a student, it is important to acquire skills such as Mathematics and language. These skills will never become obsolete.
- Research is only part of your life. Consider the purpose of life.

Chapter V: Leadership

Even if a researcher continues in the role of researcher throughout his or her life, it is natural that he or she is required to demonstrate leadership as he or she develops into a senior researcher. Gradually, the higher your position, the higher your salary and the higher the expectations from you. On the other hand, the peak of the output that a researcher can produce as an individual may be in his or her 30s. After that, it becomes a style of doing bigger work by involving others with your own influence. This does not necessarily mean line management. Many laboratory projects are led by technical leaders who are not line managers. Project leadership and organizational leadership are the same in terms of "how to move people", or motivating others. So, even if you are not interested in line management, there will be times in your career when you need to learn leadership.

To exercise leadership, people should be willing to follow you. In other words, "to move people" is the ultimate challenge of leadership. Since I became a line manager for the first time in 1997, I've been thinking about "How to move people?" all the time.

I left Canon in 2010 and wasunemployed for about six months. At that time, I was asked to give a class on technical management at Graduate School of Engineering, University of Tokyo. Since I was unemployed at the time, I thought it was a good opportunity and I accepted it immediately. The title of the lecture was "Product Technology Development Management" and I soon found I was at a loss after accepting the class. I had experience in research management, but I had never learned systematically about product and technology development. However, I did have a variety of experience within technical leadership, including when it works and more importantly, when it doesn't. If such experience could be shared with the students in hypothetical settings, I thought it would be useful for these students of the Technology Management major of the Univ. of Tokyo who would be regarded as future technology leaders. I taught the class for four years and focused on letting the students think about various situations that are common in technology management.

The style of each class was to give a case of a hypothetical situation that the technical leader faces and let them discuss how he or she would handle the situation by themselves. This class seemed to be stimulating for the students, and also I could learn a lot from them. Let's use some of the cases from this class and discuss what it means to be a technical leader.

5.1 Make Decisions

The case we used for the 1st lesson was called “The case of Mr. Yano.” I think the situation covered is so common that every technical leader will experience it at some point in their careers. Please read the following case for yourself and think about what you would do in this situation.

Case "Case of Mr. Yano"

Mr. Yano is 38 years old. He joined the Endo automotive company 13 years ago after completing a master's programme in Control Theory at university and now works at the 2nd R&D headquarters as chief engineer. Currently, he is in charge of the ACC (Autonomous Cruise Control) project. ACC uses cameras and radar to assist the driver based on the surroundings and is highly anticipated as one of the key features of the upcoming new model, the 5MG. There are 7 people on the team in total including Mr. Yano. He does have a superior, the section manager, but because he has only just moved from the vehicle development office, he is not familiar with the electronic systems and, thus, Mr. Yano has the final say on the technological direction in the division.

The project they are working on is advanced, cutting edge technology, but because it has been incorporated into the design of the new car there is a deadline. Eighteen months have already passed since the start of the project and in order to meet a planned checkpoint they must fulfill certain requirements within the next three months. If they miss the deadline, the new technology will not be able to be used in the 5MG.

The project looks like it is going to run late. In particular, the highly anticipated millimeter wave radar sensor being overseen by Mr. Akimoto has failed to produce results. Having been with the company for just 4 years, he is inexperienced and young but is also the team's only expert in electromagnetic wave engineering. Mr. Akimoto is nevertheless determined to use his own method for the multipoint sensor that he is working on because he is sure that it will work. Mr. Yano secretly thinks that although the idea is good Mr. Akimoto will not be able to get the results he desires. Mr. Akimoto is a hard working young man and, as a leader, Mr. Yano thinks it is important not to damage his morale and so has supported and encouraged Mr. Akimoto to keep working on his project.

On the other hand, Mr. Yano has an idea using a single point laminated antenna that he thinks he could complete within 3 months if he dedicates 100% of his time to developing it.

The problem is, as a project manager, he has tasks to deal with on a daily basis and at most can only dedicate 30% of his workload to this development. If he had proposed the change to a laminated antenna around half a year ago he may have been able to work something out, but to starting now seems to be impossible. On top of this, if the antenna system is changed it would mean that the control circuits and recognition algorithms that the rest of the team are working on would have to be changed, thus creating further risk of missing the deadline.

== End of Case ==

What do you think? This is a very likely situation. The question is, as the leader of a company's R&D team, what decisions would you make if you were Yano? Before reading further, please stop here and think for yourself. If I were Mr. Yano, would you bet on Mr. Akimoto's method, or would you step forward and compete on your own idea?

In the class, I asked the student right after reading this case, and there were about half of the students who said that they would continue to use Mr. Akimoto's multipoint sensor and half who said that they would immediately switch to the single-point laminated antenna system, Yano's idea. However, when I ask further whether there are really no other solutions, various ideas, as expected from smart students of the University of Tokyo, were offered. If you were to switch to Yano's idea, you would have to have someone take over your project leader's job. You may be able to ask the section chief or the department head to bring someone from somewhere else. If the system was not making the required progress, it might possible to bring experts in the field, possibly from outside the company. Collaborative research with universities, or, more likely, with suppliers who provide sensors.

What made me even more excited was the idea that "First, we should negotiate with the product development division of 5MG to confirm whether the deadline 3 months ahead is really the final deadline." Many people are involved in the development of automobiles, and not all of them progress on schedule. Maybe the development of some other essential functions had been delayed. If so, even if Yano's team's ACC development is delayed by 1 ~ 2 months, it may not affect the delivery time of the whole product. That's a plausible story.

There are two things I wanted the students to learn from this case. One is **out-of-box thinking**. In the real world, we often find ourselves in a situation that seems like a stalemate. In the movie 'Star Trek', every Cadet Candidate must take a test called the *Kobayashi-Maru Scenario*. A private spacecraft called *Kobayashi-Maru* has engine trouble and you are stranded in the territory of the Klingon Empire, sending a distress signal. Trying to help them would violate the Armistice Treaty with the Klingon Empire, which not only could trigger a war but your own spacecraft could also be destroyed. The scenario was designed with the intention of letting the spaceship commander experience a stalemate situation, but Cadet Kirk, who later becomes the captain of the *Enterprise*, hacks the simulation program and changes the settings so that he can rescue the *Kobayashi-Maru* without being detected by the Klingons.

Indeed, what Kirk did to the training program was a cheat, but I think there is an important lesson here. In order to solve a really hard problem, it is often necessary to examine every condition without any presupposition. Students who are used to exams and computer games always assume that the given problem has a solution, they can be very good at finding the solution within the given preconditions. However, most of the problems we face in business are those where the solution seems obvious or doesn't exist. If there is no solution, it is an important strategy to doubt the preconditions.

One good tool for out-of-box thinking is to list all stakeholders. A stakeholder is a person (or a group) who will be affected by your decision. Of course, your team, especially Mr. Akimoto, is one of the most affected stakeholders in your decision making. But other team members or divisions that are looking forward to this technology as a key technology for the nehxt flagship 5MG are more important stakeholders. Others are indirect stakeholders, such as users of the product, and shareholders affected by the company's share price. Write down what stakeholders you have and how they will be affected by your decision. Then you may find that the preconditions that you thought were absolute may have some flex.

The other lesson from this case is the **self-contradiction of technology management**. What does this mean?

The central issue in this case is the design of the millimeter wave radar antenna. To judge which technology should be used requires a deep understanding on electromagnetic wave engineering. We shouldn't forget that Yano's specialty is control theory, and radio engineering is not his specialty. The only expert on the team is young Akimoto. As for radio engineering, Yano, an outsider, has to make a decision about the design of the millimeter wave radar antenna. Thus, when you make a decision as a technical leader, it is almost certain that your subordinates know better on the subject than yourself. You have to make a decision despite not understanding the details. This is a self-contradiction in technical leadership.

In fact, I have to confess my own bitter experience. I became a line manager on a small XML project in 1997. I had a small team of three people writing code, including myself. Since then, the team grew bigger, but I thought I would continue to be a playing manager. When I became the Lab director in 2006, I was not writing code anymore, but I still had a strong obsession that I had to understand all the research projects in detail technically - and I did every effort to do so.

When I moved to Canon in 2009, I was able to see a similar culture. Canon is a technology company. The Digital Platform Development Headquarters, to which I belonged, was a corporate R&D division independent from business units, and when overseas headcounts were included, it consisted of over 1,000 people. Each department was headed by a manager who had grown up as a technical expert in their own field. Everybody seemed to work hard to catch up with the latest technologies in the fast-moving field of information technology. However, from my point of view as a bystander, I could see painful situations where they had to make a decision while pretending to know the technology. I realized that this was exactly what I had done in my IBM days, and I greatly regretted it.

If a technical leader has to make decisions without knowing the technology, what should we do? Unfortunately, I don't have an immediate answer to that question. I think I can share with you two things. One is to humbly acknowledge that there are things that you don't know. If you are open to admit it your staff, you may find a better solution. The other is to be ready to take the accountability on the outcome of your decision. Technology is uncertain. It may or may not work. Even though you may fail due to factors outside of your scope of judgment, as a decision maker, you should still be responsible for the consequences of the decision being made.

5.2 Move People

The first thing a leader should do is to make a decision, but when a leader makes a decision, people don't always act accordingly. This is the hardest part of leadership. Leadership is, after all, the art of "How to make people move?" Consider the following case.

Case "Curse of Quality Control"

Delivering high quality vehicles to the market is a top priority for Endo Auto, because quality failure may result in a direct result loss of human lives. Endo Auto experienced a quality problem eight years ago that was widely criticized by the mass media, and now quality control is done thoroughly throughout the company. The 2nd R&D Division is no exception, because their technologies will eventually be embedded in the products.

Maintaining quality of software is by no means easy. There is no practical way to formally verify developed code against the specification completely. Instead, software quality is usually ensured by imposing rigorous controls on the development process. The 2nd R&D Division, the only organization producing embedded software in Endo Auto, introduced a software development process five years ago.

The Quality Control Department is a part of the division, and they are responsible for defining the process standard and enforcing it within the other departments. This standard decomposes the software development process into separate phases such as requirement definition, external design, detailed design, coding, unit testing, and system testing. Every project needs to be reviewed by the QC team before moving onto the next phase. The reviewer in the QC team checks the work products (documents, codes, test reports, etc.) to see if they meet the quality guidelines. If they do not, the project cannot proceed to the next phase.

Every work product of every project is archived in the QC database, and a weekly summary report of all the projects is submitted to the weekly meeting of Mr. Kato, the division head. This way, he can grasp the status of all the projects at a glance, and if he sees that some projects needs his attention, he can do so immediately.

So far the QC process has made great progress. Everybody knows that the software quality of the 2nd R&D Division has been remarkably improved. The resulting number of quality-related claims from the product divisions has been greatly reduced. And no significant quality problem has been reported in the products that were released after the QC process was introduced.

However, Mr. Kato is concerned with quality, because he feels that recently the claims from the product divisions are increasing.

The 5MG, the next generation model of the mainstream class of vehicle from Endo Auto, is something that Mr. Kato cannot have any mistakes on. The 2nd R&D Division is developing software for a consolidated control unit, which is supposed to control everything onboard from the engine, brake, suspension, to air conditioning with a single processor. It is a very complex piece of software and requires many technical innovations to be realized as a production-ready product. Thus, Mr. Kato is determined that his team will never fail in quality or delivery of this technology.

