9 Rethinking Innovation[1]

Yamaguchi Eiichi

Japan's technology companies suffered collective convulsions on 30 January, 2004, when the Tokyo District Court ordered Nichia Corporation to pay 20 billion yen to former employee Shuji Nakamura as compensation for his patent (Nakamura, 1991) relating to the blue light-emitting diode (LED). The figure represented the full amount claimed by Nakamura, and had he claimed more, it is likely he would have got it. The court estimated his contribution at 60.4 billion yen, or half the benefits the company was expected to earn before its key patents expired. It argued that Nakamura made the invention 'with his individual power, based on utterly original thinking' despite the fact that he was 'working in a poor research environment at a small company.'

The court was mistaken on several grounds – the extent to which the invention of the blue LED resulted from Nakamura's heroic, individual efforts, the poverty of his research environment, the significance of support from the top management and subsequent investment decisions, and even the significance of Nakamura's patent in the production process (cf. Yamaguchi, 2004 for details).[2] Irrespective of the intellectual property rights dimension, the Nichia/blue LED case also encapsulates some fundamental insights about innovation (in a theoretical and practical sense), and the structure of innovation. This chapter explores these theoretical lessons, how these relate to the decline of innovation in Japan, and offers some preliminary suggestions about what might be done to reverse the decline.

It starts by re-examining Christensen's concept of disruptive technology, categorizing this as 'performance disruptive technology,' and introducing a further basic

[1] I am most grateful to Mr. Keigo Shiowaki (The Dai-ichi Mutual Life Insurance Co.) who collected the data on total stock issues. I would also like to thank Dr. Greg Lindon (UC Berkeley) for background information on why Robert Noyce left Fairchild. The research reported here is work in progress, and forms part of ITEC's activity as a Center of Excellence selected by the Japanese Ministry of Education, Culture, Sports, Science and Technology (MEXT).

[2] Cf. Yamaguchi, 2004, for details. On January 11, 2005, the suit was settled amicably at the Tokyo High Court, where the judge recognized problems in the earlier Tokyo District Court ruling. The final amount of compensation was ¥600 million yen not only for the patent of the lawsuit, but all the contributions by Nakamura. The patent portion can be estimated at ¥10 million yen according to the formulation suggested by the High Court, which is 1/6000 the amount ordered by the Tokyo District Court.

type – ‘paradigm[3] disruptive technology’ – to create a two-by-two quadrant. To illustrate paradigm disruptive technology, we return to the innovation process leading to the blue LED, and the breakthroughs which were necessary. Some of these were initiated by researchers in large corporations who had their research terminated, in part because it flew in the face of accepted paradigms. Instead, it was in the tiny company from the island of Shikoku that the final breakthrough was made.

This, in turn, illustrates the difficulties of pursuing paradigm disruptive innovation in large firms, which were exacerbated in the 1990s when large companies restructured their R&D operations and cut back on their core researchers. Since 80% of Japan’s R&D expenditure takes place in private companies, most of it in large companies, the damaging effects on Japan’s innovation system can easily be imagined. The chapter concludes with some suggestions for rebuilding the innovation system, not by attempting to turn the clock back to the ‘golden age’ of linear innovation, but cognizant at least of conditions conducive to supporting paradigm disruptive innovation.

Innovation dilemmas

The old model of ‘linear’ innovation, with great central research labs located away from the hustle and bustle of the ‘real’ world in splendid isolation, generating new technologies which then make their way through development and production and finally into markets, has become a victim of the times. As a result of IT development and the consequent unprecedented speed of feedback linking markets, technology and science, extensive efforts have been made to develop new models of innovation which go beyond feedback loops to fundamentally change the process. In the post-linear world, it becomes all the more critical that researchers search for ways to gain exposure to customers needs, real or latent.

But listening to the customer is not enough, either, as has been forcefully demonstrated by Clayton Christensen in *The Innovator's Dilemma: When new technologies cause great firms to fail* (1997). In fact, Christensen shows, listening

[3] ‘Paradigm’ is used in the original meaning given by Thomas Kuhn (1962).

carefully to major customers makes the business vulnerable to what he terms ‘disruptive’ technology. Thus market leaders, who would normally be expected to be best placed to capitalize on new technologies because of their close links with customers, often end up becoming victims of them.

One of his examples is the hard disk drive, in which successive generations of market leaders were replaced as the size of the disk was reduced. The reason the leaders failed to make the leap to the next generation was not because they were ignoring their customers (or because they were bureaucracy-bound for that matter), but because, by listening to them, they were unwilling to make timely investments in technologies which ‘result in worse product performance, at least in the near-term’. While they were unbeatable in terms of incremental or ‘sustaining’ technological innovation, they were vulnerable to disruptive technology. They were captive to what Christensen calls their ‘value network’.[4]

Thus, Seagate Technology, which in 1980 manufactured five inch hard drives, incorporated the value network of desktop personal computer users. It conducted customers surveys, and accordingly, placed emphasis on high storage capacity. The survey was unable to reveal the priorities of potential future customers, who would value portability over storage. Seagate Technology did not enter the 3.5 inch hard drive market until 1988, by which time it had missed the potential of new markets for the smaller drive.

Christensen’s thesis is well known, and need not be repeated at length here. Unfortunately, while Christensen was very careful to specify what does and does not constitute disruptive technology, some who have followed have been much less careful. Disruptive technology has become a kind of box into which all manner of technology and innovation-related threats and ailments have been bundled, a shorthand for ‘watch out for the unexpected’, non-incremental technology which may emerge from where you least expect it.

I would like to re-open that box here, and to differentiate between two fundamentally different types of disruptive technology. The first is that described by Christensen – a technology whose performance is, initially at least, inferior to existing mainstream technology but develops because there are other features which customers,

[4] A value network is ‘the context within which a firm identifies and responds to customers’ needs, solves problems, procures input, reacts to competitors, and strives for profit’ Christensen, 1997: 32).

usually in different markets, value. Let us modify Christensen's expression and call it *performance-disruptive technology*. The second is a new technology whose performance is not inferior but superior – often strikingly so – than current technology. As such it does not present the dilemmas of performance-disruptive technology. It presents companies with other dilemmas, however, because it is based on different scientific principles than existing technologies. These principles themselves are often little-understood, and incremental efforts do not lead to them. Let us call this *paradigm-disruptive technology*.

