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POINT OF VIEW

Studying modular design: an interview with Carliss Y. Baldwin

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Abstract
Carliss Baldwin has brought large advances to the fields of technology and organization design. In this conversation, we trace her academic career and the stories behind her most important contributions, including her work on options, transactions, and her seminal book, Design Rules Volume 1. We sketch out how her understanding of modular systems evolved over the course of her career and provide advice to other academics.

Keywords Modularity · Design · Real options · Technology

JEL Classification G3 · L1 · O3

Introduction

From cars and computers to solar panels and electric power distribution systems, we are surrounded by designs and systems with varying degrees of modularity. In the last couple of decades, we have witnessed the advent of sophisticated technologies and the subsequent explosion of modular design. Professor Carliss Baldwin has actively kept track of these changes and has devoted much of her scholarly attention to studying the processes of modular design construction and emergence, and their impact on firm strategy, business ecosystems and industries. Undoubtedly, her most influential work is Design Rules Volume 1: The Power of Modularity, published in 2000 and co-authored with Kim Clark. The book provides a theory of how embracing modularity can lead to unprecedented innovation and growth, illustrated with the evolution of the computer industry. She is currently in the process of writing Design Rules Volume 2, which will complement the first volume with insights on phenomena of more recent importance, such as ecosystems, platforms, and open source.

On a short biographical note, Prof. Baldwin is the William L. White Professor Emerita of Business Administration at the Harvard Business School (HBS). She completed her BSc in Economics from the Massachusetts Institute of Technology (MIT) in 1972 and went on to obtain an MBA and DBA from HBS. She was appointed as an assistant professor at MIT in 1977, eventually moving on to HBS in 1981, where she obtained tenure and has continued to serve since. Prof. Baldwin began her academic journey in Financial Economics under the guidance of Robert C. Merton, Franco Modigliani and John Lintner, before moving into the field of Innovation and Technology Management. Her academic contributions have had a large impact and influence, having generated more than 20,000 citations. An overview of her selected published works by research area is presented in Table 1.

In this article, we present excerpts from an interview conducted with her during the Strategic Management Society's 41st Annual Meeting. In preparation for this interview, we met regularly with Prof. Baldwin over several months to develop an in-depth understanding of her career, allowing us to present the personal journey behind her professional milestones. The transcript has been edited for clarity.

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Table 1 An overview of Prof. Baldwin's selected published works by research area

Research topics	Published works
Real options/investments	Baldwin (1982), Baldwin (1983), Baldwin (1986), Baldwin (1991), Baldwin et al. (1984), Baldwin and Clark (1992), Baldwin and Clark (1994), Baldwin and Meyer (1979), Baldwin and Ruback (1986)
Modularity	Baldwin et al. (2000), Baldwin and Clark (1995), Baldwin and Clark (2003), Baldwin and Clark (2006a), Baldwin and Clark (2006b), Baldwin (2019)
Transactions	Baldwin (2008), Baldwin and Woodard (2009), Luo et al. (2012)
Software and IT design	Baldwin et al. (2014), Lagerström et al. (2019), MacCormack et al. (2006), MacCormack et al. (2012)
User innovation	Baldwin et al. (2006), Baldwin and Clark (2006c), Baldwin and Von Hippel (2011), Lakhani et al. (2013)
IP modularity	Baldwin and Henkel (2015), Henkel et al. (2013)

The interview

Finding your contribution

Interviewer: We would like to start at the very beginning of your academic journey. You studied financial economics in graduate school and both your pre-tenure and immediate post-tenure work is in finance, in the fields of real options and irreversible investments. We know that finding your contribution can be a little challenging as a scholar. How did you first approach contributing to your field, and do you have any advice for young scholars on how to find our own contributions?

Prof. Baldwin: I was fortunate to be present at the very beginning of option theory. I was an undergraduate at MIT with Robert Merton as my undergraduate thesis advisor. I wrote my undergraduate thesis on convertible preferred stock, for technical reasons. First, you can solve for their value, and second, I didn't have a lot of math expertise, so that was important. In the years after my undergraduate period, while I was a doctoral student, options theory exploded, and I was struck by the fact that everything had been done. So, I went looking for something different, and I found something where the assumptions of option theory didn't hold, which was irreversibility and real options. My doctoral thesis was on irreversible investments, a case where the assumptions that allow you to price options don't hold. That was how I got into irreversibility, and it was my brand for a long time.

