

### Free Book Excerpt

CYCLES: The simplest, proven method to innovate faster while reducing risks  
**A work from Bryan Cassady and 22 Innovation Experts**

## CHAPTER 20

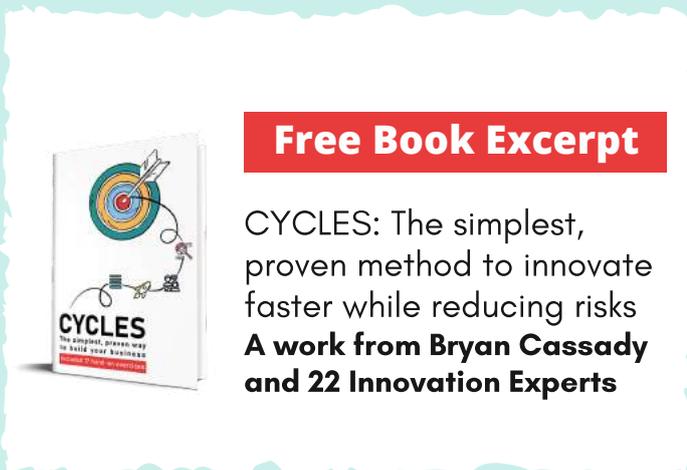
# SHOOT YOUR PUPPIES AND MOVE ON: WHY KILLING IDEAS IS AS IMPORTANT AS BUILDING NEW IDEAS AND HOW TO DO IT

“What you choose to do is important, but sometimes, what you choose not to do is just as significant.”

## REINHARD EMATINGER

CO-AUTHOR OF THE BOOK CYCLES





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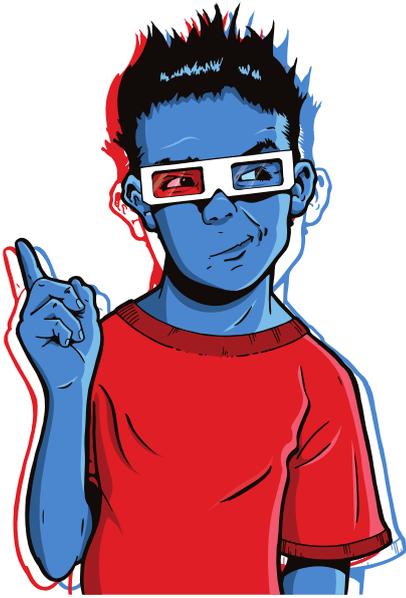
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“What you choose to do is important, but sometimes, what you choose not to do is just as significant.”

**URS ROTHMAYR**

CO-AUTHOR OF THE BOOK CYCLES





## WHAT YOU WILL FIND IN THE CHAPTER

### KEY TAKE-AWAYS

- **Quitting is not failing** – smart quitting is an essential business strategy that leads to improved focus.
- **Knowing when to quit is as important as being willing to persevere** – resources are finite. The only way to succeed is by focusing what you have on what will deliver the best results. That means killing projects that will not deliver or will only deliver something good enough.
- **Quit for the right reasons** – quitting because you are in the dip, tired, stressed, or plain nervous is a bad idea. Quitting because you realize that a project is not going anywhere or will not lead to an exceptional product is an excellent idea.
- **Know your biases** – emotional investment leads to cognitive dissonance and confirmation bias. Do not let these cloud decisions about whether projects should continue.
- **Be systematic** – build regular kill points into project planning and make real kills when necessary.
- **Really kill** – do not hide behind deferment or giving a project a lower priority. Give each project 100% commitment or kill it.

## YOU WILL ALSO GET A LINK TO SOME USEFUL CANVASES

The 4 P's Canvas To Make Your Ideas Clear		Created for:	Created by:	Date:
<b>Name</b> For simplicity, choose a descriptive name				
<b>Headline</b> In one sentence, what's that big idea?				
<b>Problem</b> What is the problem and for whom?				
<b>Promise</b> Benefit/promise				
<b>Proof</b> Reason why they should believe you and dramatic difference?				
<b>Payoff</b> Dramatic difference: how is their life different and better?				