The basic difference between the two is clear. In the former, the performance of the technology itself is initially inferior because the science on which the technologies are based itself is within the current paradigm. An example is micro processor unit (MPU), which displaced the central processing unit (CPU) of main frame computers. Managers eschew such technologies for business-related reasons. In the latter, the science on which the technology is based offers the prospect of higher – often strikingly higher – performance, but as it is not within the current paradigm. The resulting uncertainties, however, cause managers to reject it.

In practice, the distinction between the two is not always easy to make. Paradigm-disruptive technology may not be immediately distinguishable from a performance-disruptive technology because other factors may inhibit this potential from being realized immediately. Consider the transistor, which is often cited as an example of disruptive technology (by Christensen as well as others). Strictly speaking, it is a paradigm disruptive technology, not a performance disruptive one. Its performance was initially inferior to that of vacuum tubes, not because of the underlying science, but because of technical immaturity. When the transistor was in fact designed as predicted in quantum physics, its frequency response performance was overwhelmingly higher than that of the vacuum tube, which operated in accordance with classical physics.[5] The initial glitches with the transistor were related to moving to a new paradigm, and not the bold and intentional lowering of performance levels.

The reason for making this distinction is not pedantic. The reasons companies fail

[5] According to William Shockley: 'One was demonstrated on 20 April 1950 according to my marginal note. This nonphotogenic device did perform according to theory but had a wide base and poor frequency result and provoked little interest' (Shockley, 1984: 1542). The problem was that because of the thick p-layer, it was not reflecting the quantum physics.

to meet the challenges of paradigm disruptive technologies are not the same as the reasons they fail in performance disruptive technologies. The former fail not because the technology in question is inferior, and customers in existing value networks do not like it, but because it is difficult for companies to design and sustain R&D activities around paradigm-destructive scientific principles which are often poorly understood, which conflict with conventional textbook wisdom, and cannot be pursued with certainty. In other words, they present a different set of challenges for companies, and the intensity of these challenges has increased in recent years, partly as a result of intensified bottom-line pressures on managements. We shall explore these challenges shortly.

Finally, it is possible to depict both types of technology as axes – performance-disruptive technology at one pole of the x-axis, with performance-sustaining technology as its polar opposite, and paradigm-disruptive technology at one pole of the y-axis, and paradigm-sustaining technology as its polar opposite (figure 9-1). Clearly, it is easiest for large, successful, establish companies to pursue innovation in the bottom left quadrant, i.e., innovation which is both performance and paradigm sustaining. According to this representation, the challenges in the bottom right are distinct from those in the top left. By definition, the top right quadrant is empty.

< Insert Figure 9-1 here >

The blue light emitting diode

Having introduced the concept of paradigm-disruptive innovation, and with these preliminary comments in mind, let us return now to the blue light emitting diode (LED), the technology for which was developed in Japan in the late 1980s and early 1990s, and finally created by a group of young researchers at Nichia Corporation, led by Shuji Nakamura (currently a professor at UC Santa Barbara), Masahiro Senoh, Naruto Iwasa and Takashi Mukai. It is important to note that this team did not discover the scientific basis which led to the innovation; this was done by others. Moreover, it was eventually made possible because of a close working relationship with the top management of Nichia Corporation, a relationship which other would-be developers lacked.

In the 1980s red LEDs had already been developed, but not green and blue LEDs. If green and blue LEDs could be developed, the primary colours could be combined in different ways to create any colour desired. This would open the way for lights to be made that would drastically reduce energy consumption and have a semi-permanent life expectancy. With the potential for enormous paybacks, major corporations invested heavily in green and blue LED research programmes.

It was known theoretically that green or blue light emission should be possible through the use of gallium nitride (GaN) crystals or zinc selenide (ZnSe) crystals. In practice, this proved difficult. Crystals are grown using binding blocks that resemble Lego pieces. Non-Lego blocks cannot be placed on other Lego pieces because the junctions do not match. Similarly, when constructing crystals, the junction configuration of the added layer must be the same as that of the substrate crystal (the lattice matching condition). In the case of the GaN crystal, this substrate did not exist.

On the other hand, for ZnSe, there was already a known sustance, gallium arsenide (GaAs), which could be used as the substrate. For this reason ZnSe had come to play an important part in crystal growth technology by the end of the 1980s, and ZnSe had become the material of choice for blue LED research in universities and private laboratories worldwide.

A small number of researchers, however, defied conventional scientific wisdom, and insisted on using GaN. Isamu Akasaki was one of these. For him, the true meaning of research was to see what has not yet been seen (discovery) and to create what has not yet been created (invention). His company, however, was not sympathetic, and ordered him to abandon his research. Akasaki left Matsushita Electric and went to Nagoya University to pursue his agenda. In 1985, one of his students, Hiroshi Amano, discovered that placing poorly-crystallized aluminum nitride (AlN) on sapphire and then applying GaN improved the crystallization process (Amano, et al. 1986). The layer that had not fully crystallized acted as a buffer. The discovery of the buffer layer method by Amano and Akasaki was ultimately to lead to the development of blue LED.

Another breakthrough was also achieved by Amano and Akasaki, as a result of an accident. Despite the efforts of many researchers, p-type GaN could not be made. Some theoretical physicists argued that nitrogen defects prevented the creation of p-type GaN. If it was impossible to create a p-type diode, then obviously an LED composed of a pn junction would be impossible. In his efforts to create p-type GaN, Amano conducted many experiments involving doping the acceptors, but all ended in failure. In 1987, he was using an electron microscope to observe acceptor doping when he noticed something unusual. When the electron beam used for observation was projected, the GaN started to glow. The electron beam had activated the acceptor. Capitalizing on their luck, Amano and Akasaki continued to experiment and eventually succeeded in creating p-type GaN in 1988 (Amano, et al. 1989).

Encouraged by these two developments, Takashi Matsuoka and his team at NTT planned to make alloyed crystals combining GaN and indium nitride (InN). To change the colors to ultra-violet, violet, blue and green, the proportions of InN had to be altered. Current scientific knowledge held that it was impossible to mix GaN and InN. In 1989, however, Matsuoka and his colleagues defied conventional wisdom and succeeded in doing just that (Matsuoka, et al. 1990). A third critical obstacle had been removed.

