Ashridge Strategic Management Centre

Members Meeting

27th September 2018

Minutes of Meeting

In attendance

Jérôme Abrahmi	DOMO Chemicals
Sumit Bahukhandi	Direct Line
Paul Barrett	Babcock International
David Bowerin	Independent consultant, formerly Citigroup
Helen Dawson	Shell
Tom Ford	Rolls Royce
Nick Lawson	BP
Mark Meldrum	National Grid
Philip Meyers	ABF
Christoph Naumann	Siemens
Stein Rasmussen	SBM Offshore
Ben Slater	BP
Paul Titcomb	Atkins

From Ashridge Strategic Management Centre

Felix Barber
Stephen Bungay
Rebecca Homkes
Jo Whitehead

Guest Speaker

Will Bunker, GrowthX

Rebecca Homkes and Will Bunker: Strategy and Innovation – Ideation, Growth and Scaling: Learning Better Lessons from Startups

Having been struck by the very low RoI most corporates achieve through innovation and the unrealistic advice corporates are commonly given about what they can learn from startups, Rebecca has been working for about 18 months on the topic of corporate innovation, and specifically with entrepreneur and investor

Will Bunker to identify how disruptive innovation really works and what lessons corporates should actually learn.

Rebecca began by inviting Members to describe the biggest challenges they are facing with innovation at their companies. They responded as follows:

- We are a company whose core business places high value on control, safety and reliability. One consequence is that we cannot change quickly. Digital is a problem for us. We are trying to encourage people to take controlled risks without imperilling the core business or having an impact on customers.

- We have a portfolio of stable businesses with a tradition of focussing on financial returns which makes fundamental innovation difficult.

- We have a lot of very good engineers but they are a long way away from the markets and they do things the German way.

- We are very slow.

- We are very cash generative. The pressure is on generating cash now and that is how we are managed. I do corporate venturing, but we are not geared up for years of investment before you can spin off a venture. Amazon or Uber might generate a lot of cash now but it took long term investment. Our incentives are not aligned to innovation.

- At the managerial level people don't want to take risk. It is not actively discouraged, in fact they have permission to do so, but taking risk makes them personally feel massively exposed. There are a lot of lower risk things to do. So, things are underfunded to begin with. If they do in fact work out, people can jump on the bandwagon but they are reluctant to drive anything. It is also hard to generate a good story about innovation, so people can't communicate its attractiveness. People want to be second in innovation, not first.

- Managing shareholders is tricky – we are focused on profit generation rather than capital growth and don't have a license to divert a lot of money towards innovation.

- There is a very real need to change due to digitisation, but the daily pressure is on charging out hours to generate current returns.

It was noted that most of the practices and habits described were the result of the genuine needs of the core business. Innovation – at least fundamental innovation – is difficult not because companies are doing anything wrong, but because there is a clash between those practices and the practices required for successful innovation.

Rebecca suggested that there are three core issues: many companies are not really ready to innovate; many are not learning the right lessons from startups; and many are not adapting their execution approach to the one needed for innovation.

While these three pieces form her larger research in this issue, the focus of the meeting will be on the second piece as it is the focus of the collaboration with Bunker.

Will's Story

In order to help us gain an insight into how the mind of a real entrepreneur works, Rebecca invited Will to describe his journey to becoming a tech founder.

Will grew up on a farm, but was trained as an industrial engineer. He worked in a factory (and hated it) before getting his first real job with Arkansas-based oil & gas company N B Hunt, which sent him on a series of journeys across Russia in 1993-4.

While there, he found that making a phone call took five hours, whereas an emerging technology called the internet enabled him to communicate. The machine he used to access it reminded him of one used by a local John Deere dealer in Arkansas. He therefore became very interested in the internet because it provided a radical solution to a serious problem he had personally encountered, and looked as if it had the potential to used universally, not just in Russia.

A second formative experience was an interlude mining and transporting gold in Nicaragua, which involved levels of personal risk he had never encountered before. This made him more tolerant of risk in general.

In 1995 he left Hunt, went to Dallas and teamed up with a friend to set up an internet business. They did not know what that business would be, but did know that the internet had the potential to cut communication costs to something close to zero, which would be of high value to most of the population.

In deciding what the business should be, they were not driven by any vision or a desire to change the world, but analysed what data they could find. They worked out the specifications of the business in the way that engineers typically approach problems, focusing on the principles that have to be fulfilled to make it work and the boundary conditions that it has to satisfy.

The largest ISP at the time was AOL, and 50% of its revenues came from chat rooms in which the main topic was dating. The dating business was also very lucrative for conventional newspapers. So they decided to create something in that area.

