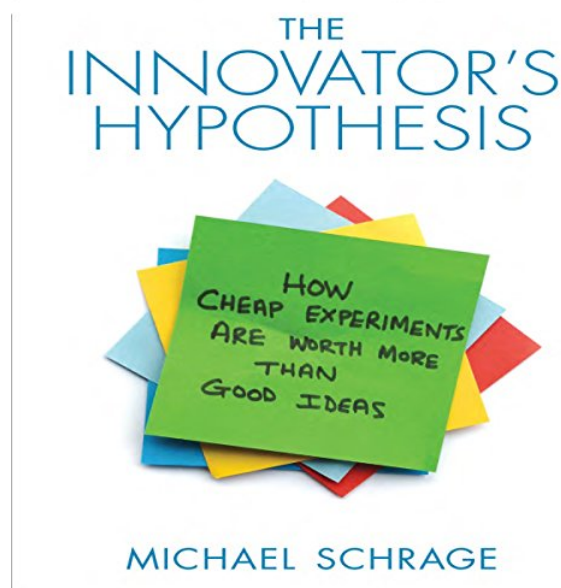


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# **THE INNOVATOR'S HYPOTHESIS: HOW CHEAP EXPERIMENTS ARE WORTH MORE THAN GOOD IDEAS BY MICHAEL SCHRAGE PDF**

What is the best way for a company to innovate? That's exactly the wrong question. The better question: How can organizations get the maximum possible value from their innovation investments? Advice recommending "innovation vacations" and the luxury of failure may be wonderful for organizations with time to spend and money to waste. But this audiobook addresses the innovation priorities of companies that live in the real world of limits. They want fast, frugal, and high-impact innovations. They don't just seek superior innovation; they want superior innovators.

In *The Innovator's Hypothesis*, innovation expert Michael Schrage advocates a cultural and strategic shift: small teams collaboratively - and competitively - crafting business experiments that make top management sit up and take notice. Creativity within constraints - clear deadlines and clear deliverables - is what serious innovation cultures do. Schrage introduces the 5X5 framework: giving diverse teams of five people up to five days to come up with portfolios of five business experiments costing no more than \$5,000 each and taking no longer than five weeks to run.

The book describes multiple portfolios of 5X5 experiments drawn from Schrage's advisory work and innovation workshops worldwide. These include financial service approaches for improving customer service and addressing security challenges; a pharmaceutical company's hypotheses for boosting regulatory compliance; and a diaper division's efforts to give babies and parents alike better "diapering experiences" with glow-in-the-dark adhesives, diagnostic capability, and bundled wipes.

Schrage's 5X5 is enterprise innovation gone viral: Successful 5X5s make people more effective innovators, and more effective innovators mean more effective innovations.

- Sales Rank: #83159 in Audible
- Published on: 2015-10-09
- Format: Unabridged
- Original language: English
- Running time: 418 minutes

Most helpful customer reviews

45 of 46 people found the following review helpful.

Innovate quickly with Cheap Experiments, but take the proposed "solution" with a grain of salt (has not been experimented with)

By Ron Kohavi

My team provides the experimentation platform for most online experiments at Microsoft (e.g., search for exp-platform), and I have been running online experiments for 12 years now, with Bing running about 10,000 experiments per year, so I know something about online controlled experiments. Michael Schrage

quotes me a few times in the book, and encouraged me to write an unbiased review.

The book is written extremely well, and the overall message is absolutely spot on: most organizations would benefit from running cheap experiments and iterating quickly. In many cases, organizations get into analysis-paralysis instead of running simple experiments; worse, deep analyses often come late and have low predictive accuracy.

The 5x5 approach advocated by Michael Schrage says "Give a diverse team of 5 people no more than 5 days to come up with a portfolio of 5 business experiments that cost no more than \$5,000 (each) and take no longer than 5 weeks to run."

My key observation is that the author proposes a solution with a plan to a problem, and violates his own advice to avoid such solutions. He's asking the reader to believe in exactly what he's asking others to suspend doing. There are many possible ways to get an organization to be data-driven. Is 5x5 better than others? Having been humbled by many good ideas that don't perform as well as people hope, I'm not sure there's data to say that the 5x5 proposal is dominating.

In the paper "Online Controlled Experiments at Large Scale" there's a section on the "Cost vs. Benefit and the Ideas Funnel" where prioritization of ideas is discussed, a mechanism that aligns more with the author's statement that "Experiments need to be seen and managed as investments" and less with the 5x5 approach.

Here are the key themes that I agree with

- Be "lean" and "agile" and run experiments quickly instead of analyzing and debating. Minimum Viable Products are a key theme

Mike Moran calls it Do It Wrong Quickly

- Reduce the cost of running experiments
- Experimentation is as much a cultural problem as a technical one.