Interviewer: You were completely in finance at that moment. What happened at the juncture when you got tenure?

Prof. Baldwin: When I became an assistant, and then an associate professor, I tried to do real options and modelling of interesting situations. And I was very much—"you got a project, I'll join". As a result, my contributions were all over the map, although there was a theme of real options and irreversibility running through them. Back in those days, option theory was everywhere and there was a great deal of excitement over it. Real options were supposed to be the next new corporate thing. Just as financial options were taking over the financial world, real options would take over the corporate world.

I did a lot of pricing of real options and was struck by the fact that it was a lot of math and assumptions. Robert Merton once said to one of my co-authors, while I was there, about the assumptions behind the real options model – "you just make it up!". Therefore, we would just make stuff up and that was a little dissatisfying at some level. Then it struck me that we were bringing in this horrendous mathematical apparatus and telling managers that they should pay attention to it. But they didn't understand the assumptions that went into calculating the real options and how the model worked either. And when you brought them the results, I didn't think that my technology was adding a lot relative to the project value. So, I went looking for a place where the real options were bigger.

I got tenure with a completely empty pipeline. I told people that I had a lot of projects under way, but the truth was, I really didn't know where I was going. And it didn't strike me that any of my projects were going to have big effects. Around the time just after tenure, Kim Clark approached me to join his interest group that he was setting up at the Harvard Business School, basically as a token. He had funding and support from the dean, but it had to be a cross-unit effort. At the time, Kim was the head of the Technology and Operations Management (TOM) unit, and there weren't any other people in his interest group from other units. It was somewhat of the right thing at the right time because I was looking around for some place where options mattered, and Kim was looking for a token from another unit to justify his project.

You see, I was a very arrogant young scholar. I was trained in finance. I did not walk with a swagger but I might as well have because finance was it. Everybody else was wrongheaded. Maybe there was a little bit going on in strategy, but it was a new field and I thought—"if people just understood finance a little better, they would make better decisions". I don't know why people put up with me. But I did start going to the TOM seminars, in the days when there was a great deal of opposition to finance. It was the time of the rise of Japan and Japanese manufacturing systems. Kim and his doctoral students and co-authors did a lot of work there. There was a sense that the finance people in corporations were ruining operational investments because they would force everything into a strict financial straitjacket and do capital budgeting, and if the project didn't clear on the capital budgeting front, then they would cross out the project. And this was destroying American competitiveness. "A strategic investment is one you overpay for", that was what my field felt about strategic investments. So, we had a lot of dialogue and conversation.

Through these seminars, I kept hearing about flexibility, Japanese production systems, but also about this thing called modularity. Everybody was talking about modular designs. And I thought, if flexibility is an optional thing, then it has value because of the options it brings you. And if modularity is a way of achieving flexibility, then there must be an options model in there somewhere. That was where I got to.

Eureka moments and switching fields

Interviewer: I would like to stay with that switch to technology and starting the collaboration with Kim Clark on modularity. We know you came from the field of real options. How did you end up settling on modularity? How did you know that this was going to be your thing that you were going to continue doing and pushing? Do you have any advice for mid-career scholars seeking to do the same thing, trying to make an impact by switching fields?

Prof. Baldwin: I didn't know. It took a while for me to get there. I knew it was a puzzle and I would worry about it. How do I capture this thing? There were reasons for it being difficult to do. At Harvard Business School, you write a case when you're puzzled about something and if you want to understand a new set of practices. So, Kim and I wrote a case on Sun Microsystems. In that case, I was mostly fascinated with a little bit of modularity and the working capital management stuff going on. Sun was doing what Dell later became famous for—they had a negative cash conversion cycle. And the case was a lot about that, but it was also about some of the strange things they were doing like designing chip architecture and licensing it to many people at a very cheap cost – that was the SPARC (Scalable Processor Architecture) architecture. While doing this project with Sun, I kept coming back to the question of—"how do you model this modular system?" In some ways, creating the SPARC architecture was a module in Sun's greater business model. They were thinking in a mix and match kind of way. It was all very inchoate.