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CYCLES  
www.thecyclebook.com

# CHAPTER FROM THE BOOK CYCLES



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- Chapter 3: The Power of Theory: If You Want Your Innovation to Fly, Theory Is the Wind Beneath Your Wings

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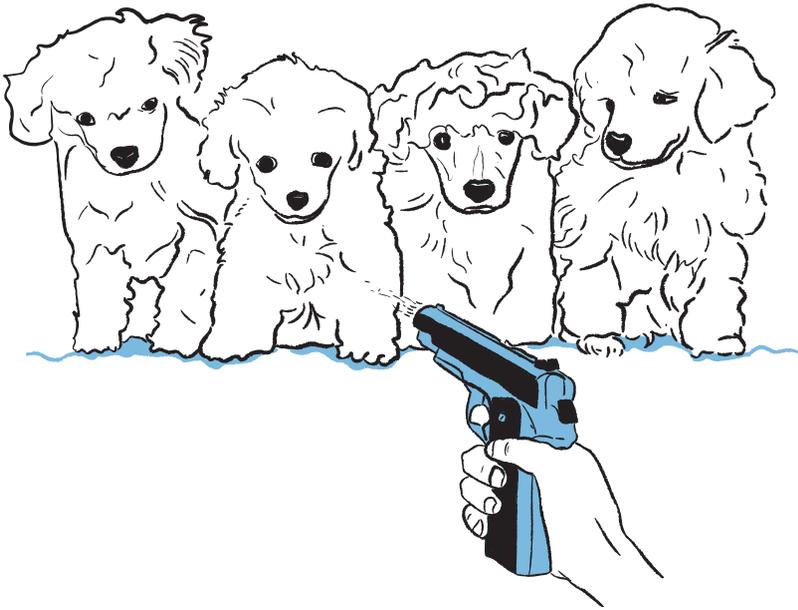
## PART 5: LEARNING SYSTEMS

- Chapter 22: Systems Introduction
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## CHAPTER 20: SHOOT YOUR PUPPIES AND MOVE ON: WHY KILLING IDEAS IS AS IMPORTANT AS BUILDING NEW IDEAS AND HOW TO DO IT



**What you choose to do is important,  
but sometimes, what you choose *not*  
to do is just as significant.**

### **Are You Here for the Shoot the Puppy Meeting?**

It was -40°C (-40°F), and I was about to learn one of the hottest techniques in project management...

As I walked into the Canada Post offices, I was greeted by the board's executive secretary, who politely asked if I knew that this was a "*shoot the puppy*" meeting?

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This chapter was co-written with  
Urs Rothmayer and Reinhard Ematinger.

For three years, I had been traveling around the world, working with different national postal services. My mission was to help them become more market-driven. The process was almost always the same. Look into the organization for a few good ideas (or bring some new ones), present them to the board, then get some feedback. From there, I let the local team work on bringing things to market. Up until now, all the projects I had proposed had gone forward. Sure, there was sometimes negative feedback, but it was always accompanied by a pat on the back.

Before I went into the meeting, I had to ask the secretary, “*What is a shoot the puppy meeting?*” The answer was so important that I still remember it clearly more than twenty years later. The executive board was responsible for making decisions about whether projects went forward and had a reputation for a willingness to kill those they were not 100% certain about. Killing projects is a tough thing to do, and it is rarer than you might think. People work hard and commit to projects, and it is easier just to let them continue even when the prospects for success are small. In a big organization like Canada Post, this can lead to thousands of “*walking dead*” projects, all using resources that could be better deployed elsewhere.

This board was different. They made real decisions about which projects should continue and which should be stopped. They called this process “*shooting puppies*” because they recognized that it is hard for people to kill their darlings. No-one likes to get rid of an ugly puppy, but sometimes it needs to be done.

At the meeting, I was asked five questions about the two projects I presented:

1. Is there a real benefit?
2. Are people willing to pay? (and will they pay us?)
3. Is it interesting enough that people will pay attention?
4. Does someone hate it?
5. Is there a reason why we can do this better than anyone else?

