Strategy as a Detour on the Way to Mars

A biographical interview with Richard Rumelt[1]

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My introduction to Richard Rumelt was an emailed syllabus for his Strategy seminar in December my first year as a PhD student. The first assignment was to read Chandler's, "Scale and Scope", and to write a fifteen to twenty-page paper. The class began the first week of January, but the 780 page book wouldn't be available in bookstores until December 18. (These were the days before Amazon). We all assumed this was a screening mechanism, but instead Rumelt found himself with six hundred pages to read.

Rumor had it that to work with Rumelt (which everyone wanted) you had to climb Mt. Whitney. One of his passions is ice climbing, but Whitney didn't require ice climbing or any other technical skill, it was a straightforward hike-- 22 miles up and back, 6400 vertical feet (beginning at 8360 ft elevation), taking 10 to 20 hours. I hate anti-gravity sports, but recognized this as the price of admission. It was glorious at the top, so we all let our guard down. Someone asked what it had been like to be on Family Feud. I said it was a blast—"when it was over I kept waiting for producers to call, but when they didn't, I decided to do the PhD". Rumelt, who I had assumed was preoccupied on his perch 15 feet away, chimed in, "You should have kept waiting".

Ultimately I realized what game we were playing. Rumelt was the Wizard of Oz and we were Dorothy. As long as we were willing to travel all of Oz, survive its horrors, manage not to be intimidated by the blustering Wizard, and be willing to go back for the broom, there was a guy behind the black curtain who was brilliant and magnanimous.

Rumelt *is* brilliant. He created the foundations for two of the fundamental questions of Strategy: Why do firms differ (How much does industry matter, 1991, Uncertain Imitability, 1982), and What value does corporate headquarters add? (Strategy Structure and Economic Performance, 1974)

But Strategy wasn't the original path for Rumelt. In fact, it couldn't have been. Even by the time he reached Harvard in 1965, there was no strategy curriculum:

"Strategy wasn't even a word that anybody in business used when I started studying it. There were two books (Chandler and Ansoff) and no articles. I guess the Harvard group began to use the word as part of their General Management curriculum, and the conceptual scheme was, 'what does the CEO do?' They had written a set of cases on the Swiss watch industry, and they began to notice that within that industry the firms were different. They had different

[1] This interview was conducted in 2012.

production systems, different distribution logics, different employee relationships, and they began to use the word 'strategy' to describe this different mixture of policies and purposes."

In 1963 there was no intellectual frame. If you turn to Economics, they had IO, which said firms either collude or they don't. The firms we got out of economics were basically choosing q, and real firms seem to be choosing a lot of things other than q, and that mixture is called 'strategy'. Most of strategy is about complementary choices, trying to get things lined up."

Rumelt's original path was electrical engineering (EE).

"I was very good at electromagnetic theory. I had some intuitive grasp for it. I was a boy scientist. A Ham radio operator. I built electrical things or electrical motors. I was fascinated.

This fascination started with a six-year old Richard Rumelt sitting on his mother's kitchen counter:

My mom is washing the dishes and there on the counter is a hairpin—a u-shaped piece of steel. I pick it up and stick it in the light socket. And there--just bang! Explosion. My finger burned and a huge spark jumped all over the counter and then on the floor. I'm like, 'Wow! Is that cool or what?"

Rumelt's first job out of Berkeley with a BSEE was at Jet Propulsion Lab (JPL). His concentration had been systems engineering, which at Berkeley meant automatic control systems.

"So I wind up in the systems division at JPL (designing spacecraft) which has nothing to do with automatic control systems. The phrase had been taken from the Air Force manuals on systems engineering, which were rewritten from AT&T's systems engineering, which basically had to do with, 'how do you conceptualize an entire system?" With AT&T the issue first arises out of long-distance telephony, which is what you might call a trade-off problem: how close do you put microwave stations to one another? You can put them close together and use low power or you can put them farther apart and use high power. So it turns out that's not all that complicated, but it's different than designing the microwave receiver and transmitter.

And with something like a Polaris missile you've got hundreds of such tradeoffs. So I'm involved in doing that. And I've written about this in the new book--these tradeoffs only happen in any serious way when you have a constraint, and our constraint is usually weight--we only have so many pounds. So we're constantly trying to get things to fit together in some way saving weight by having systems share functions".