And then I had two very big moments. One was on the Massachusetts turnpike coming out to where I am now in Stockbridge. I was thinking about the model and the issue was that I needed to be able to split a distribution and add up the pieces that would be the modules, so splitting and substitution would be the two most basic modular operators. And it struck me in the car that the only distribution that behaves well, under addition, is the normal distribution. I was working with uniform distributions, which I found easier, and they were very interesting but got very bad when you tried to add across uniform distributions, because, of course, they become normal eventually. So, I said—"well, if it wants to be normal, let's make it normal". And not log normal, which is financial option theory, but normal normal. I got home, and, my poor family, I just disappeared for the weekend. By the end of the weekend, I had a tractable model and a numerical example. It said that a reasonably sized modularization of going from one integrated design to 25, which is approximately what IBM did in System 360, with optimal levels of experimentation on each module, and a symmetric case, would create a value increase of 25 times.

From that point forward, I knew this was my thing. I said to Kim—"a 25 times value increase pays for a lot of investment. It also brings about real disruption to an industry structure. Sun is rolling tanks into their market. And if modularity is this strategic opportunity, then it's a very big one, and IBM is in trouble". This was approximately 1989–1990. And IBM was in a lot of trouble. Not from Sun, but from its own PC and modularization. The fragmentation of the industry was made possible by the underlying modularity.

Kim was quite dubious about this. I had this one project, and he had doctoral students, five or six of them. And I was getting pushback from finance people when I would give seminars based on my model, talking about modularity. Traditionally, people in finance tend to be very dubious about what people tell them. They would say—"well, how do you know there's such a thing as a module?" And I would say—"well, the engineers see them". And they said—"so you believe them?" Turns out that the engineers don't often see the modules very well. I went on the software track after Design Rules 1 because I felt unjustified.

And then, I learned about a technique called design structure matrices (DSM), which was being pioneered by Steven Eppinger and some other people at MIT. This was an objective way of tracing a network of tasks or information flows for a complex artifact. I said—"this is it—this is an objective proof of modularity, as modules are going to show up as isolated blocks in a DSM". And I took this idea to Kim, and he said—"DSMs are the most arcane and nerdy of techniques, why are you wasting your time on them?". The implicit message was that no CEO is ever going to look at a DSM, and that is all Harvard cares about—what CEOs look at. And it is true, no CEO to my knowledge has ever looked at a DSM. But I not only had a 25 times value increase caused by upstart firms, but also an objective way of talking about a module, a way that would stand up to the scrutiny of my finance colleagues if they ever cared. Kim had some time but was very doubtful. I said—“Kim, this is it, this is our thing!” And he was nice enough not to throw me out.

Writing Design Rules Volume 1

Interviewer: You have said that you first wrote a paper with these ideas with Kim Clark which went through four rounds at *Management Science*, lasting four years! How did you decide to turn this paper into an actual book? This must have been a challenging process. What happened in this journey to put your ideas in a book, leading up to its publication?

Prof. Baldwin: We are now talking about 1994 or maybe 1996. From this vantage point today, I must thank the *Management Science* referees. Frankly, I think we got to four rounds only because Kim was so eminent that the *Management Science* editors didn't want to turn him down flatly. He was and still is a very big name in the field, even though he's been doing other things for 20 years now. But the last set of review reports were all different. It was like the blind men and the elephant; each one had a different take on what the paper was about. Nobody who read the paper understood option theory, even after we tried to explain it somewhat in the paper. And one referee or the associate editor wanted us to talk about the implications for field service. I could tell you about the implications for product design for new products, but field service? No way, I don't know. And they wanted us to shorten the paper by 30%. I told Kim that they were not getting us, no matter how much we tried explaining and grounding this in actual theory. They want us to cut it, and then they won't understand anything at all. This was a total non-starter, it was impossible.

With my doctoral students, I was always very sensitive to this—getting a revise and resubmit that is really a reject. It is saying that we'll just set the bar so high that any reasonable person will withdraw, and we won't have to reject you. This is a lesson for young scholars.

