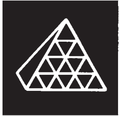

MAKING STRATEGIC INNOVATION WORK: AN INTERVIEW WITH VIJAY GOVINDARAJAN

Vijay Govindarajan talks with RTM Editor-in-Chief James Euchner about how to put his 10 rules for strategic innovators into practice.

Vijay Govindarajan with James Euchner

In *10 Rules for Strategic Innovators*, Vijay Govindarajan and co-author Chris Trimble discuss what it takes to innovate inside established companies. He emphasizes three concepts: *forgetting* lessons from the past that may inhibit progress on a new venture; focusing on *learning* and clarifying key assumptions in the early stages of innovation, not on financial metrics; and consciously *borrowing* appropriate assets from the parent organization. These are simple in principle, but they can be difficult to put into practice. In this interview, conducted as part of the 2009 World Innovation Forum, held in New York City in May, we probe the practical implications of these ideas for innovators seeking to innovate within larger organizations.

James Euchner [JE]: Vijay, it's great to have the chance to talk with you about innovation. I really enjoyed reading *Rules for Strategic Innovators*.

I'd like to structure the interview around the three principles you discuss in your book, forgetting, learning, and borrowing. Let's start with the difficulties many companies have forgetting lessons that may no longer be relevant. In the book, you suggest hiring people from the outside to bring a fresh perspective. You also suggest starting internal ventures that are quasi–independent from the core business. But you can't really do that in every single case. In your experience, how different does a new venture need to be before it makes sense to set it up with a separate infrastructure?

Vijay Govindarajan [VG]: I think probably the criteria to use is whether the innovation is breaking away from your current business model. You need to really think critically about the business model.

A business model answers three questions: Who's your customer? What value is the customer seeking? And what is the process by which you're going to create that value? For your core business you have evolved an answer to these three questions.

If you are launching a new venture, and the innovation breaks away from your core business in its answer to any one of these three questions, then I'd say that you have to overcome the forgetting challenge. And one of the most effective ways to do that is to set up a separate venture.

Vijay Govindarajan is currently the Earl C. Daum 1924 Professor of International Business and the founding director of the Center for Global Leadership at the Tuck School of Business at Dartmouth College. Widely regarded as one of the world's leading experts in strategy and innovation, Govindarajan has received numerous awards for excellence in research and been named to, among others, BusinessWeek's list of the top ten business school professors and the London Times's list of the top 50 management thinkers. He has published more than 70 articles and seven books, including Ten Rules for Strategic Innovators *(Harvard Business Press, 2005) and* The Other Side of Innovation: Solving the Execution Challenge *(Harvard Business Press, forthcoming in 2010), both co-authored with Chris Trimble.* **www.vg-tuck.com; vijay.govindarajan@dartmouth.edu**

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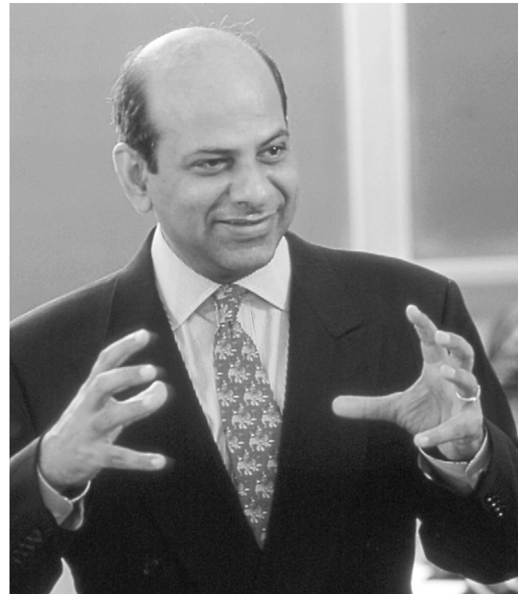

Vijay Govindarajan works with Global Fortune 500 CEOs and management teams to broaden their thinking about innovation and strategy.

JE: Won't that end up being a large number of the things that are going to be truly innovative inside of a corporation?