They were faced with major financial constraints. Dallas had very small VC community and they only had around $5,000 of their own money. So they modelled out everything that had to be true for the business to work and then tested those conditions in ways that their budget would allow.

The basic questions were:

1. How many people would put personal details on the internet?
2. Could they find enough sites to advertise on?
3. How would they charge (e.g. using email, advertising or subscription)?
4. How could they generate a positive return per visitor?

They spent £2,000 to find out that 5% of the population would put personal details on the internet, at a cost of 5 cents per person. They protected privacy by creating a web form that only e-mailed responses. They initially manually created and uploaded each profile. Match.com were ahead with 20,000 users, but they thought they could catch them.

Finding enough sites to advertise on was hard but they found that online web chat sites were the answer.

In deciding on a charging model, they observed that people were using 900 numbers, so they built a tracking system to measure revenue depending on which model they saw and after 18 months decided on subscriptions.

Only minimal resources were committed until they had proven the business model. It was very lean. Their backer wasn't in a position to give them $10m – he was Will's boss in his day job. They had a hypothesis that the revenue would scale with the number of profiles and estimated from that that they would need 20,000 profiles, so raised £100,000 to achieve that, by which time revenue per visitor had risen from 0.1 cents to 18 cents. They called the business One-and-Only.com.

Match.com had raised $10 million to reach 20,000 users, but went on a national branding campaign, which was a waste of money. They key was to learn the right things efficiently. The difference in cost was a factor of 100.

In 1999, Match.com was bought by Ticketmaster (now IAC), and the same year Ticketmaster also bought One-and-Only for $47m and merged it with Match.com.

Will's story challenges the popular view of entrepreneurs. His approach was rational rather than visionary, analytical rather than intuitive, and systematic rather than down to chance. Whilst it was personal experience that led Will both to see the internet as an area of opportunity and be willing to take the risks associated with being an entrepreneur, he developed a methodology that was grounded in an engineering approach and therefore in principle reproducible. He has since dealt with many entrepreneurs and believes that although a few succeed because they are just lucky, most of them in fact think in the way he describes. As an investor he certainly would not fund any that don't.

Better Learning from Startups: Six Myths and Six Lessons

A mythology has grown up around entrepreneurism that has led to some ideas about how to be like and work with startups; Rebecca and Will say these are either misleading or downright wrong. They need to be replaced with quite different ones.

Three of these myths relate to getting to the idea and creating a *value hypothesis*:

1. You should act like an entrepreneur → instead you should question fundamental assumptions and look for exponential changes in costs and capabilities.
2. Startups are sexy, so get sexier → instead you should be structured and methodical.
3. You must innovate or die → rather you should encourage exploration.

Three further ones relate to growing the idea and developing a *growth hypothesis:*

4. You should celebrate failure → instead, invest in ways of discovering the truth cheaply and efficiently.
5. You should focus on getting the best returns → instead you should focus on reducing the unknowns.
6. Process makes you slow → the right processes to create efficient learning allow you to experiment and innovate.

Many of these lessons are sought after because companies want to be able to change – to adapt to change, to lead change. All companies are good at incremental change or they would not survive. In order to deal with fundamental or disruptive changes they often try to learn from or partner with startups. Rather than seeking to become like startups themselves, they should seek to become good partners to them, which means not being identical but complementary.

1. <u>Generating ideas and a value hypothesis: questioning assumptions</u>

The first step in methodically searching for better ideas is to look for exponential changes in technology that are either radically reducing costs or changing capabilities or both. It is the intersection of these curves where disruptive innovation happens, and the real entrepreneurial opportunities exist.

In 1997, the internet was, in Will's words, 'a giant stinking mess', which, compared to other innovations in communications such as CB radio, looked like a geeky fad. However, the number of people connected to it began growing exponentially, a trend which was itself driven by an exponential reduction in the cost of both bandwidth and hardware. Timing is critical. If One-and-Only had been launched 4 years before it was, it would have failed because the costs would have been too high and the revenues too hard to find. It was these very same calculations that led Jeff Bezos to start Amazon at about the same time. One reason the original Match.com failed was because it was started too early.

Big opportunities are often identified by looking at very broad trends. Around 2000, Will concluded that China was about to embark on a period of exponential growth, lifting millions of peasants out of poverty. The first luxury item people buy is meat, and to satisfy demand China would have to import it. (In 1970 the average Chinese person ate 14 kg of meat a year, but now they eat an average of 55 kg.) So Will bought farmland in the US at $500 per acre. Its value today is $5,000 per acre.