So, we had to write a book. And approximately between 1994 and 1996, I did write a book. Kim was busy, but he would read the chapters. Then I went to the ASSA (Allied Social Science Associations) meetings. By then, I joined Mike Jensen's group at Harvard, and he was a quasi-finance person too. I went to the meetings and in the course of that Christmas break and the meetings, I re-read the whole manuscript. And I said to Kim—“It's not there. The book doesn't capture what we're trying to say, it's just slipping out through the cracks. We have to start over”. And around that time, Kim said—“well, I'm going to become dean at the Harvard Business School. But I'll keep working with you”. So, we started over from scratch.

To prevent the ideas from leaking out through the edges we had two rules. First, everything we said had to be grounded in a real technology. We spent a long time debating computers versus automobiles. Kim was a big automobile fan, but I had no interest in cars whatsoever, I didn't really give a damn. So, we ended up with what we had in this case on Sun—computers. As to rule number two, this was a time that most of you are too young to remember, but there were a ton of biological metaphors in management, especially in *Harvard Business Review*-type management. Everybody was doing biology metaphors. And I said, rule number two is no metaphors. The only metaphor in Design Rules isn't really ours, it’s from Armen Alchian. It's about evolution as a grasshopper jumping from point to point. Kim and I just thought it was too nice. It wasn't our metaphor, it was his, so we left it in the book. But so those were the two rules, and they kept us focused. They made us really go into it. We had to assemble our own proof, where it wasn't going to be like a large numbers empirical proof. It had to be examples that really illustrated the points in the theory. And so, it began.

Interviewer: Just before you published *Design Rules 1*, how was it perceived by your peers? Was there any talk around it before you published it?

Prof. Baldwin: No, none whatsoever. You know, Kim was very good, meeting with me once or twice a week and when he read the stuff, he was very thoughtful. But at the same time, he asked me to become the Senior Associate Dean. In that role I was, at first, responsible for faculty planning, which is recruiting and everything up to the first promotion review, bringing in and shepherding people through the first few years. Then I became Senior Associate Dean of the doctoral programs. I was completely engaged socially with lots and lots of interesting people across all the fields of management, and I was teaching organizations and markets. I was in Mike Jensen's group, where I had moved from finance because I felt I was going to have to understand agency theory better to say what I wanted to say. Mike Jensen is a force of nature; he had a lot of information. He and I were sparring a lot, because although he's a force of nature, we have totally different intellectual styles. He taught me a lot.

But nobody knew what I was working on with Kim. Everybody knew I was working with the dean on a book project, but no one knew what. When you write a book, the first advice people give you is—“who is your audience, be sure to define your audience, make sure you write for an audience”. But when our book was in the gap time between submission and publication, I told Kim—“in the whole world, there is not a single person who wants to read this book”. My own metaphor was the medieval painters who would do a beautiful painting on the back of what was going to be shown to the public. I felt we were doing a painting on the back of an altar or something, and that it had no justification, other than we were doing it. I also felt I was between galaxies, I left one galaxy, and I was in the very, very dark places of the universe, trying to find another galaxy. And there I was, and who knew how long the journey was going to take? Or if there even was another galaxy out there. It was a very strange time intellectually and professionally.

Interviewer: The book definitely ended up finding its audience, making it even more interesting to hear what it was like for you at that time. That it was much more difficult than one may expect when looking at a successful book.

Prof. Baldwin: Just as I thank the referees of *Management Science* for putting me on an uncomfortable path, I was also lucky. At that point, I was still relatively young and able to change fields. The field of real options was going nowhere, it was getting more arcane and more difficult. It was also narrowing in on the tiniest of tiny problems, there was no attempt to say something that would matter to anybody. Except for somebody who was trying to justify some very expensive dam project or runway project and he needed a high powered, very arcane study to be conducted and was willing to pay for it.

My field was imploding, so one of the things I jettisoned was Mertonian option theory. There was a big conflict then between dynamic programming and Mertonian option theory. Option theory was supposed to give you the right discount rate, while dynamic programming just used the interest rate. This was a debate that made no sense. The other problem was that I was dragging around a theory based on dynamic stochastic processes, which approximate the stock market with frequent trades. In what I was talking about, which was new products, there is no observable, dynamic stochastic process. You might as well just get rid of it, which simplifies the model greatly. And hardly anybody reads that part anyway.