VG: No company can launch hundreds of these new ventures. Therefore, each company needs to understand its capacity for new ventures. Only a few such ventures can be launched, and when you launch those, you have to get it right. You may need very different processes for HR, for instance, to support the different business model.

As an example, when General Motors created OnStar, which was a fundamentally different business model than making automobiles, they used the same HR process to recruit for it as they used for their core business. Imagine, at the height of the dot-com boom, trying to persuade people from Silicon Valley to move to Detroit on the same pay scale on which General Motors's regular employees were paid.

So you get the picture. A company pursuing a new business may need to think about human resources, for instance, very differently.

JE: Now in that situation did they actually set it up as a quasi-independent unit in order to move it forward?

VG: Initially they did not, and that's why the unit started to struggle—because they had common HR systems, even a common information technology platform with the core business. Again, imagine for a moment: the information technology that supports the automobile business is oriented towards processing efficiency, whereas information technology was the brain of the OnStar program. If you have the same IT department trying to attend to the needs of OnStar and the needs of the automobile business, the priorities will become skewed, and even the capabilities required may not match.

OnStar started to succeed when they started to isolate it.

JE: I'd like to pursue a little further the question of the capacity for innovation, or how many of these you can do at one time. It seems like each one takes a lot of managerial attention. How do you pick out the ones you ought to pursue? Is there some sort of feeder process? Do you start these ventures inside your existing structures and then reach a point at which you decide that it's time to move them out?

VG: How do you move from a large set of ideas to a few that you experiment with, and how do you select the ones to scale up? I would say the following: most companies do it wrong because they only focus on the financial attractiveness of an idea. Financial attractiveness is fine, but I would ask another set of questions. For each of the ideas that you have, try to understand the assumptions you are making in order for this to be an attractive idea. Any kind of financial justification you can make for these ideas is all guesswork anyway, because there are so many unknowns. So instead, ask the question, what are the assumptions we are making for this idea to be a very profitable idea?

Take the assumptions that you are making and classify those assumptions into three groups. First are the show-stopper assumptions; if these go wrong, the game is over. The second set of assumptions is those that require you to fundamentally rethink your strategy. And the third group of assumptions is ones you can tweak as you implement.

So take your assumptions—typically in these kinds of business model changes, the number of assumptions starts out to be about 100. Take the 100 assumptions and put them into these three buckets: showstopper assumptions, assumptions that require you to rethink your strategy, and tweaking assumptions.

If you have more than three assumptions that are show-stoppers, you should not do the project. There have got to be very, very few showstopper assumptions. Similarly, there should be no more than three assumptions that require you to fundamentally rethink your strategy. Too often, companies jump into a project because the financial attractiveness is high. What they miss is that there are 50 assumptions in the showstopper and fundamental strategy categories.

JE: Do you need to do some sort of experimentation to get an understanding of the critical assumptions? Often, I find that ideas are not fully formed, and those critical issues do not jump out at you. You have to do some work to get to them.

VG: You may be right. Sometimes you may not be able to do this exercise sitting in your office. You may need to spend a little bit of money to understand what the critical assumptions are. In the experimental stage, you try to spend a little in order to understand what the assumptions are.

Then you can be really serious about scaling up. Once you reach the scaling stage, you have to be more informed about the assumptions you are making.

JE: So give me an idea; when you're in this experimental stage, as opposed to the scale-up stage, what kind of resources should be applied, and for how long? Are you talking about projects that go on for three months or six months, with a full, dedicated team?

VG: The experimental stage is one place where the learning challenge comes in. The reason you're experimenting is that you have a lot of unknowns. You want to get at least some firm understanding of what those unknowns are before you scale up and spend large amounts of resources. My golden rule is "spend a little, learn a lot." Because there are so many assumptions, you may be tempted to spend a lot. But in the experimental stage, what you're trying to do is to focus on your critical assumptions: those in the showstopper category and those in the category that requires you to fundamentally rethink your business strategy.

The most important showstopper assumptions turn out to be not in technology but in the market.

There can't be more than a half dozen to a dozen of those assumptions, or you could be testing and validating them for multiple years. Find inexpensive ways by which you can gain some understanding about the key assumptions and put a real tight timeframe around the learning; I would say that you should spend no more than three months at this stage. Or at least have a conversation every three months to ask, "Do we know anything more about this assumption now?"