Mr. Kato started a new initiative called “Bug Zero” when the development of the consolidated control unit had started eighteen months ago. Each section had a QC workgroup where they discussed how to improve the quality. At the end of the year, the outputs of the workgroups were reported at the all hands meeting of the division, and the best teams were presented with awards. This is a division-wide initiative. Mr. Kato himself chairs the Bug Zero committee; each office has a large “Bug Zero” poster, and the ID badge of each employee has a small sticker showing the “Bug Zero” logo.

Everything was going well, until two months from the release date of the software. The head of the product division of 5MG called Mr. Kato and informed him that the division was giving up on the consolidated control unit and going to production with the traditional discrete units, the given reason being quality. This was a surprise to him. At the weekly meeting the reports had not been bad – a few minor problem reports were noted here and there, but overall the reports suggest the development was proceeding as planned.

Mr. Kato called Mr. Takahashi, the head of the QC department. He said: “That’s strange. Every QC review was done according to the book and all the records are reporting green. There must be some kind of miscommunication. I will check and get back to you quickly.” In fact, all the documentation (more than 1,000 pages total) are in place in the database, showing that all the work products passed the reviews.

Mr. Kato called a software engineer at his office. The engineer reluctantly said to him: “I was aware that the requirement definitions were a little vague, but I assumed that it would be taken care of by somebody in another team. At the stage of the system test, it became clear that there was a serious problem, but it was too late to rectify.” When questioned why he had not reported the problem earlier, he said “our part was completed as required, and our team had no extra bandwidth to increase our work scope on anything else.”

At the end, the consolidated controller was not adopted by 5MG.

== End of Case ==

Kato's decision to improve the quality of the consolidated control system was correct, of course. Unfortunately, contrary to Kato's order, the members of the Second Technology Development Headquarters did not seem to pay enough attention to quality improvement. Why? Again, please consider for yourself what happened before reading the following.

Let's look at it from Kato's eyes. It is clear that the software quality management process he introduced five years ago had produced some results. This process appeared to be working correctly, at least according to the records. The quality of the consolidated controller should not have been an issue. However, the evaluation of the product division said the opposite. It is possible that the evaluation of the product division was wrong, but let's assume that there was, in fact, a quality problem in the consolidated control system.

What does this quality problem look like from the perspective of the engineers? Their primary concern was to complete their work. This required two things: creating artifacts such as programs and documents, and passing the quality control reviews. No matter how good a product you think you've made, if you didn’t pass the review, you cannot proceed to the next step of development. This is why you can subconsciously find yourself creating easy-to-pass-reviews artifacts without being aware of it.

What does it look like from the reviewer in the Quality Control Department? It is impossible to scrutinize all the hundreds of pages of documentation and program code. As with accounting and security audits, this quality review focuses on what the reviewer thinks is suspicious and points out problems if there are any. If no problem was found, they assumed that everything is ok. Even as a reviewer, you don't want to be too picky. The reviewers are also ex-engineers in the same company, and they don't want to be hated by their fellow engineers due to being overly negative during the quality reviews.

The quality control process had already been in operation for five years. Therefore, over time the development engineer sees which points are regularly checked in detail by the reviewers. This prioritizes the team to make sure things that are checked by the reviewers are in good shape, to the detriment of everything else. So, the original goal of the QC process, which is to improve the quality, has morphed into another goal, which is to complete the artifacts to pass the QC checklists. In other words, the main cause of this problem seems to be that the QC process has been “fossilized”. It is no longer functioning as originally intended.

What are the processes and rules for? They are to make decision-making more efficient. Things like software quality are very difficult to measure. It's impossible to know exactly how many bugs remain (known bugs have already been removed, so remaining bugs are always unknown ones). For this reason, various alternative indicators are used to estimate quality, which must be evaluated over a long period of time by highly skilled experts. Instead of such time-consuming evaluation, "software based on a process is assumed to meet a certain quality" is an approach of **process-based quality control**. In this way, quality decisions can be made mechanically in terms of whether the process has been followed to the book. The same is true for rules. For example, the rule that says " expenses up to 300,000 yen shall be approved by the section chief" makes decision-making more efficient by delegating less risky decisions to lower-level management.

However, as we have seen in the above case, processes and rules also run the risk of missing the intent of the original goals. The original goal of improving quality became passing the QC process without being noticed for any problems. This means you are not giving due consideration to a problem. I think this is one of the reasons why many Japanese electronics companies that once dominated the world market are now in trouble. In the past, the Japanese manufacturing industry was field-oriented, that is, the field workers had autonomy in making decisions about how to improve quality and productivity in their production lines. Unfortunately in the 1990's, many Western consulting firms came to Japan and persuaded Japanese companies to adopt process-based quality control such as ISO 9000. However, the introduction of strict processes and rules deprived field engineers of the opportunity to think and judge for themselves. Field workers of a company that focuses too much on processes and rules are increasingly unable to do creative work. I call this **process fundamentalism**. That is why, in Chapter 4, if you are thinking about finding a job or changing jobs, you should check whether the company is falling into this trap.

There are two strategies for running an organization. The first is to define the roles, the rules, the processes, and the plan, and based on them to execute and evaluate. This is referred to here as **governance strategy**. Since everyone acts based on common rules and processes, overall efficiency is improved, and work with little variation in quality is possible. You'll find that a command and control governance strategy is necessary in an organization such as the military where every soldier has to exactly follow the given order, or in a mass production factory where the same product has to be produced with minimum variability.

The second strategy is not to decide detailed rules, but to decide only a few principles and leave judgment to each person in the field, and I call this the **empowerment strategy**. Since each person can move autonomously without being bound by the rules, it is easy to demonstrate the individual to execute and solve issues using their initiative, but it is sometimes difficult to show the integrated power of a larger organization. There is also the problem of business continuity when an employee suddenly becomes ill or leaves the company, due to the independence on individuals. Nevertheless, in workplaces where members are professionals and are expected to do creative work, such as research laboratories, it is important that the members are empowered and able to do the work using their own initiative while working within the leader's intentions.

Of course, governance and empowerment do not have to be mutually exclusive. Governance and empowerment are both important to move people. Without the minimum processes and rules, it is necessary to coordinate opinions every time a decision is required, which in turn negatively impacts efficiency. On the other hand, if the rules are too strict, it is difficult for individuals to take initiative. I think a good leader has to pay attention to the balance between governance and empowerment by using different governance and empowerment strategies where necessary, and letting members reach their potential towards meeting their goals.

Here, I would like to share with you one thing that I care about for empowerment. As discussed in Chapter 3, this is about the difference between persuasion and acceptance. When I became a corporate officer of IBM Japan in 2007, I attended an executive training course. Mr. Kanji Okubo, the lecturer of the course, had been a leader of customer satisfaction project at IBM Japan until 2000, but he left the company and became a consultant in executive training.

Okubo's seminar has a unique style called "Seminar of Awareness." He doesn't teach anything explicitly. He just tells stories and says, "Let's discuss what you think." Attendees think on their own and discover the root cause or a solution, which leads to **acceptance**. When you accept an idea, it's natural to execute it, but it's hard to do something that you don't really accept - regardless of how much someone else may try to **persuade** you to do so. When Mr. Okubo first became a management consultant, he taught a variety of methodologies, but at one point he noticed the difference between "persuasion" and "acceptance," and after that, the seminar took the current form of "Seminar of Awareness."

Because companies are required to produce results, the top management of companies tend to send out direct messages such as "Do this in order to achieve the goal". In the case of the above-mentioned Endo Auto case, General Manager Kato spearheaded the promotion of "Bug Zero." But it seems that the message "Eliminate bugs" did not necessarily reduce bugs. What would an engineer, who was told every day by the general manager to meet "Bug Zero," do when he found a bug in his program? The organization that is supposed to be "bug zero" may have become organizations where no bugs are reported. I think Mr. Okubo wanted to convey that if employees are empowered, enjoying their work, helping each other, and working motivated, then achievement of their goals will follow without explicit instructions (of course, Mr. Okubo did not directly say such a thing because it was a "Seminar of Awareness").

"The difference between persuasion and acceptance" as Mr. Okubo put it, is the essence of moving people. I try not to persuade. Instead, I always try to think how I can make people accept my goals. That's an important quality of a leader.

Company Values

Empowerment is based on a small number of principles. In line with these principles, people are encouraged to exercise their initiatives. How can we set an organization's principles? Here are some examples from IBM and PFN. IBM had corporate principles called "Three Basic Beliefs" that had been set by T. J. Watson Jr. in 1962:

- Respect for the individual
- Superlative customer service
- The pursuit of excellence in all tasks

I think each of these principles is important, but I didn't really notice them in my daily work. In 2004 these beliefs were replaced by the following *IBMer's Values*:

- Dedication to every client's success
- Innovation that matters—for our company and for the world
- Trust and responsibility in all relationships

Several things can be said from this change. First, the corporate philosophy has been changed from Basic Beliefs to IBMer's Values. Rather than talking about the company as a whole, the new principles talk about what the company expects from the behavior of each employee. The first value, "Dedication to every client's success," represents the changing nature of IBM's business. This is reflected in the fact that the word *customer* was replaced by *client*. A customer is someone who buys a product or service. On the other hand, a client is somebody who continuously receives professional services over a long period of time, such as a patient with a doctor or a client who consults a lawyer. It's a powerful reminder that IBM is turning its business into professional services like consulting and outsourcing. Second, they wanted to put priority on creating valuable innovations, even if not perfect, rather than pursuing the perfection of the existing products and services. I interpret the third principle as a statement of the importance of trust and responsibility arising from respecting individuals in and outside of the company. It is not known how much these IBMer's Values permeated into the consciousness of IBM employees, but at least when I was the director of the Tokyo Research Laboratory, I repeatedly reminded people of the IBMer's Values at every opportunity such as all-hands meetings, and I also often considered them in the daily decision making.

In PFN, as described in Chapter 3, we defined PFN Values in 2018 as the guiding principles of PFN. PFN Values consist of the following four:

- *Motivation Driven*
  "People perform best when they are motivated" is the belief behind this value. This does not mean "Just do what you want to do." Conversely, if you are not motivated by the work assigned to you, you should leave the company. I think it's that strong. Personally, I think it is the value that most differentiates PFN, as I'll tell you later.
- *Learn or Die*
  PFN's strength resides in its unmatched technology. As discussed in Chapter 4, each employee must continue to learn to stay ahead of the competition in this rapidly changing technology space. There was a heated debate on whether or not to include the strong word *die* in the PFN Values, which would be publicly visible, but we decided to use this word to show our strong will.
- *Proud, but Humble*
  PFN employees are world-class professionals in their respective fields. We should be very proud of that. At the same time we must also admit that we are amateurs in almost every other field except our own. We should respect each other with various backgrounds and various values.
- *Boldly Do What No One Has Done Before*
  There was a lot of discussion about what we should do with the latest technology. Of course, what PFN does should not be antisocial and should contribute to the peace and prosperity of human society. But that is generally applicable to any other company. Our mission in society is to do what only PFN can do.

PFN is a young company and will review its corporate principles as we grow. At each point in time in the future, we will look back at PFN Values and revise them as we see appropriate.

5.3 Evaluate People Performance

IBM, Canon, the Institute of Statistical Mathematics, and PFN have different evaluation systems, but there are common themes. Whether you're talking to individual researchers or you're talking to the management of another company's labs, one of the topics that always comes up is how to evaluate researchers. It is difficult to objectively evaluate research results. Some projects fail without any visible contribution to the company, despite great effort by individual researchers. How should this effort be evaluated? Consider the following case as a basis for discussion.