Shuji Nakamura at Nichia Corporation, a small company in Tokushima Prefecture then with around 200 employees, also selected GaN, but for a different reason from Akasaki. After entering Nichia, Nakamura was assigned to the semiconductor manufacturing division and spent time visiting customers trying to sell the semiconductor he had made. It was a painful learning experience to see customers opting for products from larger, known companies in preference to little-known companies like his. He realized that using ZnSe, the choice of the larger companies, would not gain him customers, and that he had no choice but to try paths large companies spurned.

He appealed directly to the company president, Nobuo Ogawa, arguing that unless the company created something new, it would be difficult to survive. Ogawa, who had faced death as a pharmacist on Guadalcanal during World War II, returned to his hometown and created the company from scratch, was persuaded. He allocated 500 million yen in research funds, and gave Nakamura a year off to study crystallography at the University of Florida for one year. This was no trivial commitment in a company of that size, and created a substantially larger research budget than many researchers in large companies could hope for. During 1990, Nakamura developed a 2-flow method (Nakamura *et al.*, 1991) that involved the introduction of a source gas (ammonia and tri-methyl gallium) into the reactor from the side while blowing a large amount of nitrogen gas and hydrogen gas onto the sapphire base. The conditional setting parameters for this method are quite wide, so it was difficult to determine the optimal setting. However, during his first trial he was able to grow higher quality GaN crystals than had ever been seen before.

When trying to build a p-type GaN, Nakamura initially tried the method discovered by Amano of irradiating a low-energy electronic beam onto the surface. However, this did not produce the desired results. He was unsure how to proceed when his assistant Naruto Iwasa simply annealed the magnesium-doped GaN in a nitrogen atmosphere in an effort to break it down. To their surprise, the GaN was easily turned into a p-type (Nakamura *et al*., 1992) It was much easier than anyone imagined.

Nakamura, meanwhile, continued his meticulous study of indium infusion, encouraged by Matsuoka, who openly shared his knowledge with him. As a result, surprisingly quickly, the blue LED became a reality (Nakamura, et al. 1993). There is little doubt that the direct support of the president and the lack of bureaucratic organization at Nichia helped significantly, as well as the research carried out earlier by researchers like Akasaki, Amano and especially Matsuoka.

Matsuoka, on the other hand, had his research terminated by NTT in 1992. The management at his research centre had decided that ZnSe was the answer, and that this was where both energy and funds should be concentrated. Senior management would not overturn that decision lightly. It took until 1996 for major corporations like Toshiba, NEC, Matsushita, NTT and Sony to finally recognize that their decision to select ZnSe was wrong, by which time they had effectively missed the boat. Again, we see parallels to the Cole chapter analysis of NTT's tendency to cling to wrong technology bets.

< Insert Figure 9-2 here >

The three steps described leading to the development of blue LED were not the result of incremental knowledge accumulation along established scientific paths. They involved a discontinuous jump. At the same time, they did not lower the performance of existing technology. Thus, they were not performance disruptive innovations, but paradigm disruptive ones. The process can be depicted diagrammatically. In figure 9-2, the horizontal access represents knowledge creation, or discovery (with discontinuity). The vertical axis represents knowledge realization, or accumulation.

Initially, ZnSe was the choice of major companies since it maintained the lattice match premise and could be used for crystal growth. GaN, on the other hand, did not support lattice match theory, so to use it for crystal growth ran counter to an accepted paradigm in crystallography. Why, then, did some researchers persist with GaN? It can perhaps be best described as a hunch, intuition, or tacit knowledge that a hard and strong material like GaN should be used instead of the soft and easily damaged ZnSe. This tacit knowledge was rooted in solid state physics. Moving vertically downwards in figure 9-2 represents a willingness to move against the current, into this domain of tacit knowledge, to search for a new way. The wisdom of moving in this direction is very hard to sell to top managers in large companies. It runs against their demands for theory or empirical-based reasons, for milestones and probabilities which may be used to justify support of research projects, especially in group-based decision making.

The critical question is whether the process of paradigm disruptive innovation is purely a matter of chance, or whether it can be managed, and if the latter, what kind of conditions are conducive to achieving it. Let us consider one more example of a paradigm disruptive innovation, early in the history of semiconductors, before we address these issues.

The MOSFET (Metal-Oxide-Semiconductor Field Effect Transistor) is a transistor used in LSI (large scale integrated circuits) for computers and all other digital equipment. It is made from silicon and it operates by switching the flow of electrons on the interface between an oxide insulator film and the silicon. It was invented in 1960, but its characteristics were still unstable, and the reason for this remained elusive. By 1964, however, a research team at Fairchild Semiconductor led by Robert Noyce and Gordon Moore was able to identify the cause of the instability, thus potentially opening the way to the integrated circuit of the MOSFET (MOS-IC).

The management of Fairchild Semiconductor's parent company, however, was opposed to the commercialization of the MOS-IC. The existing bipolar IC was commercially successful in an increasingly competitive market, and they were not ready to allocate resources to develop an unproven product like the MOS-IC. They could not be convinced of its potential. Noyce and Moore eventually left Fairchild Semiconductor, which they had founded, to form a company they named Intel. With the MOS-IC at the core of their business, they had remarkable success, while Fairchild Semiconductor lost an enormous business opportunity.

According to accepted quantum mechanics theory, interface states which trap carrier electrons must exist when joining dissimilar materials which are not lattice-matched. In the case of a MOSFET device, interface states *must* normally exist, according to quantum mechanics. However where an oxide film is grown on silicon, the concentration of interface states is extremely low. This discovery was serendipity, although, it seems, the researchers were spurred by a hunch, or tacit knowledge. As the management of the parent company did not share this tacit knowledge, they could not support the project, leaving Noyce and Moore no choice but to create a new vehicle for commercializing the paradigm disruptive innovation.[6]

Paradigm disruptive innovation and large firms

Such stories are familiar, and they offer clues as why paradigm disruptive innovation is difficult to carry out in large firms. The tacit knowledge described in these stories is essential for a research team which wishes to undertake paradigm disruptive innovation. It requires what can be called a ‘field of resonance’ (*kyomei ba*) to create the conditions necessary for breakthroughs. It is extremely difficult to convey the tacit knowledge to senior management, but unless senior managers share and support this field of resonance, it cannot flourish.