JE: Can you give me a couple of examples of what you see as showstopper assumptions that companies have to confront?

VG: The most important showstopper assumptions turn out to be not in technology but in the market, because in these kinds of new ventures, you're trying to *create* a market. When you're trying to create a market, you don't know the size of the market; you don't know at what price point the market will unlock; you don't know whether there will be competitors and competitive reaction.

Take for instance the *New York Times* and their movement into the dot-com world in the mid-1990s. Now when you went into a dot-com business in the mid 90s, you didn't know what the size of the market was in the digital space. You didn't know that there was a company called Google coming. You didn't know a company called Yahoo was being born. Your traditional competitors, you understood. The *New York Times* was a printed newspaper, and it maybe saw the *New York Post* [as its competitor], but on the web, competitors were not even born yet. So the key assumptions turn out to relate to the market. Less on technology, more on the market.

JE: It seems like those were killer assumptions for the *New York Times*, right? Those are showstopper

assumptions. Doesn't that sort of stop you from moving forward in new markets, even if it's in your best interest to do so?

VG: The showstopper assumption doesn't stop you from moving. If there are 20 showstoppers, that's not a project you want to touch. At the *New York Times,* they were experimenting because the size of the market was not known. They saw a potential depending on how fast the Internet penetrated. They asked, Who will be the leader? Who will come into the business? What will be the habits of leading users? And what kinds of features will they like? You can do a mini-experiment, particularly in a case like this, and immediately get some feedback about whether the customer likes this or likes that.

Even price was unclear, though, because in the printed newspaper, the revenue came from circulation and advertising. But in the online media, it wasn't clear whether they were going to monetize the content, or monetize the advertising, or what the model was going to be.

JE: So they had at least two killer assumptions? They didn't know the business model, and they didn't know whether the market would develop. But I take it that you are saying that experimentation was still within the bounds of being worthwhile because the opportunity was so strategic for them.

I want to turn a little bit to what you call the borrowing side of the equation. One of the things that we do at Pitney Bowes a lot is customer-centered innovation. We identify a strategic area we'd like to enter where we think there's value for the company. We use tools borrowed from anthropology to really understand the world of the customers and where they want to go. One of the challenges we face is that, sometimes, customers want to go where it's very hard for the company to go. We may not have a channel. It may require a very different business model. We may see a really compelling value proposition, but find it very difficult to go there.

So my borrowing question is this: what is the *minimum* you need to be able to borrow from the host company to make a venture worth pursuing internally? I'm talking about using the assets of your company to create competitive advantage for the new venture. What's the minimum that you need to make it worth going into a space that the customer may want to go, but where it's uncomfortable for you to go as a company?

VG: That's a great question. We are not suggesting moving into areas where you cannot leverage any of your critical assets. In that case, you would have no competitive advantage and the venture would be a pure startup. Pure startups, in Silicon Valley or somewhere else, have a big advantage in that they're not burdened by bureaucracy. They are nimble; they are fast; they can move. But the biggest advantage of a large company like Pitney Bowes is its large resources, the established customer relationships, the core competencies. The only way the Pitney Boweses of the world can win is by entering new spaces where they can leverage their capabilities.

The projects with the most chance of success are adjacency-oriented new business model innovation, taking your current core competencies to a new customer.

I would say that the projects with the most chance of success are what I would call adjacency-oriented new business model innovation. By adjacency, what I mean is adjacent to your core business. So, you are taking your current core competencies to a new customer. Or taking your core competencies and satisfying a need of your current customer better. Or taking your current competencies and pushing out into an adjacent space. Adjacency-oriented new business model innovation will utilize probably seventy-five percent of your core competencies.

The next step of innovation is what I call step-out. Step-out is not adjacency: you're stepping out of your core. In those instances, I would say that you've got to use forty percent of your core competency [to be successful].

JE: And when you say forty percent, you mean forty percent of what? How do you quantify that?

VG: It's not so much a mathematical calculation. Just ask yourself what core competencies are needed to win.