Case "Time for Performance Evaluation"

Mr. Kaneda hates December. In addition to all the work that needs to be done by the end of the year, he also has to complete the performance evaluation of his subordinates, including 80 engineers. Endo Auto employs the results-based approach for rating employee performance. At the beginning of the year, each employee sets his/her goals for the year with the manager. Then, at the end of the year their achievements are measured against the goals and each employee is given one of four ratings – S, A, B, or C. The rating S is defined as “Exceptional achievement,” A as “Exceeds expectations,” B as “Meets expectations,” and C as “Needs improvement” (There is also “Unsatisfactory” rating of D, which is only used for employees who are to be fired). The evaluation is directly linked to the employee’s annual income, and for middle-class employees the difference between the top and the bottom performers can be as much as $10,000 a year.

The evaluation is supposed to be based on an absolute scale, but in reality, the corporate Human Resource (HR) department instructs each organization that S should be given to 20% of the population, A to 30%, B to 30%, and C has to be given to the remaining 20%. This distribution is for the division level, and smaller units can deviate from the guideline – for example, a section with 10 people could have three S’s if the section as a team performed better than other sections, as long as the division-level distribution is maintained as instructed.

The results of the performance evaluation is communicated to the employees through a one-on-one interview. For a manager, declaring the rating S to an employee is easy. Telling an employee “you’ve got C this year” is not. Often, employees with the rating C do not feel they are being treated fairly. The interview gets longer and in a bad mood in such a case.

The evaluations of smaller units are adjusted at a higher level in order to maintain fairness. For Mr. Kaneda’s department, each of six section managers gives ratings of their subordinates, but then, at the department level those six section managers and Mr. Kaneda get together for adjusting the ratings of the 80 engineers in the department. This is a one-day-long, heated meeting. Every section manager wants to give better ratings to his/her own people, and this entire meeting is a series of negotiations such as “If Mr. X in your section gets S, certainly Ms. Y in my section should also get S." If no agreement among the section managers is reached, Mr. Kaneda has to make a final decision.

This year's adjustment meeting has progressed smoothly so far. But nobody seems to make a concession on the last slot of the "C" rating. There are four candidates for the low-performing rating: Ms. A, Mr. B, Mr. C, and Mr. D. You have to give the rating C to one of them.

Manager of Section 1: "Ms. A did her best this year. It was not her fault for not achieving the goal of the technology transfer. That was due to the delay of the product plan of the product division. I cannot give a C to her."

Manager of Section 2: "Mr. B's software release was delayed. However, he filed five patents this year, which is by far more than anyone else in the department. Do you really want to give a C to the ace inventor of the department?"

Manager of Section 3: "Mr. C may be a below-average performer, but he had set reasonable goals considering his ability, and he did achieve these goals. In his personal life, he has to take care of aged parents as well as two children who need higher education, and now is the hardest time in his life. It would be cruel to give him a C this year."

Manager of Section 4: "In recent years Mr. D did not catch up with the latest technologies, so this year I gave him auxiliary tasks to support other engineers. He is slow in completing assigned tasks but made no major mistakes this year. He received a C last year and Mr. Kaneda, you promised me that it was just for the year. I said exactly the same to Mr. D. How can I say that he is rated C again this year?"

Discussion 1: Who should Mr. Kaneda give an assessment C to?

== Case Suspends ==

In the class, I picked four students and asked them to role-play from the 1st section manager to the 4th section manager. I wanted them to feel that there are various criteria for evaluation. Whether to value the result or the effort, how much to evaluate the achievement which is not part of the main business, how much to consider the employee's private situation, whether to add in consideration to the past evaluation, etc. All these elements are considered through this simulated debate exercise. Of course, there is no right answer as to who should be given a C rating for this particular case. However, this exercise is the starting point for considering the following two issues.

== Case Resumes ==

After 8pm, the long meeting is finally over. The manager of Section 5 says in an exhausted voice: "The evaluation criteria is too vague in the first place. We should have an objective measurement system, such as five points for technology transfer, two points for patent submission, and one point for submission of a technical report. That way, we can explain the rationale of the evaluation to the employees.”

Discussion 2: Should the company introduce an objective (and quantitative) measure for evaluation? If so, what are these measures?

Discussion 3: How would you design a compensation system for your engineers to perform best?

== End of Case ==

In many organizations, the voice most often heard from researchers is "I want clear evaluation criteria." It is only natural that the evaluation should be transparent. Transparency, however, is the ability to explain why it has been assessed, which is different from prescribing goals such as "IF YOU DO THIS, A...". The evaluation based on the preset goals, or so called **performance-based evaluation** is the evaluation based on penalty not achieving goals, and it does not value unexpected results which were not in the preset goals. In the enterprise which introduced the performance-based evaluation, the annual goal of an individual researcher is set at the beginning of the year, and the researcher gets a high evaluation, if she/he achieves the goal. Once the system is well-understood, people tend to set lower goals at the beginning of the year so that they can easily achieve the goals. This, as I understand, was one of the reasons why the performance-based evaluation system that Fujitsu had introduced failed.

I think the performance evaluation should be **merit-based**, not penalty-based. If the researcher has achieved something valuable to the company, even if it has not been anticipated at the beginning of the year, it should be recognized. I think we should definitely grab an opportunity to make a big impact if we see it even though we didn't anticipate this when setting the goals. The world of information technology is moving fast. Even if you set a goal at the beginning of the year, the problem may have been solved by another research group in the following months and may have already been made available to everyone as open-source software. In such a dynamic world, it is no use to stick to goals that were set months ago. Research goals are different from sales goals for salespeople. The goals are moving targets.

But then, how can you explain the rationale of your evaluation, if the results are not in the agreed-upon goals? This is the harder part. As you can see from the mock review meeting, "the reason for the evaluation" is often vague and subjective. Even for things that can be counted, such as papers and patents, there are, without any doubt, large variations in the quality and the impact made by them. What would happen if you set objective quantitative evaluation scores, as section manager of section V said? Researchers are smart. They think about how to get the highest evaluation score. If the number of papers and patents counts, there would be more co-authored papers and patents. Even if you adjust the counts by dividing the number of co-authors, it would result in incremental (and less significant) papers and patents. It's a cat-and-mouse game. Remember what happened by introducing the quality control process in the case "The curse of quality control"? Even when you bind people to processes and rules, people always find loopholes. People will **overfit** to the rules, if we use the term in machine learning.

Then, what should we do in regards to discussion 3 "Compensation System to Motivate Engineers"? In this context, "Compensation" refers not only to monetary one but also to the mechanisms by which a company rewards an employee’s performance. In English, the word “recognition” is sometimes used to refer to how to (for example, in the form of money) convey the message "The company is aware of your contribution." In fact, many companies already have multi-dimensional recognition mechanisms. I think we should use them wisely. For example, consider this:.

- **Achievement of the year**: reflected in the annual performance review
  The results here mean the output, no matter how hard the employee tried. It might have so happened that the employee performance was good just because they were lucky, such as the client's business which the employee was in charge of happened to be good and led to a big sales. On the other hand, even though the employee did a very good job, the idea of the paper you submitted might have been published by another research group just prior to you submitting for publication, and it might not have led to the results. Performance evaluation for the year is based on results, not on efforts.
- **Ongoing contributions**: reflected in salary increases
  In general, salary raises are linked to annual performance reviews, but still leave some room for line managers to adjust. Continuous and stable contributions to the company can be recognized in salary increases, even if the individual’s yearly performance is average.
- **Individual's potential:** reflected in promotions and assignments
  For researchers and engineers, being assigned to more important and challenging work leads to increased motivation. Even if the employee happens to have no luck in research output, you should give high potential researchers opportunities to take on challenges.
- **Past contributions revealed later**: Commendation
  Suppose that a patent an employee filed 10 years ago has found a completely different application and has brought a large amount of IP revenue to your company. Such past contributions cannot be regarded as the results of the year. You should use the company's commendation system. At IBM, depending on the level of the contribution, the award could be as much as several million yen.
- **Retention**: stock options
  If the market value of the researcher is high and there is a possibility that the person changes jobs, it is better to use a reward provided under the condition that the person stays for a certain period of time, such as stock options.

When the evaluation of subordinates is decided by the group evaluation, it must be communicated to the person. One thing to keep in mind when you do this is to make that assessment your own. Don't blame others, e.g., saying "I objected, but your rating was C by the senior manager's instruction." Of course, since the evaluation is multidimensional and subjective, it is natural to have various opinions. You can say that. But what is determined by management consensus should also be your own conclusion. If there is any ambiguity in communicating the evaluation results, it may cause a problem later. So be careful.

I want you to know one more thing about compensation. What motivates people? Figure 11 shows what is called Maslow's Hierarchy of Needs. People try to satisfy their physiological needs at the bottom of the pyramid. Appetite, sleepiness, sexual desire. When they are satisfied, there is a further need for safety. These two are the material needs, and above there, there are more mental needs, "belonging and love" of not wanting to be alone, and when it is satisfied, "need for esteem" which is provided by recognition in society. At the top is "desire for self-actualization." It is important to note that financial rewards are primarily related to material needs. Frederick Hersberg, an American clinical psychologist, calls this a **hygiene factor**. If the hygienic factors are not satisfied, there is significant discontent, but once satisfied at a certain level, there is no additional motivation by having more.

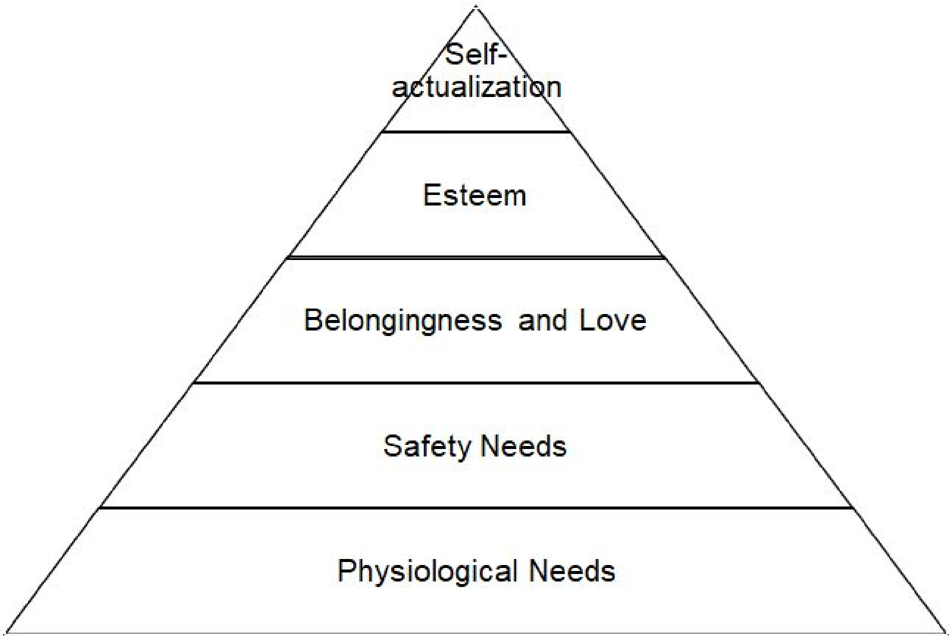

Figure 11. Maslow's Hierarchy of Needs

The real motivation for people (called the **motivational factor**) comes not from monetary rewards but from the recognition of others. When your boss raises your salary, at first you think you're recognized, but that's only temporary. It should be noted that an increase in salary above a level that satisfies a hygiene factor does not constitute an ongoing motivation factor. Psychologist Daniel Pink also points out in his TED Talk titled The Puzzle of Motivation that financial rewards actually hamper solving creative problems. In the case of researchers, the strongest motivation is that they can grow, that is, being in a good research environment and being able to work hard with excellent researchers.