Here, the field of resonance is defined by the field (*ba*) (Shimizu, 1995) in which the tacit knowledge itself can be transferred. Nonaka and Konno (1998) discuss that there are four types of field (*ba*), which correspond to the four stages of the so-called ‘SECI model.’[7] The originating *ba*, corresponding to the stage of socialization, is a world where individuals share feelings, emotions, experiences, and mental modes. The field of

[6] According to Ross Knox Bassett: ‘When Fairchild bypassed Noyce for the chief executive position, he quit. Gordon Moore, the head of Fairchild R&D, left with Noyce, out of a growing frustration over the difficulties in transferring products from R&D to manufacturing and a belief that any new head of Fairchild would likely undertake a major reorganization’ (Bassett, 2002: 172). Resolving the instability of MOSFETs by 1965, Noyce and Moore were scientifically convinced that MOS-ICs would take over bipolar ICs. However, the manufacturing department and even the management did not want to challenge such new products. Furthermore, it was unlikely, in their view, that a new head would be competent in anticipating the long-term future.

[7] ‘SECI’ stands for socialization, externalization, combination, internalization: Nonaka and Takeuchi, 1995, chapter 3.

resonance (*kyomei ba*) is similar to this type, but is more specific, relating to principles conducive to paradigm disruptive innovation. On the other hand, the Christensen's performance disruptive innovation has nothing to do with the field of resonance.

The individual who generates the field of resonance is always a researcher who confronts a dead end with the current technology. Instead of overwhelming it by an incremental improvement, he goes down to the principled science the current technology is based on. It must be noted that science, at any time, is not firmly established but fragmented, incomplete and permeated by tacit knowledge. Therefore, there are always scientists who investigate the principles in order to clarify them and make the knowledge explicit. When others joint his endeavour, sometimes for different reasons, a field of resonance is then born. Each individual recognizes the differences for any other individual's goals, works and wills.

Finally, this resonance enables the generator to discover the means of paradigm disruption. Participation of top management is ultimately essential. However, forcing externalization (in the SECI model), which often happens in the project approval and monitoring process, will disturb the generation of paradigm disruptive innovation.[8] If the top management had followed the SECI model without discretion, neither Intel nor Nichia would exist as the current world's top companies in Si electronic devices and GaN photonic devices, respectively.

Difficulties in supporting non-incremental innovation in large, successful firms have been noted by a number of researchers. According to life-cycle models, they become bureaucratically layered. The more layers, the greater the chances of research proposals incorporating tacit knowledge being killed off, and the less chance there is for top management to share in the 'field of resonance.' To use the framework presented in the Chesbrough chapter, there will be a significant number of false negatives. At the same time, senior managers tend to lose their entrepreneurial drive and become 'stewards' and '(t)he compulsion to innovate diminishes and the willingness to violate norms and bear disapproval falls,' according to Porter (1990: 556).

Anderson and Tushman (1991/1997) note the difficulties in simultaneously nurturing incremental innovation for 'today's' businesses and non-incremental

---

[8] As a matter of fact, both the reason why the density of interface states for Si-SiO_2 systems are so low and the reason why ZnSe produces such deep levels in the bandgap are still unknown in modern physics.

innovation in preparation for 'tomorrow's' business. These occur at different phases of the technology cycle and require different competences. Organizations must become 'ambidextrous' to support both kinds of innovation at the same time.[9] It becomes harder, and yet more critical, with the compression of product cycles and the rise of non-liner models of innovation.

In fact, it may be even more difficult since the conditions which support paradigm disruptive innovation may not be the same as those which support performance disruptive innovation. Both require the support of top management, but the former is particularly difficult as hunches and tacit knowledge are unconvincing for hard-pressed top managers.

In Japan, the situation is particularly critical. R&D expenditure in Japan is overwhelmingly concentrated in private firms, particularly large firms. Such firms were considered very innovative up until the 1980s. Scholars and policy makers abroad cast an envious eye on the number of Japanese corporations led by engineers when they complained about the 'short termism' of top managers in the UK and US during the 1980s. Crudely put, however, there are two types of engineer in Japan's major corporations. There are those who engage in R&D but seldom enter the top management ranks. After they hit the top specialist levels, they tend to move into universities (cf. Fujimoto, 2005: 183). The others are those who work their way up through key operating divisions, to top executive posts. In these operating divisions, there is greater reliance on explicit, paradigm-sustaining knowledge than in R&D labs[10] Such engineers can be as unsympathetic to tacit knowledge and sites of resonance as would be accountants. In fact, humanities and social science graduates can be more sympathetic, as they at least know they don't know the science behind the R&D efforts, and hence may be more willing to give researchers the benefit of the doubt for longer period.

This distinction became apparent in the 1990s. It may be that large Japanese companies were still too chaotic until the 1980s to force researchers to try to make explicit prematurely what *should have remained tacit*. Or conditions were more benign, and hence greater slack was allowed in R&D labs. The slack was closed in the 1990s, however, with devastating consequences. Ironically, conditions for paradigm disruptive

---

[9] This can be called 'squeezed states' from the analogy of quantum optics. Here, the original concept of squeezed states is minimum uncertainty states situated in between two opposite states which never coexist due to the principle of uncertainty.

[10] Empirical evidence for this argument is being gathered.

innovation were more benign in smaller companies led by founders, as in Nichia Corporation, or in universities, both of which had fewer resources to devote to it.

< Insert Figure 9-3 here >
< Insert Figure 9-4 here >
< Insert Figure 9-5 here >

As large companies restructured their R&D labs, researchers were transferred to other divisions, such as manufacturing or marketing, or were in some cases sent to subsidiaries on 'loan' or transfer. Others were lost in a wave of voluntary redundancies at the turn of the century. The number of published papers published by researchers in major organizations are broadly proportional to the number of researchers with Ph.Ds in 1999 (figure 9-3). As shown in figure 9-4, the number of academic papers published by researchers in large corporations fell continuously after 1994, suggesting a deterioration of conditions favouring basic research, or knowledge creation. Thus, it can be reasonably estimated that the number of researchers with Ph.Ds also dropped continuously from 1994. Figure 9-5 shows data on the ratio of academic papers published in 2003 relative to 1994 for specific Japanese and non-Japanese companies. Japan's electronics giants uniformly appear in the shaded area, where the ratio is less than 1. The figure also gives the 2003 to 1994 ratio of total market value of the shares issued by those corporations. There is a strong correlation between the two ratios (correlation coefficient of 0.711). If we accept changes in numbers of academic papers published as a proxy for changes in knowledge creation, a number of explanations might be advanced for the correlation:

1. As a result of strong R&D, successful products were created leading to a rise in the company's value. Weak R&D led to the opposite.
2. There was an increase in motivation in research departments at the companies that hired more researchers and technicians and encouraged the writing of research papers. This motivation spread to other departments and increased productivity and product development. Conversely, there was a decline in motivation in the research departments at companies that significantly reduced their researchers and technicians, and declining motivation, which then spread, resulting in a fall in corporate value as

productivity and product development vitality suffered.