Elon Musk thinks in a similarly broad way. Some of the first cars were powered by electricity, but petrol won because it was cheaper and more reliable and easier to store. However, solar energy capacity has been growing exponentially worldwide for some 20 years, driven by cost reductions which are themselves added to in a positive feedback loop with capacity growth. The cost of silicon PV cells has fallen from $76 per watt in 1977 to $0.30 in 2015. Musk simply asked how you would build a car if electricity were super-cheap and the result is Tesla; he asked what company you would build if solar were super-cheap and the result is Solar City.

Broad phenomena like this can affect many unrelated areas. Will believes that when solar energy has made electricity super-cheap, it will reduce the cost of desalination, which will make fresh water plentiful and so enable the transformation of non-productive land into farmland. He therefore wants to sell the land he has in Arkansas before its value plummets.

The phenomenon to look out for is when *exponential capability increases match exponential decreases in cost*. That will lead to disruption.

To identify these phenomena, read broadly about technology and innovation, especially if they change cultural behavior. Behaviour changes often emerge among just a few people, so consider how people will act if everyone is doing things that way. Then consider what compromises are being made by current businesses and ask what the business would look like if you were starting over again with a clean sheet. This means being curious, exploring methodically, recognizing that most ideas are not worth the effort - Will commented that he is surprised when he is right – and size the amount of money you spend appropriately. Timing is important and only a game changer will justify the large amount of effort involved.

Incumbents often fail to recognize when disruption is a threat because they fail to challenge assumptions which prove to be wrong. Challenging fundamental assumptions is the second part of the first lessons that corporates should do: identify, question, and challenge their fundamental assumptions.

Some of the biggest disruptions happened when companies failed to do this. Kodak believed that people cared about the quality of a photo and thus dismissed early digital photography because the quality was poor. They failed to recognize that convenience was highly valued and that as digital technology developed quality would rapidly improve. Dell assumed that because people's data is sensitive they would always want to have servers in-house - until the cloud proved them wrong. Retailers assumed that people would always want to physically interact with a product in a store - until Amazon proved them wrong.

2. <u>Generating ideas and a value hypothesis: be structured and methodical</u>

Many innovation consultants encourage 'wandering around' and 'ideation' sessions. While nothing is inherently wrong with this, if your companies survival depends on innovation, this seems to be putting a lot on faith. Generating random ideas in an innovation offsite does not always work. Nor does trying to imitate the culture of a start-up by creating a cool, sexy environment. This confuses a surface manifestation with the underlying cause.

Instead, adopt a systematic approach by:

Conducting an offsite to reveal your company's core assumptions;

Designing some fast, low cost experiments to test them;

Creating a place where they can be carried out at low risk;

Training people how to think from first principles.

One way to identify critical assumptions is to examine your P&L to identify the biggest cost items and ask what would happen if they were to change radically. Take each line item and ask: 'what would happen if this was zero' 'what would happen if this was 3x? 20x?' For example, what would happen to Fedex if drones meant drivers could deliver 20x more packages a day? Another example would be if the compromise between scale and personalisation were to be broken, enabling you to personalise a product at low cost, as with t-shirts. The application of inkjet printing to textiles promises to be a way to do this. Where there is a compromise being made which can be broken, there is a pathway to innovation.

The assumptions underlying an existing business that matter are those that create a vulnerability. If a belief you have *has to be true for the business to work at all*, it makes you vulnerable and you need to have a continuous process for testing its validity. If it starts to erode or change, you can quickly become exposed to disruption.

Some members described assumptions underlying their own business models:

- Oil and gas will remain the prime sources of energy;

- Power generation will remain centralised and flow in one direction, rather than distributed and flow in both directions;

- Aircraft will remain the fastest vehicle for long distance travel and people will use them at short notice. Elon Musk set up his Boring Company in 2016 because he had noticed that tunneling costs have fallen from $400m a mile to $40m a mile. He is already imagining what could happen if they fall to $4m. At that point long tunnels for cars or trains could replace jammed highways and planes operating in crowded airspace.

3. <u>Generating ideas and a value hypothesis: encourage exploration</u>

Timing is critical.

The fear in the minds of many is being too late. Walmart, for example, didn't really work out when to start worrying about Amazon until it was too late. There is a systematic dynamic underlying this. It is difficult to think exponentially because human beings have evolved to think linearly. In a linear world you have plenty of time, but in an exponential world nothing actually changes for a long time until suddenly everything changes and there is no time left to adapt or respond. You need to plot out mathematically when it is time to put in the effort to get going. The other problem is that you make a ton of money in your core business. You are already successful – much more so than virtually all start-ups – and any new project doesn't make much difference, so people won't want to work on it. You must validate the assumptions that would make it work and be ready to move at the right time.