In a way, I was lucky that the field I started in was so obviously stuck. And that I wasn't going to unstick it. Because life is long, and most people find that the work they've been doing is not quite what they want to be doing. For me, it was so clear that I had to do something else. I really had no choice.

New communities around modular design

Interviewer: After publishing *Design Rules 1* you left finance behind and shifted to multiple new topics in technology. How did this time of transition play out for you?

Prof. Baldwin: Design Rules 1 was published on March 6, 2000. That was incidentally the day the internet stock market bubble began to break. The high point of the NASDAQ is on that day, and it is burned into my memory, because we then watched the market drop. Following the dot-com crash, there were all these scandals of Enron and abuses at companies where high-powered incentives caused executives to do terrible things. High-powered incentives were what Mike Jensen had been recommending for about 10 years, so Mike changed his whole view of the world based on that crash and the aftermath, the accounting scandals, the Enron scandal, and all of that. He just changed his view—stopped preaching high powered incentives and started preaching integrity.

I was an efficient markets person and *Design Rules 1* is based on the theory of efficient markets, meaning that the price revealed by the market is the right value. Bob Merton was always really good at explaining that, when it might appear that the market was wrong, it really hadn't been, there was just new information that came in. But I kind of lost my faith. I always had great faith in market prices, but the weight of the evidence for me became overwhelming.

I thought, from a technological point of view, what information would the market need to really get those prices right? From my perspective, those turned out not to be in the realm of possibility and could not happen. This notion we believed in finance, that the disparate pieces of information in the market aggregate to something correct, I no longer believed in. Real technologies do not aggregate, they get combined. And they're not going to add up to anything sensible. So, I was at sea for quite a while.

Interviewer: You joined some new communities and collaborated with new people. How did you find them and your new topics?

Prof. Baldwin: It was a godsend. There were both academics and practitioners, computer scientists mainly, who understood *Design Rules 1* and were excited by it. I was also writing short translations of the main messages in *Design Rules 1* because it's a long book, so I was trying to simplify the gospel. At one point, very soon after *Design Rules 1* was published, I was giving a talk at one of the big hotels in Boston. This was a talk to a bunch of managers who had no interest in and no understanding of what I was saying. I was not reaching them. But Eric von Hippel came up afterwards and introduced himself, and began inviting me to workshops and conferences, and I learned about open source. This was in the early 2000s when Linux, the penguin, was going everywhere. Everybody was like—"gee wow, what is going on?". I said if my theory is any good then modularity should explain something about why this open source model functions.

One of the things that I did not explain in *Design Rules 1* was why transactions would go at certain places in a task network. In my field, management strategy, this idea of a task network didn't exist, the closest were Michael Porter and Nicolaj Siggelkow, who would do value networks. Masa Aoki, a game theorist from Stanford, invited me to a big conference in Paris. I wrote the first draft of what became the transactions paper ("Where do transactions come from?") for that conference, and little by little I built a community.

Michael Jacobides and Sid Winter, who were at Wharton, invited me to give a talk before the book was published.

Michael was doing stuff on mortgage banking, the fragmentation of the industry and industry evolution. I thought this fragmentation you're seeing in mortgage banking is exactly like the fragmentation you see in computers. They're dividing up the tasks into the component parts and creating contractual interfaces between them. Michael was doing industry architecture and came in as a visiting scholar a couple of times. I wrote for the first *Journal of Organization Design* issue, because of my colleague Michael Tushman, a paper called “Organization Design for Business Ecosystems”. So, I had a very early paper with ecosystems in the title and that gave me some credibility in the ecosystems field. Annabelle Gawer, who was a doctoral student of Michael Cusumano and Rebecca Henderson at MIT and was doing work on platforms and platform leadership, had read *Design Rules 1*. She had the idea of an edited volume and created a conference as an opportunity to publish something on platforms. I said—“well, platforms are just like at the center of a bunch of options”. So then Jason Woodard, a doctoral student, and I wrote the architecture of platforms for that book.