A platform/system can solve the technical challenge, but it's harder to address the cultural issues

- Try to craft a hypothesis that the experiment answers, from which you can learn, whether the experiment is successful (e.g., rejecting the null) or not. If you change too many things, you learn little when the result comes out "flat."

A couple of themes I disagree with

- Experimentation is not a panacea for every problem and every organization.

There is little discussion in the book about when NOT to experiment.

Netflix and Amazon can make mistakes in recommendations, but you don't want many of those in Nuclear Submarines.

Knight Capital was agile and proud of their experiments, until in Aug 2012 they had an error that caused a \$440 million loss and erased 75% of Knight's equity value.

- The section about "Good ideas" didn't work for me. I equate a good idea with a hypothesis that is true, i.e., that we validate in an

experiment. The author writes "The Earth is not flat; the moon isn't made of green cheese; and the future of business innovation

doesn't depend on good ideas. It never has....Good ideas are usually bad investments. They're rarely worth the time, money, or effort. They overpromise and underdeliver. They seduce and they cheat....Good ideas might be better described as the empty calories of enterprise innovation: accessible, tasty, and momentarily satisfying. But they're not good for you. They'll make you sick."

I believe this is a terminology issue, but I would have used "flash-in-the-pan idea" that upon evaluation is not really good.

Overall, this is a great book to read and buy for your managers, if they need to be better motivated on how to innovate better and faster with experiments.

16 of 17 people found the following review helpful.

5x5x100

By Abacus

I am sure the author is a very effective consultant. He knows his stuff. He has a lot of experience. But, this narrow subject just does not warrant a whole book. A 10 page essay would have more than sufficed. The writing is so repetitive because the author struggles to spread 10 pages worth of content over 200 pages of print. It just does not work. The title of my review indicates the author repeated his 5x5 concept a hundred times throughout the book. And, that's probably a lowball estimate. But, the book has other weaknesses.

There is nothing magic about the number 5. Although 5x5etc... has a good ring to it, there is an unlimited way of gathering resources to conduct short-term, rapid feedback experiments. You can indeed try 5 different concepts with teams of 5 people each costing no more than \$5,000, over a 5-week period. But, many innovations have been done with a single person, trying a single concept, costing close to nothing, with near immediate feedback in this Internet age. The single innovator may succeed not by trying very different concepts but by iteratively honing and tweaking his original concept and watch live the performance of his last iteration. That's a similar approach the author indicates that Edison, Jobs, Amazon, and other innovators have used. So, the author clearly understands this. But, it does contradict his 5x5 dogma.

Regarding designing rapid low cost experiments, there may be some avenues the author did not explore. Within the book, I did not see any mention of internal prediction markets that are used by many hi-tech companies to quickly differentiate what projects are successful. Also, I did not see any material on clothing companies like H&M that have specialized in doing short-runs of newly designed clothes and measure their sales performance nearly live and rapidly scale up production of the winners and shut down the losers.

Finally, his whole chapter on good ideas are actually bad titled "Ideas are the Enemy" is really bad. It goes without saying that whether an idea is good or bad can be assessed only after it has been tested. Nevertheless, you better give some thought to what idea/hypothesis you want to test to begin with. Just randomly, testing 5x5 a bunch of concepts you have not given serious thoughts to it is a waste of time.

Within that same chapter, his criticism of Keynes' supposedly vapid intellectual elitism is either misinterpreting what Keynes meant or misplaced. In this post-crisis era, economists make for good punching bags; and, often for good reasons. But, the author's punches for the most part missed the punching bag. He could have just as easily lauded Keynes who was among the first ones to advance that most often we really can't forecast (Keynes' uncertainty concept). Societies, economies are very complex systems with unanticipated interactive feedback loops. As such, Keynes provided the intellectual foundation for the author that given that we can't forecast, we better instead experiment to find out what actually works.

If you are interested in innovation and related developments, I have come across two books that do cover what the author does and a lot more. You may find those books interesting. The first one is Duncan Watts Everything Is Obvious: How Common Sense Fails Us and the second one is Smartcuts: How Hackers, Innovators, and Icons Accelerate Success by Shane Snow.

10 of 11 people found the following review helpful.

How and why "simple, fast, cheap, smart, lean, and important experiments can supercharge any serious innovation process"

By Robert Morris

As I began to read Michael Schrage's latest book, I was again reminded of a passage in Paul Schoemaker's

latest book, *Brilliant Mistakes*: "The key question companies need to address is not *'Should we make mistakes?'* but rather *'Which mistakes should we make in order to test our deeply held assumptions?'*" This is precisely what Anjali Sastry and Kara Penn have in mind when introducing, in *Fail Better*, what they characterize as a better approach to innovation: designing smart mistakes, learn from them, and thereby achieve greater success and do so sooner.