However, it is possible to be too early as well, before the cost and capability curves cross. At the front of the curve it's not interesting, but at the back end it is too late. How should you approach this?

One suggestion is to create a 'corporate 'sandbox', which is a 'safe' place where mistakes can be tolerated, to regularly explore emerging trends. Facebook carries out many of its experiments in New Zealand, a market which provides sufficient size to learn, but limits scope so that not all of its customer base is subjected to continuous experimentation.

4. Growing the idea and developing a growth hypothesis: discover the truth cheaply and efficiently

The challenge is to validate the idea and then ride the growth wave upwards at the point at which the numbers will start to matter to the company as a whole because exponential scaling is possible. The way to validate is not to argue about who is right, but to conduct fast, cheap experiments.

Large corporates spend a huge amount of time selecting, preparing and debriefing experiments. Conventional wisdom has it that the reason is a fear of failure and that start-ups celebrate failure. This is a myth. In fact, start-ups do not waste time and resources trying to get things right but carry out large numbers of cheap experiments with fast feedback loops. The winners are the fastest to find out what is wrong. The idea that they celebrate failure confuses another superficial effect with the underlying cause. If you are trying to get things right, a failed experiment is one that does not work. If you are trying to find out what is wrong, a failed experiment is one that does not yield any information. The most valuable experiments are ones that surprise you by disproving a fundamental belief you hold.

Contrasting views of how to experiment are illustrated by the US pet and pet food retailer Petco, and Amazon[i]. Petco runs 75 experiments a year (down from 100), which have to be approved to avoid duplication and 'small ideas'. Amazon focusses instead on reducing the time and cost of experiments in order to become an innovation machine. If Petco's 75 carefully planned experiments have a 50% chance of success Petco will learn 32.5 things a year.[ii] Amazon conducts 10,000 experiments a year without putting any time into trying to guess the right answer. If they each have a 5% chance of success, Amazon will learn 500 things a year, giving them a learning advantage factor of 15x.

Lowering the cost of experiments enables you to conduct the large number of them you will need to carry out in order to be successful. There is nothing new about this. Thomas Edison carried out thousands of experiments before deciding how to make light bulbs. Today, James Dyson works in a similar way. In the 1950's, instead of setting up a lot of real stores to test the most efficient layout, McDonalds spent a day with some students trying out alternative formats drawn in chalk on a tennis court, which could be altered within minutes and cost almost nothing to do. Today, simulation and VR software can play a similar role. Some generative design software can run 10 million variants to deliver a set of real options within hours.

Today there is also infrastructure to draw on. Crowdsourcing enables companies to experiment in communities, and resources like Upwork enable them to cheaply access specific skills from anywhere on the planet. Local ecosystems are growing up to facilitate experiments, such as Techshop in Silicon Valley for manufacturing systems or Trinity Groves in Dallas for restaurant concepts.

Resources are limited so even if you are carrying out thousands of them, experiments do have to be prioritized. This is in many ways the hardest skill to acquire.

The way to do it is to break experiments down into the smallest element that will yield any information. All aspects of the business should be tested, with particular weight given to customer behavior. They should then be ranked according to the resources required, the expected payoff, and most critically of all, velocity, i.e. the time it will take until learning is achieved.

Everything that can be digitised should be, in order to profit from Moore's law. Having tested the technology involved, which they describe as a mixture of 'computer vision, sensor fusion and deep learning', and run some pilots, Amazon has launched six 'just walk out' stores under the brand name 'Amazon Go'. Customers download an app which they use to check in, take the items they want and leave. They do not have to queue and pay at a checkout, but are charged on their Amazon account. In China, Alibaba already has 65 Hema Supermarkets which use similar technology but also offer home delivery and a robot-operated restaurant.

5. <u>Growing the idea and developing a growth hypothesis: focus on reducing unknowns</u>

Great innovators do not use an innovation funnel or pipeline to identify the highest RoI innovations. Spreadsheets only have value if the past is a reliable guide to the future. Since the whole point of disruptive change is to create a break with the past, any numbers put into a spreadsheet will be pure fiction and are likely to do more harm than good. The important metrics for experiments are velocity and cost.

However, the point will come when the innovation project is generating numbers of its own, and these can then be used to create a 'conversion funnel'. This tracks the moves from isolated wins to opportunities to qualified conversions which enable you to quantify the number of customers which should be targeted and therefore the future resources needed to scale. When the data you have generated enables you to produce a reasonable quantified estimate of potential materiality and value, that is the time at which to move top talent into the new business and commit resources.