I was moving forward just little by little, inch by inch. Hardly anything got into a refereed journal, maybe one or two things. But the next thing I knew I was totally busy and had several communities that I was welcome in, and those are still my communities today.

Know your phenomenon

Interviewer: You talk about new communities and how you met new people after 2000, requiring you to learn a new language as well. What has changed in the last two decades? You are now finishing up the second volume of *Design Rules*, and your research has played a major role in the fields of management and innovation. How do you view your academic legacy of the past two decades?

Prof. Baldwin: First off on learning a new language—I told you that I was very arrogant. As a young scholar, one of the things I was arrogant about was the organization field. I didn't even know the field of organization design existed. And I didn't want to study organizational behavior. And no one I respected at that time was working in any of it. But then, during my shift in interests, I started writing for more management-oriented journals. Suddenly I'm getting all these questions about James Thompson and Charles Perrow, and I now needed to know these scholars to communicate with my new audience. I came to know a lot about user innovation and open source through Eric von Hippel. I didn't know about capabilities either, much less about dynamic capabilities, and had to learn about them as well. But these classics in management of organizations, I just didn't know about them, which made me, by the field’s standards, ignorant and not worth talking to.

I was lucky that the economy was moving in my direction and platforms and ecosystems, which started out as two different and pretty obscure things, were now in the mainstream. The whole of *Design Rules 1* does not contain the word platform or ecosystem, even though others had published on both by then. I wanted to call ecosystems “modular clusters” and a platform “design rules”. But later I was perfectly willing to lose that naming battle to join a larger army of folks who got together and became platform and ecosystem scholars. Another shift was that the classic organization design was very hung up on Chandlerian corporations. When these started becoming meta organizations, managers needed help in thinking that way. So, a whole bunch of scholars started pursuing the field.

Interviewer: Do you have any closing advice for young scholars who want to study new problems in your area of technology and management, a field that has been developing a lot recently?

Prof. Baldwin: What to say to young scholars? First, get tenure. I've known a lot of people who crashed and burned trying to be too radical too soon. Tenure gives you the space and the latitude to say something, and if your own field is imploding as mine was, then you have to go on to something new. Secondly, trust in serendipity. Serendipity will bail you out time and time again. Trust in colleagueship.

I watched how over the years large sample empirical work took over many of the fields of management. This was not true when I was a doctoral student, where it was all about theory. You wanted to be a theorist, and then suddenly with the large sample work there's lots and lots of very good work and very exciting work and wonderful work. But it's like the keys under the lamp post—it just excludes so much else that is out there that does not lend itself to measurement of that kind but will lend itself to reasoning, logic, examples and modeling. The best thing Kim Clark taught me, because I was a pie in the sky theorist, was to really understand the phenomenon. Get close to it, talk to people, study the documents and then figure it out. If you're far away from the phenomenon you can do these pretty models and be celebrated by economists and sometimes by management scholars, although I think management scholars have more discernment about what's real. I've seen so many people get lots of excitement about their work, but because it's not close to reality, it never goes anywhere. So, you know, be ready.

Conclusion

Postscript by Carliss Y. Baldwin, March 25, 2022

In addition to the acknowledgements at the beginning of the article, I would like to thank my interviewers, Navya Pandit

and Constantin Prox who prepared so thoroughly and made everything so easy.

In reflecting on what to say in this Postscript, I find I need to acknowledge my intellectual debts to two great scholars whose visions and works gave me the confidence to take an errant path, skirting the edges of several fields but far from the center of any. They were my teacher and mentor, Robert C. Merton and my coauthor and counselor Kim B. Clark. Merton's father, Robert K. Merton was fond of the phrase "I have stood on the shoulders of giants." In my career, it was my good fortune to encounter two giants. Their works concerned very different phenomena and pointed in different directions. But their ideas converged in unexpected ways to reveal expansive new vistas.

Bob, perhaps unknowingly, was a strong Platonist. His genius was and is to strip away many distractions found in practice to arrive at a truth-revealing ideal form. Working in the field of finance (which is abstract to begin with) he swept away the unnecessary assumptions in the original Black–Scholes model to obtain a more general model—and more beautiful proof—that their option pricing methodology actually worked (Merton 1973a, b; Scholes 1998).