Finally, I would like to introduce one bold idea about evaluation. *The Art of Possibility* [Ref. 18] authored by Benjamin Zander explains a practice of giving everyone an A rating.

Zander, director of the Boston Philharmonic Orchestra, teaches music in classes at the New England School of Music. In the first class of the term, he declares, "I will give everyone an A rating". "However", he continues, "please write a report now, with the first sentence starting with “I got an A grade in Mr. Zander's class, because...'. What do you think? Is it possible?

Behind this practice is the strong belief that if you believe in the students, make them believe to achieve, and help them, anything is possible. I'm sure it is. However, to implement this practice you must be very determined. Of course, in the case above, performance evaluation in a company is usually relative, so technically it is impossible to give everyone an A rating. Although this may be the case, your company may have an informal system of recognition that is independent from the formal systems. I think at the heart of this practice is to respect everybody and give an A rating to everyone around you. Not only to your subordinates, but also to unnamed people around you, such as janitors, security guards, and taxi drivers, you can give them an A rating secretly in your mind, praising that "Oh, she's doing a great job".

I think the management of the laboratory should give the maximum evaluation to every researcher. That's what leadership is all about. If you can't get the most out of your team, it's your fault. On the other hand, from a management point of view, we still have to rate people. I think that it is a never-ending journey for the management on how to carry out a multidimensional and subjective evaluation. This is a matter of human values, and no matter how advanced technology is, it will be impossible to replace human managers.

5.4 Manage Uncertainties

Time is special in the physical world where we live. Though we can move from here to there freely in three-dimensional space, in our timeline, we have to move in a single direction, from the past to the future at a constant speed. Like it or not, you can't go back to the past or see what will happen in the future (at least until a time machine is invented). We're always living in a world of uncertainty where we don't know what will happen in the future, and we can't undo our decisions from the past.

In the management of research, uncertainty is the norm. After all, research is about doing things you don't know are possible for you or not. It is development, not research, to build a technology that you know you can. If 90% of a laboratory's research projects are successful, I don't think it's a successful laboratory. I consider it as not taking on enough challenges and only developing small pieces. I think it is ok if the failure rate of the projects is 50%, although it depends on various factors such as the business situation of the company, the role of the laboratory, the research ability of the laboratory, and the size. DeepMind, a UK company acquired by Alphabet, spent nearly 60 billion yen in 2018 and, as a result, provided only a small amount of value to its parent company Alphabet (Google), including data center optimization. Even so, it has produced remarkable research results like AlphaGo.

Management is the science of dealing with uncertainty. How to manage uncertainty is a well-studied theme in management science. The know-how for dealing with rare events among uncertainties is called **risk management**. In risk management, you discuss how to prepare for rare events, such as natural disasters and how to respond to them[19]. On the other hand, when you are prepared to do business with many failures, such as research, it is called **portfolio management**. A well-known example is trading of stocks. The risk of losing the entire investment is minimized by diversifying the risk by investing in stocks that may or may not go up.

The portfolio approach is important in the management of research. The central idea is to admit that some research projects will fail, and then maximize the chances of generating great successes from other projects. For that purpose, it is important how to combine high-risk, high-return projects, which seem difficult but can produce large impacts if they succeed, with low-risk and low-return projects, which can produce incremental, but certain results.

Consider the **three-horizon model**, McKinsey's framework for innovation management. Horizon 1 are solid projects that contribute to existing, profitable businesses. In the case of corporate research projects, they are projects that improve existing technologies based on the requests of business units and customers. Research results almost certainly lead to business. Horizon 2 is a contribution to a business plan that has not yet started as a business, but is expected to grow rapidly if it succeeds. The risk is high, but if it is successful, it leads to a large profit. Horizon 3 is a basic research that is not yet known how it can be used for business.

Portfolio management in research is to decide how much management resources to invest in Horizon 1, Horizon 2, and Horizon 3. In laboratories that are strongly required to contribute directly to the business, the proportion of horizon 1 is high. Companies looking for rapid growth in the near future will need more investment in Horizon 2. It is also possible to increase the research share of Horizon 3 if the company has spare capacity.

In research departments of mature companies such as IBM and Canon, the proportion of horizon 1 is large. At the time I was in IBM Research, the percentage of horizons 1, 2, and 3 was maybe about 5:4:1. About half of all the researchers were involved in projects that led to immediate business contributions. If the percentage of the Horizon 1 project exceeds, say, 70%, I don't think it is sound for a research lab. In such cases, lab researchers should suspect that the business unit is using it as an inexpensive in-house development resource. Horizon 1 research projects may be popular among researchers because the results are easily visible. But the real value of a company's research lab is to contribute to the company's future growth. Investment in horizon 2 and 3 should not be neglected.

On the other hand, if a company has extra strength, it is possible to create a research portfolio focusing on horizon 3. AT&T's Bell Laboratories (now a part of NOKIA), which is known for the invention of transistors, the C language, and the UNIX operating system, and

[19] Pay attention to the distinction between manage and control because both are often translated into "management (kanri)" in Japanese. **Control** refers to adjusting parameters for preventing undesirable situations (as in "quality control"), while **manage** refers to managing any unexpected situations, both desirable and undesirable.

Xerox PARC, which is known for the invention of Ethernet, mouse, personal computer, etc., are known for their horizon 3 research projects in which their business contribution were not seen. IBM Research defined its mission as "Famous for its Science and Vital to IBM's Business" until the 1990s. In other words, achieving fame in science, or horizon 3 projects, were considered to be aligned with business contributions. At present, DeepMind can be said to be a horizon 3 laboratory.

The start-up research portfolio is very different. Start-ups such as PFN, which are externally funded, are expected to grow rapidly. Therefore, the high-risk, high-return projects must be positively pursued, but the return should be expected within a few years. Therefore, many research projects are at horizon 2.

A high level of commitment is required to carry out high-risk research projects. In particular, start-up companies that have limited resources and are unable to build a sufficient portfolio have to bet everything on the success of a particular project, knowing the risks involved. I think it takes a lot of courage. PFN President Toru Nishikawa makes such decisions every day. I am sure I could never do that myself[20].

Many high-risk projects should fail, but my experience at PFN suggests that the key to success of high-risk projects is in **flow management**. Flow is a concept proposed by psychologist Csikszentmihalyi, and it means that people tackle one task in a heightened state of mind. It is said that in the early days of Sony, engineers worked enthusiastically on innovative projects and succeeded one after another. This is flow management.

In the summer of 2014, Eiichi Matsumoto, an internship student at PFN, showed that he could control model cars autonomously using a technique called deep reinforcement learning. PFN decided to demonstrate an extended version of this technology in cooperation with Toyota at CES (Consumer Electronics Show) held in Las Vegas in January the following year. However, reinforcement learning is a difficult technique, and it does not always produce the desired result. After all, almost all of the PFN employees at that time went to Las Vegas to continue the development and complete the final fine adjustments while being there. The system was completed just before the demo day and the demo was a huge success. PFN has many more examples of such last-minute successes, including the Amazon Picking Challenge robot contest (second place but the base point was tied with the top) in the following year, and the demonstration of autonomous drones at CEATEC, a national trade show. I could not believe these consecutive successes of high-risk projects. I was probably witnessing flow management.

5.5 Roles of Staff Members

Before we conclude the chapter on leadership, let's talk about the roles of staff. The support of staff members is essential to be an effective leader. When I was the director of

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[20] Read Ben Horowitz's *Hard Things* [Ref. 19] about how start-up founders spend heroic days.

IBM Tokyo Research Lab, I could carry out my management tasks thanks to the support of excellent staff.

IBM teaches the concept of **completed staff work** as to how staff members should work. "Completed" here means that the work you do as a staff member must be completed in the sense that all that's left for the boss is to say yes or no. Let's say you're a staff member and the boss is making a proposal to bring to the customer tomorrow. If the boss looks at PowerPoint slides and says, "change here this way," then your job is not completed. In order to make it a completed staff work, it is necessary to anticipate exactly what your boss would say, as well as to make sure that there are no omissions or inconsistencies in the materials.

On the other hand, if you are the boss, you should not touch the materials prepared by your staff even if you find something, unless it is really, really fatal. If you lose your face in front of your customer because of a misspelling in the slides, your staff who made the slides will never forget it. They will make no mistakes in the future. On the other hand, if your staff knows that you will always check and modify the "final" document they prepared the night before the presentation, your staff will lose motivation to do their completed work.

In his book *What Got You Here Won't Get You There* [Ref. 20], Marshall Goldsmith states that one of the things you should not do as a boss is to "add value." When your subordinate brings up an idea, you may find a thing or two to make it better. But you must not say so. Sure, the idea may get a little better, but it kills the motivation of the people who brought it in. It doesn't have to be perfect. What you should say is, "Very well. Do it".

Back to the role of staff members in leadership. **Followership** is a form of leadership. Check out Derek Sivers' famous 3-minute TED Talk [How to start a movement](). When someone comes up with a new idea and starts demonstrating leadership, a follower has to have the courage to follow in order to make it a new social movement. Similarly, in order to win a business, you can't do the same things as others, and you have to do things that others would say are crazy. At the end of 2016, Nishikawa of PFN declared to build an in-house data center for deep learning. It would have 1,024 of Nvidia's latest GPUs. The list price of one GPU is 1 million yen. The GPUs alone would cost 1 billion yen (or ~10 million US dollars). It was unprecedented for a start-up with at most 60 employees to build such a huge data center. I couldn't believe my ears. However, we, including Yamamoto, our CFO, made every effort to realize this vision. As a result, PFN, through the work of Akiba and his group, was able to establish the world's record in ImageNet's training speed as I mentioned in Chapter 2, and thus, PFN was recognized as a key world player in deep learning. As Derek Sivers puts it, followership is an important form of leadership. I want to have the courage to follow a leader who has a vision that everyone thinks is crazy.

Chapter Summary

In English, there is an expression - "learn something the hard way." Each of the cases presented in this chapter is one that I learned from my management experience in the "the hard way." I have hurt people and been resented by people many times as a result of my decisions. It was also a very painful experience for myself. I've often wondered why no one had told me that this is what it means to be a manager.

You will have similar experiences in your life. I think, as I did myself, you will learn the same lessons described in this chapter. There is an argument that unless you learn something the hard way, you don’t really learn it. I'm sure that is correct. At least through the cases in this chapter, I think you will be a little more prepared for similar situations. Then, you may recall the contents of this book. If that happens, I think the value I wanted to offer in this book will have been achieved.

Just as there is no cookbook for good papers or good research, there is no right answer for leadership. Leadership is a concept that only exists when there are many people following the leader, and the relationship must always be changed flexibly according to the people you are leading. Learn from the best practices introduced in this chapter, from other people's experiences, and find your own leadership style from your own practices.

- A technical leader must often make decisions about matters outside their own expertise.
- You can't move people by just showing them goals. They should accept the goals as their own.
- Always pay attention to the balance between governance and empowerment.
- People are evaluated by the additive method. Evaluation must be multi-dimensional and subjective.
- It is natural that a research project fails. Take a broad view of the portfolio.
- Without good staff, leadership is not effective.

Chapter VI: Intellectual Property, Contracts, Integrity

In this chapter, patents and copyrights are explained as the minimum intellectual property rights that researchers should know. We also discuss contracts and integrity.

6.1 Intellectual Property Rights

Intellectual property is property that is intangible, such as an idea or a written text. Inventions, copyrighted works, trademarks, trade secrets, etc. can be protected by intellectual property rights. There are two points that companies and researchers should be aware of, regarding intellectual property:

* Protect your intellectual property rights
* Do not infringe the intellectual property rights of others.