Core competencies, not all resources; just core competencies. Out of those core competencies, if you have fifty percent of them inside, I think that's a good step-out opportunity. If out of those core competencies you have nothing, I'd say that probably that's not where you want to step out.

JE: And how do you evaluate your core competencies? Because it's very easy to fool yourself into thinking you have something special when you might not. How do you evaluate whether a given asset is really giving you a unique advantage?

VG: You learn during the assumptions testing phase, because in the assumptions testing phase, you are not only testing external market assumptions, you're also testing capability assumptions. You're trying to understand what it takes to win. And you learn that through some experimentation. Because you're right: people always kid themselves into thinking that they have a capability that they can leverage, and when they actually get in the market they find that the capability is of no use. So expending a little bit of money [to learn about your own capabilities] is a good idea. This goes back to the example of OnStar that I mentioned earlier. They thought that the IT capability they had developed in the automobile business could be immediately leveraged into OnStar. In reality, the IT experts that they had were good in business process efficiency, but they weren't very good at information products.

JE: I see. That's helpful.

In many of the examples in your book, there was what I would call a reset or a restart event. The venture started somewhere, it was proceeding along a path, and then management had both the patience and the insight to say, "Wait a second; this is worth doing, but we're not doing it right." And they reset. In some instances, they made the new venture more connected to the core business, and in other instances they made it more independent. Is that inevitable? Is finding the balance between forgetting and borrowing something you can't think your way through [from the outset]?

VG: It's a very fascinating question. I would say that you have to allow for evolution, because it is very, very difficult to get the formula right at the start. Having said that, I don't think that you need to stumble your way as much as they did in some of the examples in our book. We were studying those examples as historical lessons and we were presenting a framework. Using the framework, you can increase your chances of getting it right on day one. It's better to get it right on day one, because if you overplay the forgetting challenge or underplay the borrowing challenge, you isolate and reduce the chances of success. The core business can sometimes squash such a venture.

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On the other hand, if you keep the venture too close in order to maximize borrowing, then it may not have the ability to move to a new business model. The needs to forget and to borrow can pull you in different directions. It is possible to strike the right balance on day one, but keep an open mind because it may need to evolve.

JE: I think your book is very clear on what these assumptions are, and yet I feel like some of them need to be made at gut level. An executive may not really understand the underlying assumptions in a new area, especially since these executives have a lot going on and the new effort might conflict with some of their existing plans. It seems that it could be very hard for executives to make these gut calls without an experiential base. When you work with executives, how do you get them to accept the reality of the need for either the separate entity or better borrowing? How do you get them to feel it in their gut?

VG: If you're serious about spending money to innovate a new business model, then get serious about the organizational question, because if you don't get that right, the opportunity is never going to materialize. When I'm asked for advice, I look into the CEO's eyes and say, "If you are really serious about innovation, you've got to make some tough, important decisions up front."

Now, our approach is not a cookbook. The ten rules in our book are helpful in thinking things through, and when you do and you try things, something new will fall out. That just increases the art. If you think about these questions, you are likely to increase the odds of success. But keep an open mind, because this is still an art: you will probably stumble at some point along the way, but now at least you have a framework.

JE: There's a lot of learning that has to happen, not just inside the unit but in the executive team as well. One of

your suggestions is that you use the quality of predictions as a metric for evaluating these types of initiatives, but it's only a metric. What does a typical operating review look like for one of these Newcos, as you call them?

VG: For these Newcos, the operating review has to focus on the assumptions, because when you prepare your financial projections for a new venture, it's guesswork. So why are you even preparing the business plan, knowing that it is likely to be wrong? The reason you are preparing the plan is not because the numbers are what you believe in, but because they enable you to focus on the key assumptions behind the numbers. In the operating review, I want to focus on the ten assumptions that you identified up front, the critical assumptions. Use the operating review to tell me the experiments that you have done. Tell me what you have learned and how you are revising your plan as a result. Preparing a plan is important for clarifying the assumptions. Then you revise the plan based on what you have learned about those assumptions. That's what I'm going to judge Newco on.

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JE: Thanks very much. It has been helpful to dig deeply into how you make your model work in practice. As you say, true innovation is still an art. Your thoughts are helpful for people at all levels in learning to practice the art.

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