The Black–Scholes-Merton model in turn was an interesting contribution to the social sciences. It did not describe practice at the time. Rather, it changed practice by showing how someone using the model to trade against agents *doing anything else* would earn risk-free profits with no need for capital investment. In this sense, the model was a description or prediction about the world *as it might be*. The model combined with the trading strategy *was a technology*—a recipe for changing the world. It was "an engine, not a camera," as Donald MacKenzie convincingly demonstrated (MacKenzie 2008).

However, as a young scholar, the model I found most inspiring was Merton's intertemporal capital asset pricing model (Merton 1973a, b). In contrast to most of the mathematical models used in finance and economics at the time, Merton's model allowed for dynamic uncertainty. Asset prices would change in probabilistic ways as new information came in. The model brought all of time (infinite horizon), space (all traded assets), and all uncertainty about the future into a single encompassing framework. It indicated how assets would be priced relative to one another at a given point in time, and how those prices would evolve in response to unexpected events affecting each asset. The scope of the model was breath-taking.

If Bob Merton followed Plato, Kim Clark followed Aristotle. "It is from particulars that universals are derived."[1] Kim was fascinated by practice. He loved puzzles—like how unionization might lead to an increase in productivity in a cement plant or how Japanese automakers achieved greater efficiency and quality using the same equipment as US automakers. And he differed from many economists in the 1970s and 1980s by actively seeking insights and explanations from participants in the events he studied.

What impressed me most, however, was Kim's understanding of and ability to build theories—specifically theories about objects and designs. Kim's most cited work today is his paper with Rebecca Henderson on architectural and modular innovation (Henderson and Clark 1990). Many people know that they built up their theory by analyzing the structure (architecture) of an everyday room fan. In the working paper that preceded the article, there is a detailed algebraic model that attempts to characterize the fan's parts in terms of their functions, and then aggregate the functions to arrive at a value.

The functional analysis was cut from the published article—correctly so because it would not have worked for most readers. But it blew me away. *This was what we needed* to build an economic theory of technology and designs. Unfortunately, the model did not cohere: the functions as written were intractable. Kim and Rebecca were trying to build a complex edifice with inadequate mathematical tools. It was an idea far before its time—a time that might never come.

Kim was good at recognizing when a theory didn't work. He and Rebecca carried on in the paper and built an elegant verbal theory of how information flows within an organization would come to "mirror" the product design and production processes in use, and how those patterned routines could lead to severe strategic mistakes and "the failure of established firms."

He had better success in building a theory based on the concept of mirroring—a theory of the duality between a "hierarchy of designs" and a "hierarchy of cognition" (Clark 1985). The fundamental idea was that people can't imagine what they want from an artifact except by using it. Thus, complex products evolve through a process of desire-trial-experience-criticism-desire, etc. On each iteration of this cycle, users will desire new features and properties, they would not have imagined before experiencing the prior model.

In *Design Rules Volume 1*, Kim and I basically merged Merton's and Clark's preferred methods. We shoehorned complex designs into a simplified options model. We then carefully studied *the structure and the history* of the evolving designs and their market valuation. We assumed mirroring between product and organizational structure—as a product was split into modules, the activities (tasks) involved in making modules could be distributed across many separate companies. We supported our arguments by analyzing the evolution of a small number of computer systems in great detail using the tools we developed.

[1] Aristotle (NE 11426) translated by MacIntyre (1988).

Our arguments had three major weaknesses, however. First, we never really explained what mirroring meant or how it came to be. Second, our models, taken from finance theory, were based on a critical variable: "sigma" or technical potential, which turned out to be unobservable. Last, our principal method for determining the modularity of real systems—so-called Design Structure Matrices or DSMs—could only be applied *retrospectively* to existing processes or systems. Thus, our theory could explain structures after they emerged and even explain why one structure succeeded better than another. However, *in cases involving new technologies*, the theory could not predict structure or advise decision-makers on the relative merits of different strategies. What good is that?

Needless to say, in *Design Rules Volume 2*, I am striving to address these shortcomings.

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