First, let's look at the types of intellectual property rights.

Patent Rights

A **patent** is a legal protection for an invention. An **invention** is a technical idea with the following two conditions: **novelty** (not known to the public until the point of filing) and **inventive step** (not something that anyone can think of easily). For an invention to be protected as a patent, it must be **filed** and then **registered** through examination by the Patent Office. It is likely to take at least a few months, or even several years, from filing to registration. After a certain period of time (18 months in Japan) from the filing of the application, the invention is made public. Once a patent is registered, the patentee is granted an exclusive right to use the invention for a period of 20 years from the filing of the application. If someone else uses the invention in the meantime, they can be sued for **patent infringement**.

If the same idea was invented independently by different companies or individuals, the one who filed it first has the right. Even if you make a patentable invention, if you are late, other companies might make the same invention and file an application first. Therefore, if you come up with a good idea, you should first consider filing a patent application before writing a paper.

Patent applications usually require a specification of several tens of pages, and the composition and presentation of the specification is unique, so the researcher is responsible for writing the specification jointly with the person in charge in the Intellectual Property Department or an external patent office. If an examination by the Patent Office rejects an application as unpatentable, a document must be prepared and explained to counter the rejection. This is a very time-consuming task for researchers.

Thus, many procedures must be followed in order for an invention to be granted as a patent. It also costs to maintain patents. If you want to file a patent abroad, you have to repeat this for the number of countries in which you apply. Therefore, from the filing of one patent application to the expiration of the right, the cost is at least at the order of million yen. Add in the cost of the researchers or the intellectual property person who makes the specification, and it's a substantial amount of money. Therefore, when an enterprise files a patent, it is necessary to carefully examine whether the patent will produce enough value compared to the cost.

Copyrights

While patent rights protect inventions, or ideas themselves, **copyright** protects the creative expression of ideas (**works**), such as texts, diagrams, programs, papers, and books. Because work is an expression, a text that contains the same idea is a different work if the expressions are different. Similarly, the same contents of some text can be represented in the form of a diagram which would be considered as a separate work.

Whereas an invention protects only what is registered as a patent, copyright automatically engenders rights to the author when the work is created. Copyright, as it is called in English, is mainly the **right of reproduction**, but it also includes other rights such as the **right of public transmission** (e.g., right to distribute on the World Wide Web). In addition to copyright, **moral rights of author** (rights not to be altered without permission, etc.) also arise in the creation of a work.

In the case of a researcher who works for a company, the company automatically becomes the author of a work created at the company's initiative (**work made for hire**), unless otherwise stipulated. The copyright of a paper written within the scope of the company's business therefore belongs to the company. Recently, academic societies often request the transfer of the copyright, when the contributed paper is published in the journal. This is because they want to get the right not only to publish the paper in the journal but also to distribute it on the Web page of the society. In this case, the company needs to transfer the copyright to the academic society. Even if the copyright is transferred to an academic society, in many cases, it is permitted for individuals to copy their papers and send them to related researchers or post them on the Web pages of their own organization. In this respect, there seems to be a common understanding in the academic world that papers should be treated as public goods so that they should contribute to the advancement of science. So you don't have to think too much about protecting the copyright of your papers.

On the other hand, when you write a paper, you have to be careful not to infringe the copyrights of others. A paper may refer to another paper. Appropriate citations satisfying the following conditions are permitted by the Copyright Law: (1) the source is clearly stated, (2) the scope of citations is clear in the author's work, (3) there is a logical reason to cite the portion in the argument of the author's work, (4) the author's original parts are main and the citations are secondary in the author’s work, and (5) the cited parts are unchanged from the original. There are rich contents available on the net that you may want to use, but you should not cut and paste without considering their copyright.

You should also be careful when writing programs. There are many sample programs on the net these days, so you may want to run them first and then modify them as you develop your code. However, care should be taken to ensure that no code ultimately used in your company's product infringes another's copyright.

Trademark Rights

On the back of the iPhone, there is an apple-with-bite mark. By seeing it, everyone understands that it is an Apple product. An easily recognizable sign or design indicating a particular business, product, or service is called a **trademark**. If other people can use the Apple logo freely, Apple will have trouble differentiating its products. For this reason, a trademark is granted an exclusive right of use by law if it is filed and registered in the same way as a patent.

Trade Secrets

A **trade secret** is information held by a company that is 1) controlled as a secret, 2) useful for business activities, and 3) not publicly known. For example, the list of customers and technical contrivances will automatically become trade secrets if these three conditions are met. The Antitrust Law prohibits the illegal acquisition and use of trade secrets of other companies. In fact, most of the intellectual property that companies hold in their R&D are so-called **know-how** that are not protected by patents or copyright. Know-how must be protected as a trade secret under the present legislation. Therefore, it is necessary to pay attention to the above three conditions.

Companies and Intellectual Property

Why is it necessary for companies to respect intellectual property rights? There are three reasons. One is to **differentiate products and services** by protecting intellectual property rights. If a product or service requires the use of a particular invention, obtaining a patent can prevent competitors from entering the market with the same product or service. If it depends on specific software or data, it may be considered as an entry barrier by protecting them with copyright. If it is difficult to protect by patent or copyright, it should be kept as a trade secret.

The second reason for emphasizing intellectual property rights is defensive in order to **secure a degree of freedom for products and services**. If a technology necessary for a product or service is patented by another company, the freedom of the company to use the technology will be reduced. In order to keep your freedom, you can file your own patent, or if the cost of patent filing is too high, you can also publish your invention. Nobody is granted a patent if the idea is public knowledge. You can publish ideas through either publications, standardization, or open source.

The third use of intellectual property right in a company is **IP business**, i.e., offering technologies to customers. What many IT enterprises (including PFN) do can be widely regarded as an IP business because what we provide to our customers, in most cases, are intangibles, such as software, new business processes, licenses for inventions and ideas, or undocumented know-how.

As another form of IP business, even if there is no possibility of using the technology in a company's own products and services, the company can use their IP portfolio to claim royalty income by licensing patents, selling patents, or sueing other companies who infringe on the patents. As I will explain later, I have a negative view on the use of intellectual property this way because it impedes innovation.

Researchers and Intellectual Property

Then, what are the merits of invention disclosure for us individual researchers? Some companies have quotas or incentives for patent applications, which can be an incentive for invention disclosure. I tend to think there are additional advantages for researchers. When an invention disclosure is written and sent to the Intellectual Property Department, the first thing the Intellectual Property Department does is determine whether there is any prior art. As discussed in Chapter 2, the investigation of related research is always an inevitable step for researchers. If you write an invention disclosure, professionals in the IP department will search the prior art for you, and may find something that you have overlooked. Even if the idea turns out to be public, you can get a lot of valuable information about what related ideas exist, which fields they can be applied to, and what companies and researchers are working on in those fields. There is no reason not to use the help of the IP professionals.

How do you write a good invention disclosure? As with papers and presentations, it's good to read a lot of good patents. If you have a chance to participate in a co-worker's invention panel, go and listen intently to the discussion. In the same way, you should try to write many invention disclosures yourself. If your invention disclosure is more likely to result in a patent application than someone else's, you shouldn't be happy, but rather reflect that you have more capacity to write more invention disclosures. Write more. Even if the invention is not selected for viable patents, there is a great advantage for researchers to document their ideas. Your idea will be clearer by documenting it, and the idea may be found by somebody else, somewhere else at some point in the future. Remember, we had the same discussion on “why writing papers” in Chapter 2.

If you disclose many inventions, that means you did not stick to one idea. I have been involved in many intellectual property lawsuits as a third party subject-matter expert appointed by the Intellectual Property High Court. In one case, an action for infringement of intellectual property was filed by the plaintiff who was the right holder of intellectual property, against the defendant who had used the patent without permission. The plaintiff was demanding suspension of the invention use and payment of damages. Of course, infringement of intellectual property rights should not be tolerated, but it seems that in many cases it is difficult to judge whether it really constitutes an infringement. I hold dozens of patents myself, so I know how the plaintiff feels about being infringed, but if you have the energy to fight for your past ideas, I wonder why you do not use that energy to come up with more new ideas. At least I want it to be that way.

Problems with the Current Patent System

For 10 years from 2005, I was a subject matter expert who was appointed by the Intellectual Property High Court. A subject matter expert gives advice to judges when a dispute requires deep technical knowledge, such as in a lawsuit concerning intellectual property. When I worked for IBM and Canon, I was often evaded by the parties involved for potential conflict of interest and was rarely called to court, but when I was at the Institute of Statistical Mathematics, I was involved in many trials.

In one case, an overseas NPE (Non Practicing Entity) filed an injunction against a telecommunications company, alleging infringement of its patent. An **NPE** is a legal entity that holds a patent but does not exercise the patent themselves. I don't think all NPEs are bad, but so-called patent trolls are typically NPEs. The NPE filed a patent covering claims for e-mail technology in Japan that they had purchased from a large German company many years earlier. As soon as their patent was granted in Japan, they filed a lawsuit against the defendant for an infringement. It was obvious that the defendant's carrier had just entered the market and was not ready for such a lawsuit. Prior to court, the plaintiff and the defendant held a technical briefing to explain their respective positions, but it was obvious to me as a legal amateur that there was a difference in the levels of experience between the plaintiff's attorney and the defendant's attorney. Although it was obvious to me that this e-mail patent had been publicly known at the time of original filing in Germany, the defendant's attorney focused their arguments on whether the defendant's system met the requirements for the infringement of the patent, and did not pay attention to the validity of the patent in the first place. Unfortunately, an expert cannot comment unless questioned by a judge. The judge only considers issues presented by the plaintiff and the defendant. So I was listening to the briefing with regret. From what I later heard, the NPE seemed to choose the weakest of the companies that may be infringing on their patents. If, by any chance the defendant lost the injunction, they had to get out of business. Due to this, companies without a strong legal department tend to prefer a settlement by paying some money to the NPE before they get sued.

As a recent example, there was a case where an NPE in the US called Sound View Co., sued many user companies who used open source software that involved patents owned by Sound View. These patents were originally invented by AT&T's Bell Labs and used in Hadoop, an open-source massively parallel middleware technology. Lucent Technologies, the successor of Bell Labs, took over these patents, and then Sound View bought them. The company filed a series of lawsuits against Delta Airlines and other user companies, alleging infringement of the patents. Sound View seemed to have hoped that these user companies would not challenge the dispute of infringement because the user companies were not technically familiar with these technologies.

In light of these examples, I am afraid that the current patent system is not very effective in terms of achieving its original goal -- to promote invention. The following two points should be noted:

1. Penalties on Use
   Under the current patent system, a person who uses another's patent without knowing it is punished, but there is no penal provision for a person who has a good patent but does not use it. Good patents are common property of the humanity. The more widely they are used, the more they contribute to human society.
2. Unlimited Claims
   Most modern systems contain software, and most pieces of software use many patents. It is virtually impossible to check all those licenses in a comprehensive manner. If even one patent that has been overlooked falls into the hands of a patent troll, no matter how minor it is in the overall system, the patent troll can claim a business injunction or substantial damages. This is a major risk for business operators.

How can we deal with this problem? One idea is IP Marketplace that IBM discussed and proposed as a topic in a Global Innovation Outlook (GIO) report. Here's the idea:

- The patentee shall declare the license fee for the patent upfront.
- Anyone who wants to use the patent can buy a license at that price.
- The patentee shall pay XX% of the value thereof to the Government as a "fixed asset tax."

If a patentee wants to use the patent exclusively, a very high licensing fee can be set to prevent entry but they also must pay a high property tax. This can be considered as a penalty to disincentivize monopolies forming (while also minimising low utility in the IP asset). This is the solution to problem 1 noted above.