Peter Sims has much of value to say about this strategy in *Little Bets: How Breakthrough Ideas Emerge from Small Discoveries*. As he explains, "At the core of this experimental approach, little bets are concrete actions taken to discover, test, and develop ideas that are achievable and affordable. They begin as creative possibilities that get iterated and refined over time, and they are particularly valuable when trying to navigate amid uncertainty, create something new, or attend to open-ended problems."

In *Serious Play* (1999), Schrage introduces several core concepts that he develops in much greater depth in this, his latest book. The 5x5 X(experimental)-team approach is a rapid innovation methodology emphasizing lightweight, high-impact experimentation, as follows: "Give a diverse team of 5 people no more than 5 days to come up with a portfolio of 5 business experiments that cost no more than \$5,000 (each) and take no longer than 5 weeks to run." Schrage adds, "the 5x5 seeks an 80/20/20 vision. That is, what hypothesis could we test -- what experiments could we run -- that generate 80 percent of the useful information that we need to make a decision in 20 percent of the time, and with but 20 percent of the resources that we ordinarily use to do so?" Schrage thoroughly explains the theory and practice of this method in the book. Yogi Berra once observed, "In theory there is no difference between theory and practice. In practice there is." Presumably Schrage agrees.

These are among the dozens of passages of greatest interest and value to me, listed also to suggest the scope of Schrage's coverage:

- o The Business Experiment: A fast, inexpensive, and informative test of a business hypothesis (Pages 10 & 12)
- o Implementation and prototypes (24-33)
- o Experimentation and innovation (36-38 & 85-87)
- o Warren Buffett (49-56)
- o Resistance to business experiments (67-68, 85-88, & 178-179)
- o Blockbuster (77-85)
- o Julian Simon (89-92)
- o 5x5 X-team approach (95-138)
- o Business experiments (141-153)
- o Charles Kettering (146-148)
- o Business hypotheses (177-185)
- o David Kelley (210-211)
- o Decision theory (213-216)

I commend Schrage on his innovative use of (boxed) mini-commentaries, inserted throughout his narrative that include "The 5x5" (Page 5), "Formulating the Hypothesis" (11), "Moving from Hypothesis to Experiment" (12-13), "A Prototype Is a Hypothesis" (30-32), "Scott Cook, Founder and Chairman of Intuit, on Experimentation" (57-58), and "Q&A: The Experimenter [Gary Loveman]" (205-206). This material complements rather than supplements the information, insights, and counsel he provides elsewhere in the narrative. The same can be said of passages that examine experiments by real people in real-world situations, experiments that produced mixed results to be sure but demonstrate the truth of Thomas Edison's claim, "I have not failed. I've just found 10,000 ways that won't work." The companies that have --or should have --

followed the 5x5 X-team approach include (in alpha order) Amazon, Apple, Blockbuster, Delco, Facebook, GE, General Motors, Google, NCR, P&G, Tesco, Toyota, and Walmart.

John Kotter once suggested that the most difficult challenge to change initiatives is to change how people think about change. The same can be said about innovation and experimentation, as Schrage suggests in the book's final chapter, "Experimenting with Experimentation." He quotes Kevin Kelly: "Anybody who works in science knows that they're constantly finding new things that they don't know. It increases their ignorance...while science is certainly increasing knowledge, it's actually increasing our ignorance even faster. So you could say that the chief effect of science is the expansion of ignorance." You could also say, at least I do, that rather than increasing ignorance, science increases our knowledge of what we don't know. Some of the worst decisions people make are based on insufficient or inaccurate information. Hence the critical importance of testing all assumptions and premises. Paradoxically, the most innovative and creative thinking requires the multiple disciplines of the scientific method.

Here's Schrage's response to Kelly's comments: "Substitute business for science and opportunity for ignorance, and Kelly's tongue-in-check observation explains why the ongoing revolution in experimentation defines the innovation future. There's never been a better time for innovating with experiments or experimenting with innovation."

Obviously the "Whack-a-mole" approach to innovation makes no sense nor does a concentration of massive resources on one major experiment. Breakthroughs are few and far between lots of small, low-risk, prudent, and numerous experiments.

Invoking a set of horticulture metaphors, here's my take on Michael Schrage's approach: innovation requires careful planting of seedlings (i.e. ideas, possibilities, hypotheses) in properly prepared soil. Cultivate them and protect them for a while, then nourish those that begin to indicate promising growth. A few will. Perhaps combine a few of the others and keep an eye on them. Eventually, hopefully, there will a few robust results.

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