That was my first real introduction to strategy in a sense--making a bunch of complementarities work. Nothing I learned in Berkeley prepared me for this because there

wasn't any equation. It's just that you've got to hold a bunch of things in your head and think about them: we've got this radioactive thermal thing and the trouble with it is that it sends out radioactivity which screws up our measurements, so let's put this thing on the end of a long bar. That bar starts to go like this (motioning oscillating). *Well that means you've got to put more gas in your attitude control system. So the consequence of that design is that you need a more accurate antenna. And maybe we shouldn't do that. Maybe we should put more shielding in it. So that's systems engineering. Systems engineering is like designing a BMW. Everything interacts with everything else, because you're trying to get high performance, or because you have a tough constraint, or both. I found it very frustrating and very fascinating.*

After a few years wrestling with the technical issues surrounding projects such as the Mariner Mars and Voyager, Rumelt set his sights on being the project manager for the Manned Mars Expedition (then twenty years in the future).

He applied to the management programs at both MIT and Harvard, and chose Harvard because it offered him an additional $500. Because he already had a Masters (MSEE), he applied to the PhD program. (In engineering there is no schism post-masters between the applied degree and the research degree).

When he entered HBS, he was originally in decision sciences, but discovered that Bruce Scott and Joe Bower were asking very interesting questions and not coming up with much: why do some firms succeed competitively and others don't.

We were trying to understand diversification. There was a guy named Leonard Wrigley who had invented these four categories and started classifying firms into single, related, dominant and non-related categories. Wrigley had done a sample of a hundred American companies and classified them, but didn't know what it meant.

I wanted to do a thesis on does it matter: Does Corporate Structure Matter? And the idea I had was to take some kind of exogenous shock…and compare the responses of a division of a diversified company with a single business company. Does Maytag behave differently than the washing machine division of GE?"

Nobody at Harvard wanted to get behind this. Nobody wanted to help me do this. It was considered like Physics, you know, atomic physics. That's way out there.

But they were interested in this business of diversified firms, and Bruce Scott said, 'Well, why don't you go and take the Wrigley categories and relate them to organization structure? Because Chandler says strategy is structure. Well, you can go in and see.

This was basically Bruce's idea. He got four guys to do this. He got Derek Channon *in the UK*, Heinz Thanheiser *in Germany*, Gareth Dyas *in France*, Bob Pavan *in Italy and me in the*

US. All four of us did strategy and structure. Except I did more because I took the categories and busted them into seven and I added the economic performance stuff that nobody else did.

While Rumelt's first academic job was at Harvard helping them open an Iranian campus that later became a training camp after the Shah was deposed, and while Rumelt did a 3-year stint at INSEAD in the mid-ninety's, the bulk of his 39-year career has been spent at UCLA. He credits UCLA with forcing him to think more about fundamentals—to be abstract, and to learn economics.

"I never took an economics course in my life--ever! Once I was here at UCLA it became evident to me I was going to have to learn the discipline, and I couldn't just go around saying, "Strategy is the big picture".

"In 1979 we had a retreat. I had to present the future of strategy, so I remember having this easel drawing of three circles. I'm going on about the three circles, and Armen Alchian, the inventor of the new institutional economics, raises his hand and says, 'I don't know the subject. Strategy, I've never heard of it'

"Well it's general management. Why some firms succeed. Why they fail"

He says, "Do you have any propositions?"

I remember going home. I'm just angry. I'm telling my spouse, "I quit. Why do they let people like that in? Why do I have to deal with them? What did he mean "do I have any propositions?"

So, it was out of that rage that I said, "The proposition is these firms are different." So, I began to really think hard about why there are sustainable differences. And so I began to read more economics. I began to look in economics for why there are differences, you know, because there isn't anything since the early ideas that some corn fields are more productive than others.

I was sitting there reading the Bell Journal, and there's a paper, which I could pull up today, having to do with the cost of coal mining. And the guy draws three cost curves. And they're labeled by seam width: 6-inch seam, 12-inch seam, 18-inch seam. And the 18-inch seam has a lower cost of mining than the 6-inch seam.

Now looking at the graph, I'm thinking, 'but you don't know which cost curve you have until you dig the coal mine. So I had that basic idea just looking at these three cost curves, and I began thinking, "what's the equilibrium in a situation where the cost curve isn't revealed until after you invest? After you build the business?"' And I got into the math. It got a little hard and I went to Steven Lippman and I said, 'How do you solve this problem?' He looked at it overnight and he said, "There's no equilibrium." I said, "Give me a break. Of course there's an equilibrium!" So we went around that for awhile.

The intuition is obvious. If you don't know what your costs are until you jump in, at some point the industry is going to be full up and the companies inside are going to be different. This is not complicated, but what's interesting is that once you set that up, it predicts that the average return in the industry would be above the cost of capital because the failed firms are on the other side. It predicts an association between market share and profitability and it predicts an association between industry concentration and profitability. All with price-taking competition. So the simple model predicts the three or four basic IO outcomes without any reduction in competitive activity whatsoever. So I was very pleased with this.

This response to Alchian's question became, "Uncertain Imitability," Rumelt's most cited paper. His personal favorite however is the 1991 article, "How much does industry matter", which he started at Harvard in 1975.