If an invention has price elasticity (if you cut your licence fee to 1/2, you can expect more than twice the licensees), a large amount of income can be obtained by setting the license fee low. This is the idea of introducing transparency into the licensing of patents, and a reasonable licensing fee is set by the market principle. This solves problem 2 above.

A marketplace patent system is a good idea, but I don't think the current patent system will change anytime soon. One short term solution that I think is effective in fighting patent trolls is **LOT** (Licence on Transfer). In LOT Network, which is a nonprofit organization realizing this concept, any member has to sign a contract saying "If any of my patents falls into the hands of a patent troll, all other members are automatically granted a free license of the patent." As long as the patent doesn't fall into the hands of a patent troll, LOT changes nothing, so as long as everyone keeps the patent in their own company, there is no difference from the existing patent system. However, if any of the member's patents fall into the hands of a patent troll, all of the members who use the patent invention receive a free license to the patent in question.

6.2 Contracts

The industrial structure of the world is rapidly changing into the service industry. IBM used to be a manufacturer and was selling computers as its main business, but now more than half of its sales are from their services business. What you think is a manufacturing company may become a service company in the future. What should researchers know if they are expected to invent and research for the services business, not through products?

One important thing to know is that research activities may be tied to contracts with customers. For example, your company may have a service that optimizes the customer’s data center to reduce carbon dioxide emissions. To do so, you as a researcher have to do many things. From a survey of the customer's data center, suppose that you find that you could use the semiconductor cooling technology and an optimization algorithm for power consumption that is being developed in your lab by your colleague. Therefore, your fellow researchers have to visit the customer’s data center for further investigation. Based on the data disclosed by the customer, they can improve the existing technology, modify it to fit the customer's data center, apply it, and finally achieve the desired CO2 reduction. This is certainly an excellent work, and it is a task that corporate researchers should do. At the same time, however, researchers must pay close attention to the customer contract. This is because there are obligations arising from contracts with customers.

In such professional services as consulting, system integration, outsourcing etc., in the information industry, you should be careful about two things, regarding the contracts: confidentiality and intellectual property.

The duty of **confidentiality**, or non-disclosure agreement (NDA) is, as the name implies, a commitment to not leak customers’ confidential information. Customers will trust your company and disclose confidential information. For example, here is the 365 day, 24 hour utilization data for each customer server. Very interesting data. If you look closely, you can save a lot of energy by rearranging servers or changing their roles. To that end, you would like to hear the opinion of your fellow researcher, Mr. A, in a different section. Can you show this customer's data to Mr. A? The answer is, "depending on the contract". If the NDA says that disclosure is limited to the project’s members, disclosing this data to Mr. A would be a violation of the contract. If it is a contract that says “May be disclosed internally to the extent necessary to carry out the project," there will be no problem to show it to Mr. A. Then, can you show it to Mr. B, who is a member of this project but is only affiliated to your company as a systems engineer of a subsidiary company of yours? The answer is "depending on the definition of 'internal.'" Of course you don't show the data itself, but can you tell others the essence of what it means, such as "The customer's business peaks on December 23 each year?" Or can you write a paper about the project three years after the project is finished? The answer to all of these questions is "depending on the contract." Therefore, researchers need to know the content of contracts if they are engaged in a service project with a customer.

There are not so many variations in arrangements for intellectual property, but it is necessary to understand them well. For example, suppose that you come up with a new idea and the invention was in some way thanks to insights gained by looking at the customer's data during the project. Of course, without a contract, legally, the invention automatically belongs to the inventor or the company to which the inventor belongs. What does the contract say? Check it out.

There are other things you need to know about contracts other than those with customers. We have to consider works on the Internet, especially code and data. The use of these intellectual properties is often restricted by the user agreement. For example, academic datasets are often not approved for commercial use. Also, it should be noted that if you use open source code as a part of your system, the open source terms of use may also be applied to the whole of the system. If you are unsure, consult with your IP or legal team.

6.3 Integrity

Patent and contract matters are related to the law, and ultimately any dispute will be settled by a lawsuit. But there are other ethical codes of conduct which must be observed by researchers and engineers. Observing such a code of conduct is sometimes called integrity.

Integrity of Researchers

In computer science, the word “integrity” is often used to refer to completeness and correctness of data and systems, but when used for humans, it is used to mean honest and with strong moral principles. We would like to be a “researcher with integrity,” always honest and acting with strong moral principles.

Unfortunately, there seems to be some **misconduct** by researchers recently. One such incident was the scandal of STAP cells. At the time of this writing, the consensus is that the STAP cell papers were based on fabricated data.

In 1999, Victor Ninov of Lawrence Berkeley National Laboratory published a paper in *Physical Review Letters* in which he claimed to have discovered an element with the atomic number 118 by impinging krypton ions on a plate of lead. In fact, the first author, Victor Ninov, was found in 2002 to have fabricated the data. It appeared that the other 14 co-authors of the paper had failed to double-check the data.

Of course, these papers were peer-reviewed. However, reviewers usually trust that the author has been honest about the correctness of the presented data the author claims to have obtained from properly conducted experiments. So I think it's relatively easy to write a high-profile paper if you try to fake the data to fool reviewers.

The main misconduct in research are categorized into:

1. **Fabrication** of facts,
2. **Falsification** or intentional **concealment**, and
3. **Plagiarism** of others' ideas and achievements.

You MUST NOT do any of the above when writing a paper, when writing a research proposal, when reviewing other people's research, or when writing any other reports.

Of course, people make mistakes, so it is inevitable that there will be mistakes in the work done in good faith. Therefore, if a person who has acted in good faith makes a false report as a result, it is not considered misconduct. However, if a mistake is noticed, it should be corrected immediately and completely. For example, if you find a mistake in a published paper, you should report it to the editor immediately.

In addition to the above three points, researchers should pay attention to the following points:

- All co-authors should contribute sufficiently to the content of the paper and be able to explain the overall picture of the paper correctly. People who do not contribute should not be added as co-authors solely for the sake of personal relationships.
- Correctly cite relevant or prior studies. Credit the contributions of other researchers correctly.
- If there is a possibility of a conflict of interest during peer review, etc., report it honestly.
- You should not act with intent to mislead the parties involved.
- Data related to research should be retained for the period specified by the company.
- If the presence of misconduct or improper conduct is known, it should be reported to the management.

In practice, there are many cases where delicate judgment is required. For example, even if you think that "This is my idea," it may be gradually formed through discussion with others unconsciously. What matters is mutual respect and trust, and when problems arise, deal with them sincerely. I think that's the "integrity of researcher".

The community of researchers was built on mutual trust in the first place. The peer review system is almost always voluntary based. In this sense, the community of researchers is a society based on so-called **Reputation Capital**, that is, a society that puts more value on reputation than money. I think this is one vital model of future society. If so, we, researchers, should be proud of spearheading such a future society.

Technical Ethics

It was not long ago that the profession of researcher took root in the society. It was in the beginning of the 19th century. Until then, scientific research had been carried out as a hobby by some rich people - sometimes by themselves and sometimes funded by patrons such as aristocrats. At present, research and development of science and technology is professionalized, and, depending on the country, 2 ~ 4% of GDP is invested in research and development of science and technology, and a part of it is from the taxpayer's money.

It should be understood that the role of science in society is changing as a background of the professionalization of research. Before the 19th century, science focused on understanding the providence of nature. As a result, science should not have been influenced by people's lives and values; it was to be conducted away from them. Since the beginning of the 20th century, as science and technology have rapidly developed, it has been expected that science directly contributes to our society. Japan's 4th Science and Technology Basic Plan, enacted in 2011, clearly positions science and technology as "Addressing Various Issues Facing Human Society." That is to say, science is no longer independent of the activities of humanity, but has a definite application purpose. To meet this purpose, universities and companies pay researchers for their research and enjoy the results in return.

As corporate researchers, we are developing new technologies that humanity has never seen before. It is also necessary for researchers to consider how such technology affects society. Nuclear power generation is a great engineering success, but at the same time, it can bring great disaster to the society. In the future, the results of genetic engineering and artificial intelligence research will greatly affect the values of human society.

I have been invited to several "artificial intelligence" ethics discussions. "Artificial intelligence" is a strange expression. I understand that it originally means "the branch of research that seeks to understand intelligence by creating machines that mimic it", but at the same time, a system applying the technology obtained from this artificial intelligence research is sometimes called "artificial intelligence". (On the other hand, there is no one who calls the automobile which applies thermodynamics "thermodynamics"). This seems to be causing a great deal of confusion in the ethical debate over artificial intelligence.

I think anthropomorphic "artificial intelligence" appearing in Sci-Fi movies is a wonderful technology if realized, but at present, there is no technology roadmap to realize it. It's just a creation in our imagination. On the other hand, the technology of deep learning, which has attracted attention since around 2012, has greatly advanced artificial intelligence research, and products applying this technology, such as the so-called "AI Speaker", which are beginning to be widely used. It is not a sound argument to confuse the technology of the imagination with the real technology.

Unfortunately, some profit-seeking companies seem to use the word "artificial intelligence" to make their technology look better, knowing that the market misunderstands it. For this reason, it seems that society has over-expectations for the technology and also excessive fear on the other side of the coin. As experts in our field, it is our duty to convey to the world what our technologies can and cannot do. You should not oversell or undersell your technology. I think that is the most important aspect of engineering ethics at the moment.

From the viewpoint of how the society is affected by technology, we have recently focused on the effect of deep learning on programming education. Machine learning can be regarded as a technology to automatically develop a program by imitating input and output examples. This style of programming is called **Inductive Programming,** in contrast to the type of programming that writes down algorithms top-down. If inductive programming is going to be an important part of the programming of the future, then the programming education in primary education, which is being discussed at present, should naturally include inductive programming. For that purpose, PFN prepared teaching materials for inductive programming which can be used in classes of elementary school, and offered them to the Ministry of Education, Culture, Sports, Science and Technology.

Figure 12. Inductive programming materials (left) and a class using them (right)

The importance of programming in primary education has been recognized recently. Social values change over time. We need to keep an open mind and always think about how our technology affects society, using our full imagination.

Business Integrity

As researchers in a company, we are primarily employees of the company, and required to contribute to the organization's business, while following the organization's code of conduct. In many cases, there should be no problem as long as we follow the company's code of conduct, rules, business processes, and customs, but sometimes it is necessary to seriously consider integrity.

It gets tricky when it is hard to tell whether something is right or wrong. Integrity may clash with the logic of profit-seeking companies. Consider the following case used in my class at the University of Tokyo.

Case "Integrity"

It is November and the Adaptive Cruise Control (ACC) project that Mr. Yano is leading is getting closer to its conclusion. Each component is almost done, and the team is looking forward to doing the integration test that is planned for early next year. If the system passes this integration test, the new ACC will be demonstrated in a real vehicle in a test course in April.

A supplier named EnSoft is still developing software for some components but the software will be delivered by the end of November, as planned. The EnSoft engineers are excellent and Mr. Yano sees no problem with the quality and development timeline.

One day Mr. Yano's section manager called. In the designated meeting room he was greeted by his section manager, along with the manager of the accounting section of the division. They told him to delay the acceptance inspection of the software until the following year. He thought this was ridiculous and he immediately objected that delaying the software delivery would make it impossible to do the integration test on time. In return, they said "No, no, you can receive the completed software. Just put off signing the acceptance document".

Mr. Yano was not happy with the answer, but his boss's words were an order. He instructed EnSoft to conduct several additional tests. He had maintained good relationships with the engineers of EnSoft, so although the tests were known to be easy to perform, he told them that the test data would not be available until late December. Thus, the test was done around Christmas and the acceptance inspection was inevitably delayed until the new year.