All the questions made sense to me except #4. The board explained that if everyone likes something, it is usually not a great idea. It is like vanilla ice cream – inoffensive but unlikely to inspire strong positive or negative emotions. A “yes” on all the other points could lead to a go decision, but the board believed that strong emotions around an idea are always a good sign.

I presented project #1; yes, no, no, yes, no. Shoot that puppy!

I presented project #2; yes, yes, yes, yes (two people really hated it) and a final “*probably*”. They gave me four weeks to turn that “*probably*” into a “*yes*”.

I left the meeting feeling frustrated about project #1, but I now knew where I needed to focus. I did not realize it at the time, but I had just learned one of the most important things that separates winning innovators from the rest. Meetings like this are rare. It is common (and much less painful) to just let projects keep rolling. It takes boldness and discipline to decide to kill a project. But turning great ideas into successful products takes time and effort, and innovation winners learn to focus all their resources on the best ideas.

A bad idea, even when well-executed, is still a bad idea. Ultimately, it is a waste of time and energy that deflects resources from where they could be better utilized. This chapter will tell you how to identify the ideas you need to kill by following a simple process, and it will explain when and how they should be killed.

## Steve Jobs – Puppy Shooter.

The three most common mistakes that entrepreneurs make are:

1. Investing in the wrong business.
2. Failing to let customers understand a product or service.
3. Staying with the wrong idea for too long.

Of these, number three is the most common and the most expensive. We all fall in love with our ideas, and it is hard to give up on dreams. But sometimes, quitting is the most effective solution.

**“The most important thing winning companies do better is that they kill bad ideas faster.”**

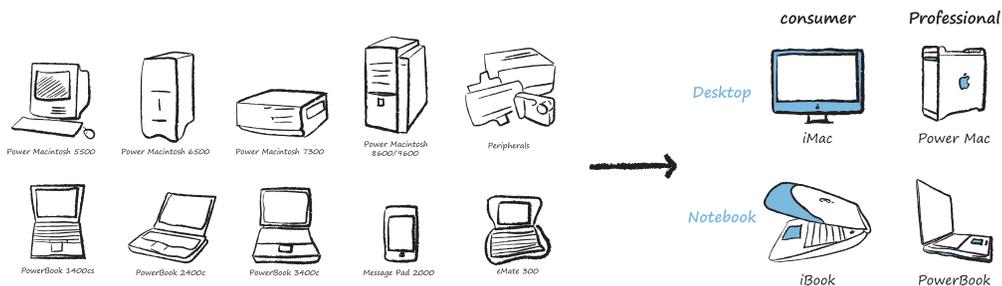
ROBERT G. COOPER

THE PDMA HANDBOOK OF NEW PRODUCT DEVELOPMENT

It is important to note that quitting is not the same as failing. It is a business strategy where you switch focus away from something that is not productive to something that is. No organization has unlimited resources, and it is essential to assign time and effort to projects that will provide worthwhile returns. If you try to focus on everything, you will achieve nothing. Steve Jobs is often held up as an exemplar of creativity and innovation, but one of the most important things he did was to kill projects.

In September 1997, Jobs returned to Apple as CEO, almost precisely twelve years after leaving the company. Apple was not in a good place. It offered an array of different Macintosh desktops and laptops and peripherals, including printers and digital cameras. Few lines were making a profit, and the business was losing tens of millions of dollars every quarter.

After only a few weeks, Jobs called a meeting. He drew a simple two-by-two grid on a whiteboard. He labeled the rows “*desktop*” and “*portable*” and the columns “*consumer*” and “*professional*”. Jobs told the stunned meeting that their task was to select one Apple product for each of the four squares on the grid. Everything else was to be canceled. Jobs went on to kill more than 70% of Apple’s existing hardware and software range. One of the things he canceled was the Apple Newton MessagePad, a product in which many people at Apple were deeply invested, though sales were not great. Talking about the Newton, Jobs later said: “*My gut was that there was some really good technology, but it was fucked-up by mismanagement. By shutting it down, I freed up some good engineers who could work on new mobile devices.*”



The only way to do this was to cull their bulging product line. One of the first things Steve Jobs did was kill dozens of products, including the much-publicized Newton. Apple went from over 20 key products to 4. (A notebook for professionals and consumers and a desktop for professionals and consumers.)