3. At companies that increased their value due to higher profits, greater resources were allocated to R&D, leading to an increase in the number of researchers and academic papers published. In contrast, in companies that lost value due to an inability to generate profits, cost considerations led to cutbacks in research departments and thus a reduction in the number of academic papers published.

Of these, the most likely explanation is a combination of 2 and 3. Widespread reductions in researchers took place across the board, depriving these companies of a critical source of creativity. Motivation in research labs dropped. Either as a result, or in tandem, motivation also fell in manufacturing divisions, resulting in a decline in the overall vitality of the whole industry group.

Of course, this did not happen in every large Japanese company, as figure 9-5 shows, but it was widespread. Recently, these same companies have attempted to restore their R&D capabilities, but they will undoubtedly find that it takes much more time and energy to rebuild R&D dynamism than it took to destroy it.

Towards a new innovation system

Let us summarize the characteristics of paradigm disruptive innovation. First, this type of innovation cannot be found on a line extrapolated from known technology A. Second, it is not until researchers ‘burrow down’ to basic scientific principles S that a new paradigm P is discovered. Third, once a new paradigm is discovered, expertise for creating new technology A* becomes explicit knowledge.

The process A--S--P--A* is not linear. In addition to the researchers who undertake this process, moreover, it requires a key person on the business side who ‘co-owns’ the process to succeed. In the case of Nichia, this was Nobuo Ogawa, the president. Noyce and Moore, on the other hand, could not transmit their tacit knowledge to the senior management of Fairchild Semiconductor, and hence were unable to secure the support of such a key person, leaving them no choice but to spin out.

As described here, tacit knowledge is transmitted through a 'field of resonance.' The critical question is how to manage this. It requires firms to develop new kinds of competences. In general, large firms excel at sustaining innovations, but they also work as potential incubators for paradigm disruptive technologies. This requires a willingness to accept diversity and experimentation by researchers with a desire to create what has not yet been created, able to descend to scientific principles. It also requires mechanisms to value and transfer concepts not easily quantified by normal indicators.

Let us reflect briefly on why NTT ordered Matsuoka and his team to abandon their research, and further, why major corporations like Toshiba, NEC, Matsushita and Sony also lagged behind Nichia in the race to create the blue LED. It appears that the managers in these companies did not create a field of resonance with their researchers through which they could co-own the tacit knowledge their researchers had. Nakamura and his team, on the other hand, were able to succeed due to a close working relationship with the management of the company. Although Nakamura and his team did not discover the scientific basis that gave rise to this innovation, Nichia worked as the field of resonance among researchers and management teams, and rapidly commercialized the paradigm disruptive technologies discovered by Isamu Akasaki and Hiroshi Amano of Nagoya University and Takashi Matsuoka of NTT.

This offers another lesson. Managers in large companies would do well to recognize that in the emerging system of innovation in Japan universities and small businesses also play an important role. Many large companies have been actively engaged in creating spinouts or even new ventures, but the former are usually the result of cost-cutting measures, and the latter, by retaining links to the 'parent' company generate potential conflicts of interest, since disruptive innovations have the potential for undermining established markets through which large companies obtain their profits.

Fields of resonance can be created across corporate boundaries. In fact, in the emerging system of innovation in Japan there are opportunities for small businesses not only to commercialize paradigm disruptive (and performance disruptive) innovations, but to co-ordinate the various types of expertise and resources which can enable these innovations to happen. Large company managers may miss valuable opportunities if they continue to cling to corporate-centric views of innovation, without looking for potential fields of resonance.

In a more flexible industrial order, large firms are not necessarily the originators of paradigm disruptive innovation, nor even the co-ordinators of it. They may be providers of capital, however, or customers of technology, or undertake contract R&D. The forces which prevent managers from adopting a more flexible mindset to create diverse win-win situations must be addressed. The biggest of these is social-political forces for centralization, which leave little autonomy for regions, or for individuals. Conversely, the quest to recognize and establish new fields of resonance could unleash long-frustrated creative forces, ushering in a new era of innovation.

The seeds of paradigm disruptive innovation have always been generated at universities as well as corporate research institutes. Unfortunately, under the current industrial model, companies have killed most of them. An ideal model for the 21st century industry is one in which the society and large companies encourage the generators of fields of resonance to start companies as vehicles for it, and provide coordinators to create new markets by networking across current industrial categories with large companies. New architecture for the 21st century must be designed so as to maximize the conditions for generating fields of resonance.

References

Amano, H., N. Sawaki, I. Akasaki and Y. Toyoda (1986), 'Metalorganic vapor phase epitaxial growth of a high quality GaN film using an AlN buffer layer', *Applied Physics Letters*, Vol. 48, pp. 353-355.

Amano, H., M. Kito, K. Hiramatsu and I. Akasaki (1989), 'P-type conduction in Mg-doped GaN treated with low-energy beam irradiation (LEEBI)', *Japan Journal of Applied Physics*, Vol.28, pp. L2112-2114.

Anderson, P. and M. Tushman (1991/1997), 'Managing Through Cycles of Technological Change', *Research/Technology Management*, May-June, pp.26-31, reprinted in M. Tushman and P. Anderson eds, *Managing Strategic Innovation and Change*, Oxford: Oxford University Press.

Bassett, R. (2002), *To The Digital Age*, Baltimore, MD: Johns Hopkins University Press.

Christensen, C. (1997), *The Innovator's Dilemma: When new technologies cause great firms to fall*, Cambridge, MA: Harvard Business School Press.

Fujimoto, M. (2005), *Senmonshoku no tenshoku kozo* (The Job-changing Structure of Professionals/Specialists), Tokyo: Yushindo.