Later he learned what had happened. Because of the high appreciation of yen against dollars in the past August, Endo Auto's financial performance had declined badly. In an attempt to avoid a financial deficit for the year, the CEO had ordered every division to cut their 4Q expenses by 5%. The annual budget of the 2nd R&D Division accounted for about 40B yen, half of which was allocated to software development subcontractors. Mr. Kato, the head of the division, decided he could achieve the assigned 5B yen cost reduction goal by putting off the payments of some software development contracts until the next year.

"... but isn't it a violation to the SOX law[21]?" Mr. Yano asked Mr. Kaneda, his department manager, when they went out drinking one night. "Don't be childish", Mr. Kaneda said. "We are fortunate because we can be flexible about our subcontract budget. Other divisions have to reduce headcount if they do not have enough discretionary expense. They say that the

[21] The Sarbanes-Oxley Act. This law strictly requires the internal control of enterprises in response to Enron's scandal. In Japan, the Japanese version of SOX law was enforced in 2008.

HR division really did that. Besides, EnSoft’s fiscal year ends in March. They do not care if the payment is done in December or January. ”

== End of Case ==

Is this an integrity issue? Strictly speaking, it is. If it is, what should Yano, as a senior researcher at a large company, do? Should he appeal directly to the president, saying "there is an integrity issue?" There is no absolute right answer to this question. You have no choice but to make your best judgment on the spot.

I myself have had many experiences in which I had to make subtle decisions as a manager. As a result, I was punished for violating the rules. Not once, but a few times. Based on these bitter experiences, I learned two things: 1) to tell bad news as soon as possible, and 2) to make sure my decisions are recorded with the reason for the decision. Sometimes you make bad business decisions. That's not a problem in itself. If it turns out to be a bad decision, correct it at that point (keep in mind that, of course, you need to be accountable for any bad outcomes). I think it is more important that you make a decision that is right in light of your faith about having done the right thing.

Integrity is not a problem solved by rules alone. In English, there is an expression: **due diligence**. It has a special meaning when it is used for the acquisition of a company etc., but, in general, it means "always pay attention to". In Japan, there is an expression “the duty as a faithful person[22],” which has the same meaning. Under the Japanese Civil Code, the obligation is related to "due care of a bona fide manager". I learned this word when I was a board member of the condominium owners association where I live. Daily condominium management is usually handled by a management company. The management company manages the property in accordance with the contract they have with the owners association. However, if an employee of the management company finds something is burning on the emergency staircase, he or she must extinguish it even if it is not a part of the specified duty in the contract. A well-intentioned person would, of course, do so, even though it is not explicitly stated in the contract.

I think the integrity of researchers is the same. You should do what a faithful researcher would do. This is the integrity of a researcher. It is not about blindly following the rules given by the company. I don’t like rules. I think people who rely on rules too much are giving up their responsibility to make their own decisions. Simple decisions can be made without depending on rules. Difficult and subtle decisions are not well-suited to prescribed rules. I would like you to be a researcher who is willing to assume the responsibility of making decisions yourself. And if you see something wrong, I would like you to have the courage to bring it up. I want that culture where I work.

The Lessons of Theranos

[22] Zen-Kan-Chui-Gimu (善管注意義務)

Theranos was founded in 2003 by Elizabeth Holmes, a sophomore in the Chemistry Department at Stanford University. An epoch-making technology had been developed in which all blood tests could be performed using only one drop of blood from a fingertip, instead of the conventional blood test in which blood is drawn from a vein. It was a grand vision that, if realized, a device as small as a refrigerator could enter each home and people's health could be monitored at all times. The Theranos investors included Oracle founder Larry Ellison and the former Secretary of State Henry Kissinger. At one point, Theranos overtook Uber as the world's largest unlisted start-up.

Unfortunately, Holmes's vision never materialized because of many technical difficulties. The core problem was that she had lied a lot to cover up development failure. Research is uncertain. Nobody knows when an unexpected breakthrough will appear. So, even if you think you are heading towards a dead end in the research, you may feel inclined to double-down, so that you may eventually succeed. It may create a behaviour to tell a little lie now, on the thinking that a revolutionary result may just be around the corner. As a result, the accumulation of these "little lies" eventually led to the development of a fraudulent service in which one drop of blood was diluted, analyzed by an existing blood test device, and reported to the customer. In 2018, the US federal prosecutors indicted Elizabeth Holmes and Sunny Balwani, the chief operating officer of the company, on fraud charges.

When she started her business in 2003, she must have never dreamed she would end up committing fraud. I think it started with a pure desire to save sick people. I think the idea of using microfluidic technology to analyze blood in a drop of blood was also excellent. But couldn't they have gracefully admitted that they couldn't have done it with this technology?

Again, we, researchers, have a duty not to oversell or undersell what technology can or cannot do, and to communicate the reality. We need to keep this in mind.

Chapter Summary

- Understand how intellectual property rights work (patents, copyrights, trademarks, and know-how).
- It is important not only to protect intellectual property rights but also not to infringe on the rights of others.
- In research projects involving cooperation with customers and other companies, read the contracts carefully.
- To protect yourself and your organization, you must become a researcher with integrity.

Chapter VII: Where is our R&D Going?

In this book, we have discussed how to conduct research in a company, how to be a good researcher, how to communicate with and lead people while taking "Research that Matters" as the main theme. Our R&D must ultimately contribute to the progress of our society tomorrow and solve the immediate problems of your customers and business today. At the same time, however, we must not lose what it means to do research.

At the end of the 19 century, Albert Michelson, the winner of the 1894 Nobel Prize in Physics, stated[23]:

> *"The more important fundamental laws and facts of physical science have all been discovered, and these are now so firmly established that the possibility of their ever being supplanted in consequence of new discoveries is exceedingly remote … our future discoveries must be looked for in the sixth place of decimals."*

The expression "look for in the sixth place of decimals" means that, for example, to find a new celestial body, it must be measured with such accuracy. In other words, Physics at that time could accurately predict the movement of known celestial bodies with six significant digits.

Not only Albert Michelson, but many physicists at the time seemed to think, "all the basic laws of physics were discovered." But six years after Michelson said this, Max Planck published quantum mechanics, and 11 years later Albert Einstein published the theory of relativity.

I learned about Michelson's quote from Dr. Paul Horn, my immediate boss when I was the lab director of IBM's Tokyo Research Laboratory. He used this quote in an IBM Research brochure, saying that when innovation looks stagnate, it is the biggest chance for a breakthrough. I think it is important for a researcher to always question what everyone takes for granted.

So I would like to close this book with a question that I wrote in my blog titled "An Invitation to High-Dimensional Science".

Blog "An Invitation to High-Dimensional Science"

I am interested in how Information technology affects our society. Here, I would like to consider how deep learning, which has made remarkable progress recently, affects the activities of science. I think a new methodology (that I call "high dimensional science") is emerging.

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[23] https://www.goodreads.com/author/quotes/568740.Albert_Abraham_Michelson

1. Deep learning and science

The background of this idea gradually formed during the five years I spent at the Institute of Statistical Mathematics. The Institute, as an inter-university research institute, studies statistical methods to help various fields of science. As you know, statistical testing and statistical modeling are important tools in modern science. I became interested in why such tools came to occupy such a position in the long history of scientific methodology.

The catalyst came with the book *The Fourth Paradigm* [Ref. 21], which discusses how information technology has changed the methodology of science. It starts with experimental science (the first paradigm) which leads to laws by observing the natural world, theoretical science (the second paradigm) which leads to new laws based on logical deduction on the assumption of mathematical rules, computational science (the third paradigm) which seeks numerical solutions by computers for problems difficult to solve analytically, and data-centered science (the fourth paradigm) which is to look for laws by computers from a large amount of experimental/observational data.

I was fascinated by the role of statistics in inductive science (the first paradigm). When we observe a new phenomenon, we may think we find a new law. But one instance of observation is not persuasive, so we repeat the same observation many times. How many times should we observe it for it to be agreed as a law? The statistical hypothesis testing, established in the early twentieth century, was the first in inductive science to enable quantitative assessment of hypotheses. Unfortunately, in today's computing world, current methodologies based on statistical hypothesis testing are in trouble. In statistical hypothesis testing, a hypothesis should be fixed first and then design of an experiment, asking "if this hypothesis does not hold, what is the probability that we get the experimental results by chance?" The point that you fix the hypothesis before the experiment is important but with current computational power, it is possible to conduct an experiment first and then search for the best hypothesis that fits the experiment results. For example, "the science budget in the United States is correlated to the number of people who commit suicide by hanging in that year" is a hypothesis that is accepted by statistical hypothesis testing. Such "parasitic correlations" are well known [Ref. 22]. For this reason, statistical hypothesis testing should not be used without a deep understanding of the statistical reasoning behind it (In 2016, the American Statistical Association issued a warning about statistical significance and the use of *p*-values [Ref. 23]). In the 4th science era, it is clear that it is time to review the methodology based on statistical hypothesis testing. If a scientific methodology that has been accepted for more than 100 years is about to expire, what else is also expiring that we take for granted in science?

I joined the Institute of Statistical Mathematics in 2011, shortly after the Great East Japan Earthquake hit the Tohoku area. Gentaro Kitagawa, then the president of the Research Organization on Information and Systems (The Institute of Mathematical Sciences is part of it) asked me to look at what science can do in response to such a catastrophe. We formed a multidisciplinary team consisting of researchers from the National Institute of Polar Research, the National Institute of Genetics, the National Institute of Informatics, and the

National Institute for Environmental Studies to start a new interdisciplinary research project called "Systems Resilience" with the aim of shedding scientific light on the concept of resilience [Ref. 24]. One of the most compelling themes was complexity. Complexity researcher John Casti wrote a book titled *X Event* [Ref. 25] and argues that any system with monotonically increasing complexity inevitably leads to a massive collapse. So somehow we must manage complexity. On the other hand, in the classical literature on cybernetics, there is W. Ross Ashby's Law of Requisite Variety [Ref. 26], which says that in order to control a system, the controller system must have more states than the controlled system, meaning that a complex system cannot be controlled by a simpler system. In other words, you can't create a mechanism that reduces complexity. If this is the case, we will need a tool to handle a complex system as it is.

Then I came across deep learning. I had joined PFN as an adviser in 2015, and I had the opportunity to participate in a discussion with Okanohara and other members once a week. At that time, deep learning was beginning to attract attention, and PFN was studying these technologies by reading new papers one after another. What deep learning does is obviously statistical modeling, but it deals with an incommensurate number of parameters, one million or even tens of millions. Common wisdom in statistical modeling says that such a complex model is useless because it overfits the training data set. But deep learning can somehow make a model with good generalization performance.

Statistical modeling, such as linear regression and principal component analysis, have been used to elucidate the mechanisms of action in nature. In other words, they are in the "scientific toolbox." So what does deep learning (or statistical modeling with very large number of parameters) add to this toolbox? What will it bring into science? I think that was the question that prompted me to think about "high dimensional science".

2. Limitation of reductionism

René Descartes' **reductionism** is the most common tool to deal with complexity. A complex system[24] can be studied by breaking it down into smaller pieces and then studying each piece individually. This is a great wisdom of humankind. I believe that the thinking that led to it could have had a fundamental belief in the principle known as **Occam's Razor** which says that a law with the smallest number of assumptions is the correct one. If we decompose a complex system into pieces and apply a simple law to each (for example, the First Principle), then you can explain the complex system by combining them. From an engineering point of view, reductionism means **abstraction** (hiding details) and **modularity**. We can confidently use highly complex engineering systems such as financial systems and jetliners because they can be broken down into modular and abstract components that can be individually verified for correctness. If you put together the correct parts that you know are working correctly, you should be able to predict the behavior of the combined system.