I was looking around to do some research to get promoted. And I didn't know what exactly I was doing. I didn't understand variance components very well, but I did compare diversified companies with single-business companies. I got the basic result, but my statistics weren't as sophisticated as they should have been. I sent if off to AER and they sent it back saying, "not interested, plus your statistics aren't sophisticated."

So I let it lay. And then Richard Schmalensee, who's a pretty good economist at MIT, wrote a paper using FTC line of business data saying that industry explains most of the variance. I knew it didn't because I'd done this previous work. It took me awhile to understand his paper and to realize what was wrong with it.

He needed to treat business unit effects the same way he treated corporate and industry effects and I knew how to do that. I went and got hold of the line of FTC line of business data and I did it properly. Sure enough, the business unit effects way outweigh the industry effects, which to me was a very important emotional and intellectual closure on the early Harvard study (1963) saying "these watch firms are different from one another."

I sent the results off to the American Economic Review, and they wrote back saying, "It's not interesting". And so I wrote back. I said, "Look, you have a lead article by a very top economist saying the opposite. Now I don't see the fact that that's totally wrong is uninteresting. Now it may be that I'm wrong, but I just don't see how it's uninteresting."

So they wrote back and said, 'Well, we don't like your statistics'. They found some (reviewer) who said there's auto-correlation in this data. He didn't know what he was talking about, but they used him and they axed it. So, you know, economics is a bit of a closed shop"

So I was kind of frustrated and I sent it off to SMJ and they reviewed it for a long time and finally published it. That was exciting, and I did win the Best Paper Award. There have been a lot of replications of those results since. The basic results stick.

That these two papers provide the foundation for a field of strategy separate from the strategy, conduct, performance (SCP) paradigm within Industrial Organization, might suggest an Ivory Tower academic, but Rumelt is as committed to practice as to academic rigor. His deftness managing the tension between managerial relevance and academic rigor is reflected in his being profiled here, while also having been profiled in the 2007 McKinsey Quarterly article, "Strategy's Strategist". It is also reflected in his current work, which includes definitional work on opportunity cost on the one hand, and the book "Good Strategy/Bad Strategy" (Crown Books, July 2011) on the other.

The concept of rent is screwed up and it permeates a lot of things in strategy. The place where it shines the most error is in things like the EVA measure where they take profit and subtract opportunity cost of capital and say what's left is economic profit. That's a mistake. And it's not obvious at first glance.

Economists say the opportunity cost of doing A is the value of the forgone alternative B that could have been done instead. The cost of the corn is the value of the beans that could have been grown. This is just silly because there is always a foregone B as close as you like to A, just change it by a hair. You can decide to pick some B for comparison, but that is just analysis, not an economic principle.

The impetus for *Good Strategy/Bad Strategy* was a 2007 meeting with the former director of the CIA and others on national security strategy.

Barry Watts, an analyst for a think tank on K street, says "One of the reasons we don't have a meaningful national security strategy is that nobody knows what strategy means." So my job there was to say "is business any better at this?" And my answer was basically, "they're no better. In fact, the military is better at it than business." But at the broad level of national strategy, or the DoD strategy, there are just broad visions of wonderful end states you wish you could get to:, "Our strategy is peace in our time." I began to go down the list of companies I've worked with who said things like 'our strategy is to increase our bottom line to 50%', stuff like this. This is confusing goals with strategy. And I began to really analyze this, what is going on here?

People like me and my colleagues that teach this stuff and research this stuff spend most of our time on airy fairy details that aren't the issue, that aren't the problem for firms that mess up strategically. The don't mess up because they didn't get the five forces right, or they didn't get the resource-based view just quite right. They mess up because they don't actually have a strategy at all. Their strategic plan is a wish list of numbers that they would like their managers to hit, but that's not strategy.

So then if that isn't, what is? And I'm forced to define it in a context-free sense. So I spend a year working on those terms saying what is it that really makes a strategy a strategy,

because it isn't just a set of economic principles because the concept applies to the military, it applies to the government, it applies to anybody.

Strategy is a response to a challenge. No challenge, no strategy. Strategy has to deal with not just what you wish, but why it's hard to get there. What are the obstacles? What are the particulars? I think a strategy has to identify the structure of the situation. You can't have a strategy for dealing with the financial crisis unless you know why you have this financial crisis. It's coherent. It's a bunch of things that focus energy and resources on some critical issue. And it has a proximate objective. It has something you're doing about it now. Those are the context-free elements of strategy.

I'm trying to say strategy isn't everything. It isn't magic. It doesn't lead you to the golden path to continuing success. It's not the sustainable competitive advantages of myth. Strategy is part of the creative process. It's a hypothesis, like a scientific hypothesis. You're testing it with money and real action.