6. <u>Growing the idea and developing a growth hypothesis: design processes to enable efficient learning</u>

Most processes within an existing business are designed to increase efficiency, prevent mistakes or manage to the lowest common denominator because of a perceived lack of employee competence or trustworthiness.

In the context of innovation, processes like these are unhelpful or destructive. Efficiency will become important in the scaling phase, but not when innovations are being developed. Trying to prevent mistakes and managing to the lowest common denominator will raise the cost of discovering the truth.

However, this does not mean that all processes are bad. Effective innovators actively design processes to lower the cost of generating insights and uncovering the truth.

In response to some questions, Will and Rebecca commented that a sandbox is any context where failure is acceptable. Facebook uses a bounded geography, but it could also be a customer segment where making mistakes does not affect the major KPI's. Learning from experiments involves having a process for adopting lessons within the organisation. A tyre company was looking for a market segment that would promote fast learning. They found the segment that wears out tyres fastest and was most interested in managing tyres due to the high cost for the segment. Learning takes place on both sides.

Working with start-ups: can elephants dance with mayflies?

Rebecca and Will also introduced the topic of an upcoming article entitled 'Can elephants dance with mayflies?' Corporations should not work with startups to become more innovative or build an entrepreneurial culture. They should do so in order to achieve a fundamental advantage in terms of a KPI, create a new customer segment or new sources of customer value, or to challenge an existing assumption. The best collaborations are when both parties are challenging the same fundamental assumptions.

Working with startups is challenging because elephants have a long lifespan, and mayflies have a short one. Most startups have an 18 month funding runway. The first thing corporates need to understand is the level of urgency which this implies.

Before working with a startup you should ask yourself:

> Whether you are prepared to sign a deal in a month or less;
>
> Whether you have a budget which will enable the startup to reduce their burn rate or raise more money;
>
> Whether the project will challenge an assumption or make your company more valuable if it works.

One successful partnership is between eFishery, which make underwater robots with sensors and the AKVA Group of Norway, which farm salmon. By being able to monitor how much feed is actually eaten by the fish, the robots are reducing AKVA's feed costs by 21%. The project began with an experiment in three nets and is now worth £160m. There was a matching sense of urgency on both sides.

Common traps are:

Not realising how fast startups have to move. Most have a funding runway of 18 months;

Treating them like a normal vendor and negotiating aggressively;

Talking to random startups without knowing what you are after.

If what you are after is not urgent, not important or you cannot make someone in your organisation accountable for the relationship and the results, don't bother.

We ended the afternoon with summary comments and questions from Members.

Round the table comments

- We are working with two startups. How do you decide when to acquire? The suggestion was to acquire or build an execution team if the work could affect the core business. Some people worry about teaching competitors something if the startup is not acquired, but in practice they do nothing. Nor, on the flip side of the coin, should you worry about stealing.
    - ➔ Update, Rebecca and Will are working on a framework for thinking through this
- We focus heavily on process and tests can take two years. Machine learning will reduce their cost, but we need a better process.
- To get innovations going we have to first beg for money and everyone waits for someone else to start. We need a better structure.
- Our question is knowing when to scale up. The answer is to do the maths and if you can play it out by making plausible, evidence-based assumptions rather than guesses, you know enough to scale.
- We are currently funding 10-20 startups and the presentation offers a useful way of framing how we prioritise innovations. Will commented that the most successful corporate venture capitalist is Intel. They are very disciplined about getting pilots out of their relationships with startups. It is actually a legal form of insider trading.

- The idea of making beliefs explicit is a very valuable one for the general practice of strategy. The experimentation approach outlined is very similar to lean.
- We have just put some money into a food technology fund. There is currently a lot of VC money going in to synthetic biology. There is a safety problem but AI should make some exciting things possible in 3-5 years.
- The problem we have with running pilots in order to learn is that a lot of people oppose them because they think they are wrong.

Future meetings

The next Members' Meeting will be held from **13.00 – 17.30 on 28th November 2018.** It will take place at the same venue, The Royal Horseguards Hotel, 2 Whitehall Court, Whitehall, London SW1A 2EJ.

The topic will be 'Corporate Stumbles' and will be led by Jo Whitehead.

Members are also reminded about our next Seminar to be held at De Vere Venues on 22nd November, when the subject will be Digital Strategy and will be led by Felix Barber.

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[i] See HBR: The Discipline of Business Experimentation. Harvard Business Review December 2014

[ii] The 50% figure is incredibly generous. In Online Experimentation at Microsoft, the team estimates that only 33% of experiments improve the metrics they were intending to change.