Kuhn, T. (1962), *The Structure of scientific revolutions*, Chicago: Univ. of Chicago Press.

Matsuoka, T., H. Tanaka, T. Sasaki, and A. Katsui (1990), 'Wide-gap semiconductor (In,GaN)', Inst. Phys. Conf. Ser. 106, pp.141-143 (Proceedings of Int. Symp. GaAs and Related Compounds, Karuizawa, 1990).

Nakamura, S. (1991a), 'Handoutai kesshou no seicho hoho' (Methods to Grow Semiconductor Crystals)', Japan Patent No. 2628404.

---- (1991b), 'GaN growth using GaN buffer layer', *Japan Journal of Applied Physics*. 30, pp. L1705-1707.

Nakamura, S., Y. Harada and M. Seno (1991), 'Novel metalorganic chemical vapor deposition system for GaN growth', *Applied Physics Letters* 58, pp. 2021-2023.

Nakamura, S., T. Mukai, M. Senoh and N. Iwasa (1992), 'Thermal annealing effects on p-type Mg-doped GaN films', *Japan Journal of Applied Physics*, 31, pp. L139-141.

Nakamura, S., M. Senoh and T. Mukai (1993a), 'P-GaN/n-InGaN/n-GaN double heterostructure blue-light-emitting diodes', *Japan Journal of Applied Physics*, 32, L8-10.

---- (1993b) 'High-power InGaN/GaN double-heterostructure violet light emitting diodes', Appl. Phys. Lett. 62/19, pp. 2390-2392.

Nonaka, I. and H. Takeuchi (1995), *The Knowledge-Creating Company*, NY: Oxford University Press.

Nonaka, I. and N. Konnno (1998), 'The Concept of "Ba": Building a foundation for knowledge creation', *California Management Review*, 40/3 pp.40-54.

Shimizu, H. (1995), 'Ba-principle: New logic for the real-Time emergence of information,' *Holonics*, 5/1, pp.67-79.

Shockley, W. (1984), 'The Path to the Conception of the Junction Transistor', *IEEE Transactions on Electron Devices*, ED-31 No.11, pp. 1523-1546.

Yamaguchi, E. (2004), 'The Dimness of the Blue Diode Lawsuit', *Japan Echo*, 31/3, pp.27-31.