[24] A complex system that can be analyzed with "divide-and-conquer" strategy is sometimes called a *complicated* system and distinguished from an inherently complex system that I will describe later.

Sure, it would be beautiful if a set of simple principles could explain everything as it is postulated in reductionism, but the world we live in doesn't seem to work that way. Many of the things that interest us are complex. A typical example is biology. Shinichi Fukuoka's excellent book *The World Cannot be Understood by Decomposing It* [Ref. 27]' is worth reading because he shows that biological systems cannot be explained solely by reductionism. Human intelligence and social dynamics are also hard to explain by decomposing into a smaller number of orthogonal bases. Oriental beauty is another example - why can the dry garden in a Zen temple in Kyoto heals people's minds? Is it because there is a stone over there and a pine tree here? Can you explain it by decomposing the Zen garden into pieces? I don't think so. It cannot be decomposed, and you should take it as a whole. Even in engineering systems that consist of modular components, often there are "abstraction leaks". The **integral development approach**, where the details of interaction between modules are not determined by a documented interface but by discussions among affected engineers, has been considered to be the secret of success of the Japanese manufacturing industry. The integral development approach admits that any abstraction leaks and uses the leakage in a positive way (In 2010, I wrote a blog titled The Curse of Abstraction).

Currently, PFN is working with the National Cancer Center to diagnose cancer based on ExRNA in human blood. ExRNAs are fragments of RNA (MicroRNA) that normally stay within cells but somehow move out into blood. There are more than 4,000 types of ExRNAs. It is hoped that by measuring the expressions of each ExRNA type in blood, various cancers will be diagnosed. So far, many researchers have been working to identify the predominant exRNA types out of 4,000 types for identifying any given cancer. The National Cancer Center and PFN have found that we can dramatically improve the diagnostic accuracy of cancer if we stop searching for specific dominant ExRNAs but instead look at all 4,000 types simultaneously. This is another example of a breakthrough by not being reductionist but by seeing the whole as it is.

Looking at the whole means, in terms of statistical modeling, not arbitrarily selecting explanatory variables. Looking at the whole does not identify a small number of principal components or bases (or rather, not explicitly). In other words, we can assume that the model that determines the system's mechanism of action is an ultra-high dimensional model with essentially too many parameters (generative models actively utilize the latent space obtained by compressing the space of input variables, which is usually a space consisting of several hundred or maybe a 1000 parameters).

In the literature, there are other approaches to complexity. One is **complexity theory**. In complexity theory, a complex phenomenon can emerge from a relatively simple model. Another approach to complexity is **information theory**, which tries to deal with complexity as one particular aspect of randomness, or entropy, and it can deal with as many parameters as you like, but it largely ignores the system internal structure. I think the third way is possible, by thinking that the model itself is very complex with a large number of parameters but their relationships are very complex and their values are constrained by many other parameters (mathematically, it could be represented as a **manifold** embedded in a super-multidimensional space). The realization is that this way of thinking is needed to model many other interesting problems in biology, sociology, and science, in general, is the essence of what I call higher dimensional science.

As one physicist mentioned, nonlinear, nonequilibrium, excited state, and dynamic physical properties are areas that physicists have traditionally avoided as a "dirty area" (note: after publishing this blog post, I was told that "non-equilibrium physics has been studied actively in recent years and with rapid progress"). Or, in science, it is considered difficult to solve an **ill-posed problem**, that is, a problem in which a solution is not unique or a discontinuous point exists in the solution space. On the other hand, deep learning has a very large number of parameters, so it can approximate arbitrary high dimensional nonlinear functions. With enough training data, it's possible to model a "dirty" or "ill-posed" problem of science.

3. Human cognitive limitations and scientific goals

I think there has been an awareness of the necessity for such high-dimensional models for a long time. Why has the science of the past adhered to the value of "Occam's razor" and always tried to build low-dimensional models? I think one of the reasons is human cognitive limitations.

Organisms have different levels of intelligence. Dogs and cats are pretty smart, and bees and ants have collective intelligence. Even relatively simple organisms such as slime molds are known to behave quite intelligently. Of course, the most intelligent creature on earth are us, humans. But many people believe that there are more diverse creatures in the universe. Some of them may be much more intelligent than humans.

Consider the scale of intelligence, if such a thing really exists. We have no idea what "intelligence" is measured by, but as a very rough approximation, as an intelligence indicator, let's consider the computational speed, that is, how much information can be processed per unit of time. Since the computational speed of computers increases exponentially thanks to Moore's Law, it would be natural to express intelligence on a logarithmic scale if this trend were to continue. The question is where the human intellect lies on this scale. As the historian Yuval Noah Harari points out in his book *Homodeus* [Ref. 28], if the human brain's abilities have changed little over the last few tens of thousands of years, then the abilities of a single person out of 7 billion people are distributed in a very narrow range on this scale. Whether it's Einstein or a city scientist, their intelligence are almost the same on this scale. It is not surprising that our 2000 year history of science has been strongly influenced by this fact - the limits of human intelligence.

Zvi Artstein's book *Mathematics and the Real World* [Ref. 29] tells us how Mathematics, as we know, has evolved, being guided by human intuition. Euclidean geometry is constructed from 20 axioms, including "Two parallel lines do not intersect" and "There is a straight line passing through two arbitrary points." But why these 20 axioms? Non-Euclidean geometry has a different set of axioms. This leads to the question of how many other axiomatic systems are possible in the world. There are various axiomatic systems that we use in mathematics such as set theory and probability theory. Do they cover all the useful axiomatic systems? Aren't they the result of arbitrarily choosing something that happens to fit our human intuition? Aren't there more axiomatic systems that should be important to human society but have not been found because the limitation of our cognitive ability is blocking our sight? If that is the case, even in mathematics, which is considered to be highly abstract concepts, are limited by human cognitive capacity. What kind of axiomatic systems would be possible if some being who is several orders of magnitude smarter than human beings studied physics, biology, mathematics, and information science? These beings may be able to intuitively understand properties of a manifold in a one-million dimensional space, just as we can easily imagine the shape of a baseball or a doughnut.

But then, now we have computers that can boost our intellectual abilities. A deep neural network with tens or hundreds of millions of parameters can model a very complex system. If the model describes the mechanism of action for an object, it is hopeless for a mere human being to understand how the model works, but the model can still make accurate predictions or control the target system with the desired results. This is where the question "What is the ultimate purpose of science?" comes up.

The purpose of traditional science (I'm going to call it "low-dimensional science") was to understand the mechanism of the actions of nature. If Newton's law of gravitation can explain the motion of an object, it can be used to predict the motion of a celestial body or to calculate a canon ball's trajectory so that it will land where you aim it. If we understand the mechanism of action, we can predict and control. Still, what if the understanding is not the necessary condition of being able to predict and control? High dimensional models have made it possible to predict and control with high accuracy without really understanding the underlying mechanism.

What does "understand" mean here? It should be noted that it is an understanding for humans, and this concept is relative to human intelligence. A deep neural network with 100 million parameters might be able to predict the behaviour of a certain system, but understanding its mechanism would be well beyond our intelligence. If "science" is something universal, should it be bound by the current level of human intelligence? We can go beyond it with the help of deep neural networks. Thus, "high dimensional science" can unlocks these constraints.

Professor Marten Scheffer, a researcher on complex systems at the University of Wageningen in the Netherlands, warned, "Most of the success comes from branches of science that study only tiny subsets of the complex world[25]." For researchers, defining a problem on tiny subsets so that you can write a paper is extremely important, so this is probably the right strategy for individual researchers[26]. However, "problems to solve for human society" may not necessarily correspond to "problems understandable to human

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[25] https://www.wur.nl/en/show/Speech-2010-by-Prof.-Marten-Scheffer.htm

[26] This attitude is exactly what I suggested in Section 2.1 in the context of "Choosing a good problem." I know this sounds contradictory, but I think it is necessary to reconsider to tackle large problems for human society at some later stage in your research career.

beings." If that is the case, now that we have a new scientific tool called high dimensional science, isn't it time to review the ultimate purpose of science?

== End of Blog ==

It can be thought that many elements of our society, such as the methodology of science, the mechanism of democracy, the mechanism of economy, and the organizational structure of a company, have evolved into the present form over a long period of time. Why did it evolve that way? Why is the methodology of science in its current form? Why did democracy become the current system? Why is the organizational structure of my company like this? Considering the environmental conditions which contributed to each evolution, and the answer of "Why?" can be seen.

We, researchers, want to be free from existing concepts and always keep asking "Why?". Curiosity, or questioning any established concepts, is the driving force of breakthroughs. So keep questioning. That is the most important thing for a researcher.

Afterword

The financial crisis triggered by the failure of Lehman Brothers in 2008 had a major impact on the world and IBM Research was no exception. In order to cope with the rapidly changing economy, IBM Research was forced to drastically cut costs. As the director of Tokyo Research Lab, I had to ask many researchers to leave as part of the restructure. When the restructuring was completed at the end of the year, I seriously considered my future. It was in January of the following year that I decided it was my turn to leave. For the 26 years since I joined the company, I was with Tokyo Research Lab with the only exceptions being when I spent one year in the U.S. and again when I was on loan to the IBM Business Consulting Service. In my time in IBM, I enjoyed technical discussions with many researchers in the lab and I learned a lot. In a way, the Tokyo Research Lab had been all about my work life. When I left the lab in April, I think it was psychologically very hard on me. There were times when I couldn't stop crying all of a sudden on the train coming home. I could not sleep at night, so I consulted a doctor who sent me to the department of psychosomatic medicine and there I was prescribed antidepressants.

It was a painful experience for me, but in retrospect, ten years later, I think it was the best decision. Without my experiences in Canon, the Institute of Statistical Mathematics, PFN, and teaching at the University of Tokyo, it would have been impossible for me to gain deep insights into the conduct of research from many different perspectives. The relearning of probability and statistics from scratch in the Institute of Statistical Mathematics led to the launch of a new field of machine learning engineering.

In 1985, Steve Jobs was ousted from his company, Apple. In his famous speech at Stanford University in 2005, he said "getting fired from Apple was the best thing that could have ever happened to me." The next few years were a period of great creativity for Steve Jobs, including the creation of Next who developed NextStep (the foundation of the current iOS), and Pixar, which is famous for its animated films.

Of course, my experience is nothing compared to that of Steve Jobs, but I still feel that there is something in common in the sense of "a setback as a springboard for great growth".

As I wrote in the foreword of my previous work, I quoted an old Chinese proverb, roughly translated as “inscrutable are the ways of heaven.” Many things happen in your life. Even though I had a hard time, I didn't become pessimistic about my life, but rather cherished the time with my family. I think you can live your life without worrying too much about the past and the future. Then, opportunities will present themselves. That's my last piece of advice to all of you.

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When I quit IBM, I said to my colleagues there that it does not matter which organization you work for, but the important thing is the connection you build with people. I still have connections with many of these people both in the research and personal life, such as drinking, cycling, skiing, and hiking.

Like the previous book, this is a book based on my own ideas. Mr. Koyama of Kindai Kagaku Co., Ltd. willingly responded to my request to publish it. This book is machine-translated into English with minimum post-editing. Without the generous help of Mirai Translate, Mihai Morariu, and Stuart MacDonald, this English version would not have materialized.

Finally, I would like to thank my family for supporting me with their full happy smiles. Thank you.