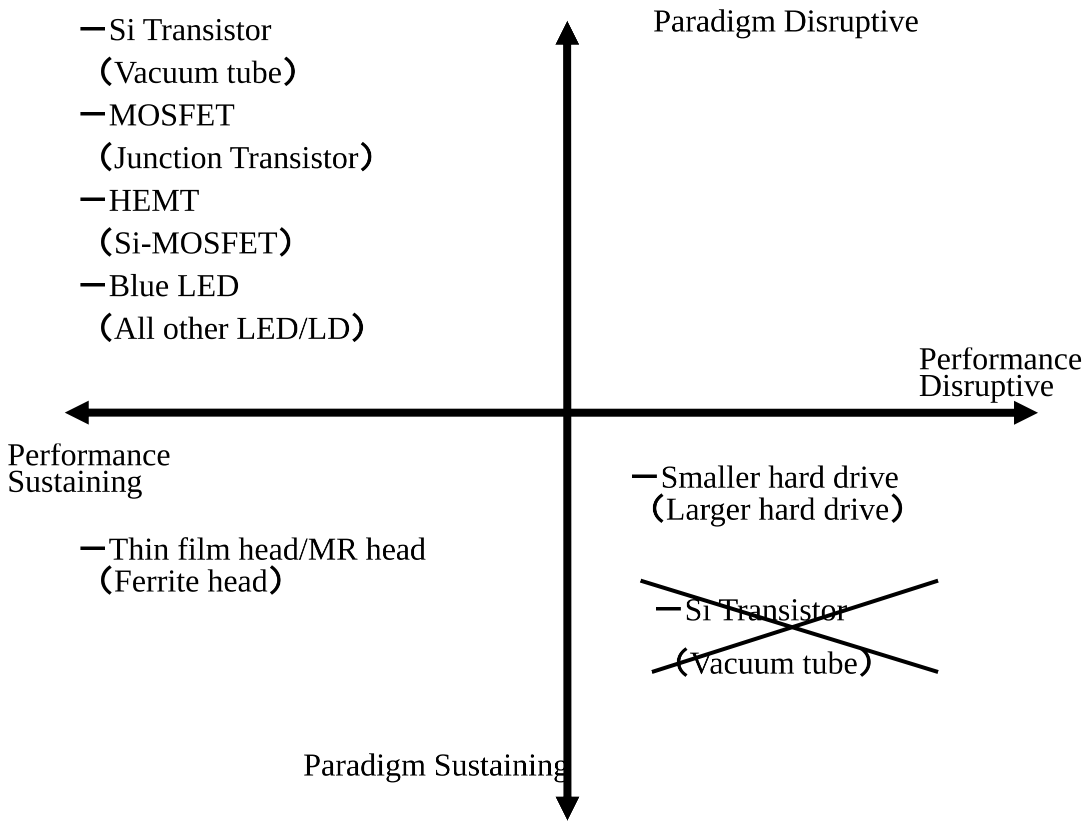

Figure 1 Paradigm disruptive innovation and performance disruptive innovation

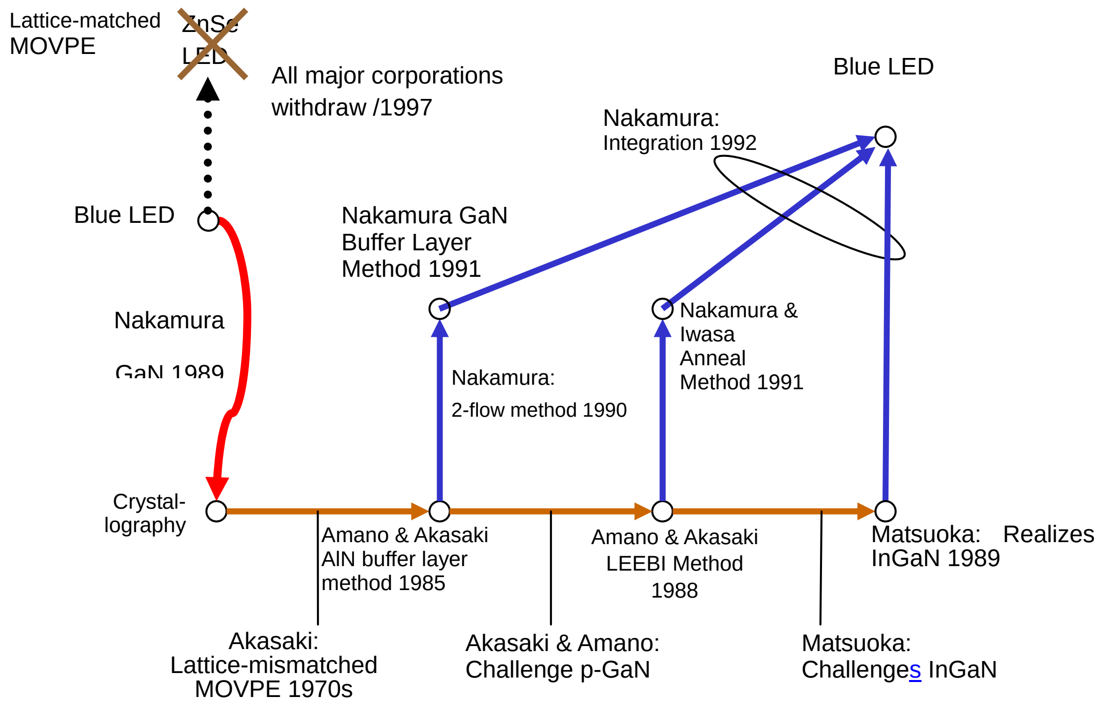

Figure 2: Innovation process for the Blue Light Emitting Diode.

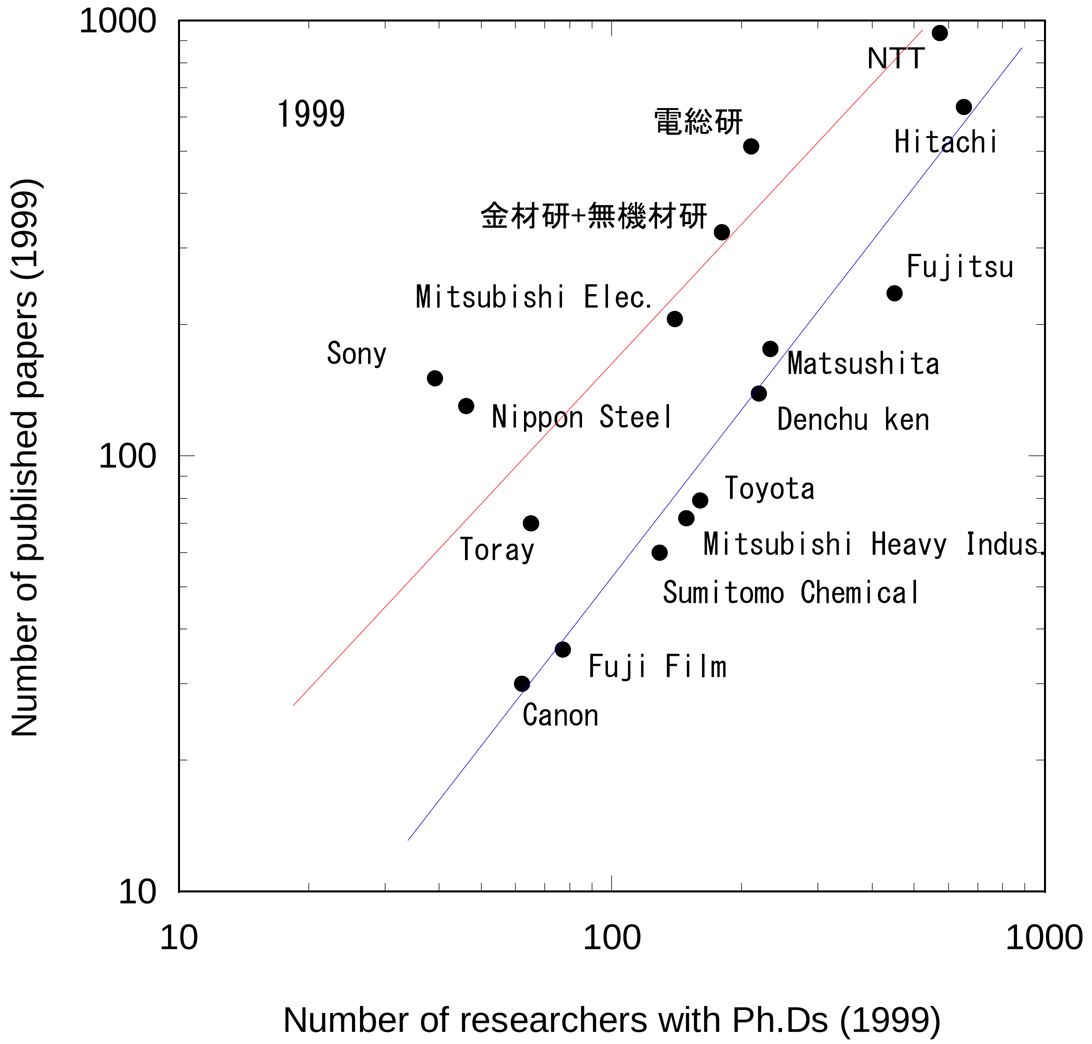

Figure 3: The number of published papers vs. the number of researchers with Ph.Ds in 1999

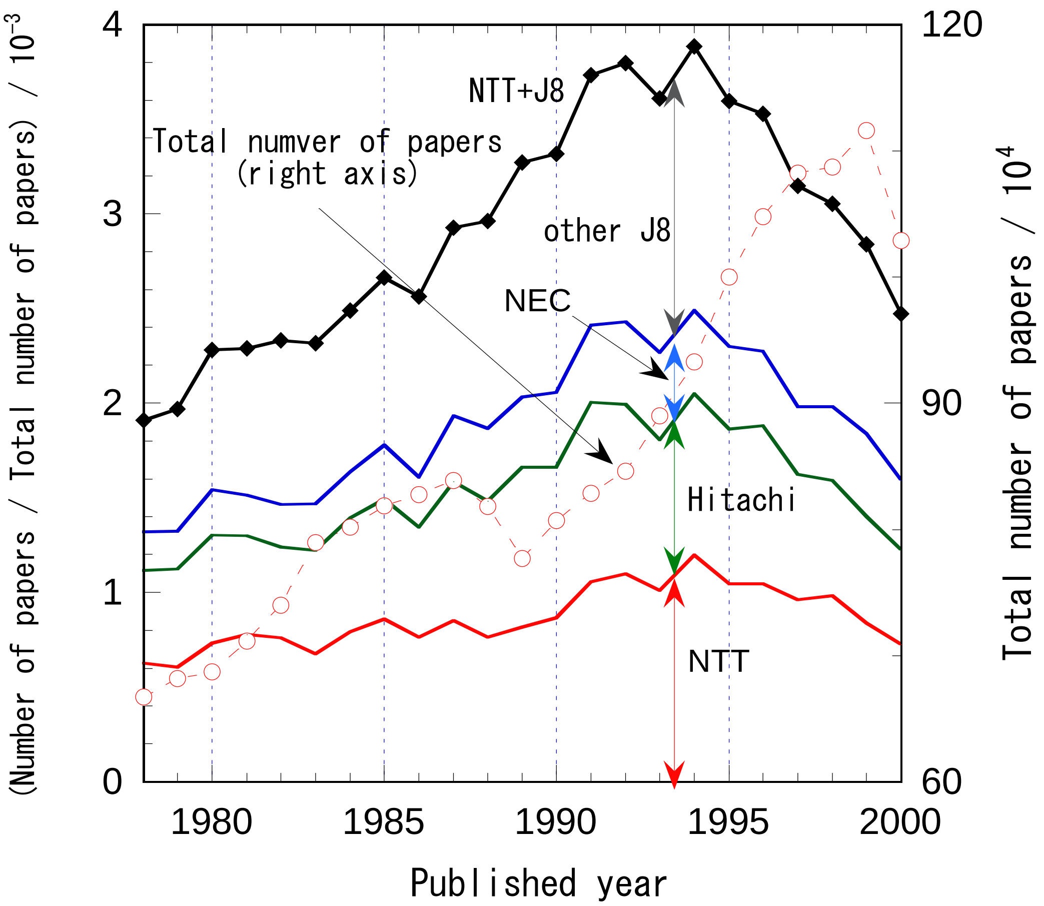

Figure 4: Trends in academic papers by year.

Note: The number of papers is normalized by the total number of papers on the respective data bases. J8 = Canon, Fujitsu, Hitachi, Matsushita, Mitsubishi Denki, NEC, Sony and Toshiba.

Source: SciSearch, Social SciSearch, respective years.

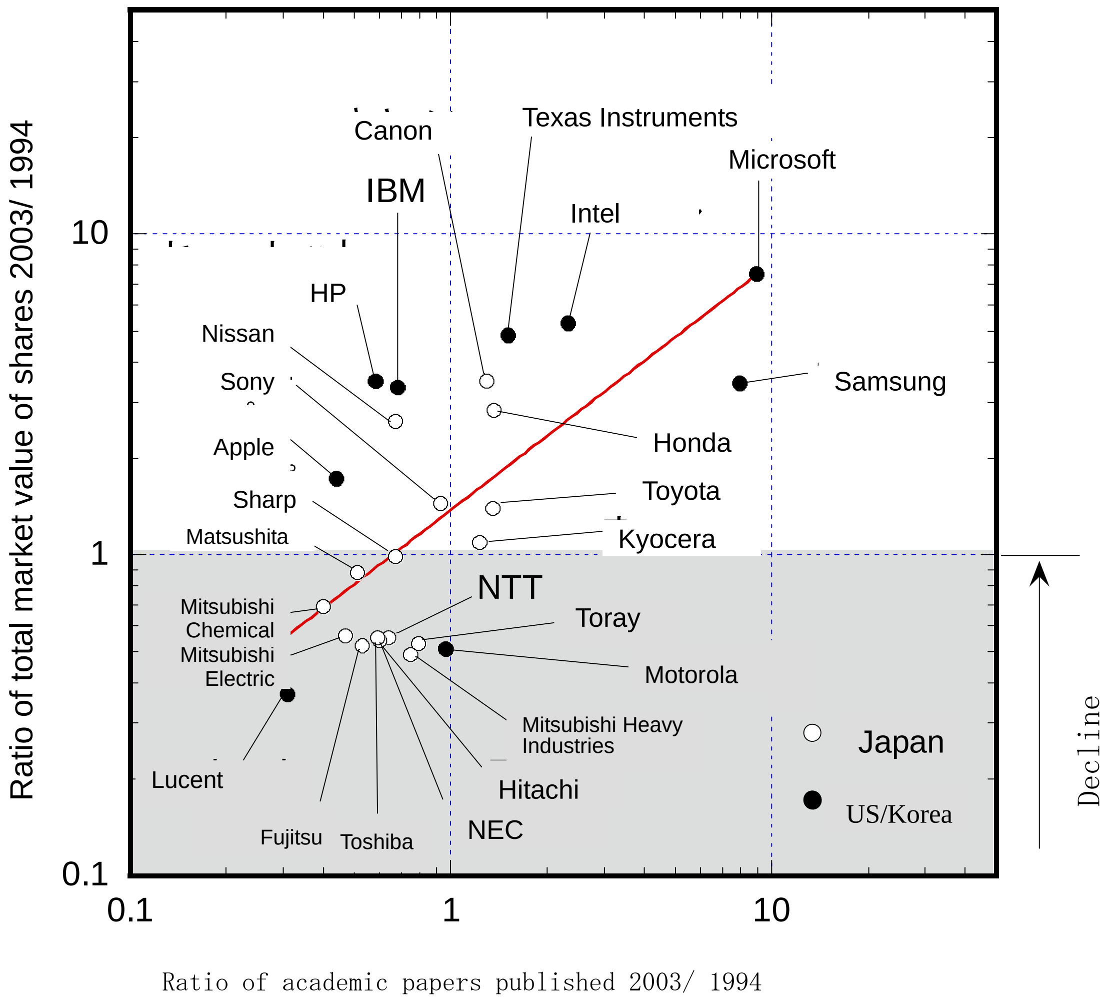

Figure 5: Academic papers and company value

Note: Lucent denominator is papers for 1996

Source: SciSearch, Social SciSearch, respective years.