These radical changes moved the company from a \$1 billion loss in 1997 to a \$300 million profit in 1998. Maybe even more importantly, they freed up time for engineers to work on other products like the iPod, iPad, and the iPhone, which launched several years later. They would have never had the cash or the people if they hadn't started by killing products. Quite simply, Apple wouldn't be the company they are today without the kills in the past.

Innovation is often about making bold decisions. Sometimes, the most important decisions involve recognizing the projects that do not have the potential to deliver exceptional and profitable products and shooting those puppies.

## Why You Need to Kill Projects.

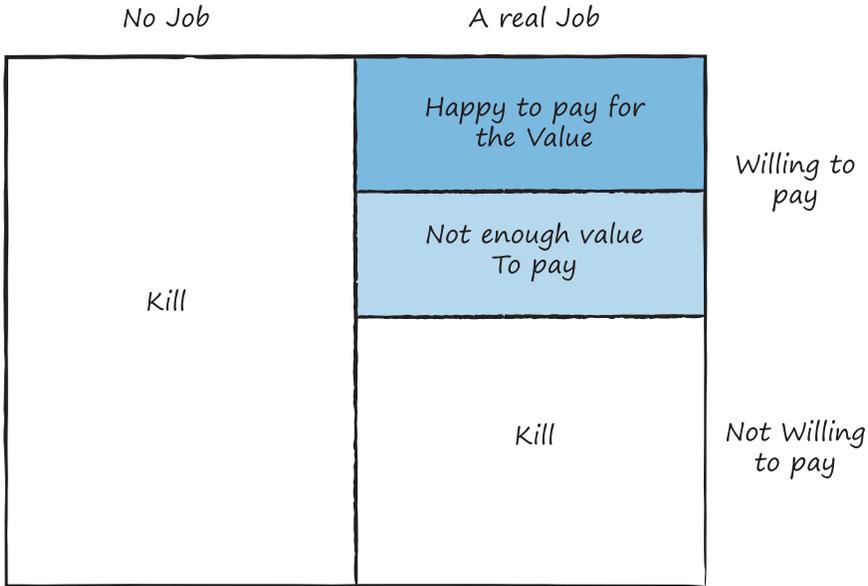
As discussed in the previous chapter, the majority of new products fail. Innovation generates good ideas, but it also inevitably generates less-than-good ideas. It is essential to be able to tell the difference and to kill the wrong ideas. An important part of that is protocol, providing explicit kill or go decisions throughout a project.

Studies show a clear correlation between having such a protocol and projects that result in profitable products. A project definition must have the criteria and review process for deciding whether it should continue or be killed (for more, see perhaps one of the best articles written on the difference between winners from losers; *New products, what separates winners from losers* by Cooper & Kleinschmidt).

Passion, persistence, and commitment are needed to turn ideas into products. But these can also prevent killing a project at the right time because we find it hard to give up on something in which we have invested time and enthusiasm. We believe in the new idea, but this must be tempered with a realization that, further down the line, it may turn out not to yield significant benefit. If that happens, we must set aside our pride and emotional involvement and kill the project if necessary.

It is easy to check if a new product is likely to succeed. Just ask these simple questions:

- Does it fulfill a real customer-job-to-be-done? (see Chapter 6) and is it something consumers really want (see Chapter 19)
- Will customers like it enough to pay you for it? (see Chapter 19)



If you can answer “yes” to these questions, your product belongs in the top right quadrant of the diagram above.

If you cannot answer “yes” to these questions, you need to shoot that puppy. Now!

Even if you answered “yes”, that may still not be enough; commercial success also depends on *how much* your potential customers are willing to pay and whether that will enable you to make a profit.

Several studies show that in today’s interconnected, hypercompetitive world, “good enough” is just not good enough. If your new product is not going to be exceptional, it will probably not be a commercial success, and it is better to kill it and focus resources on other products.

**“People think focus means saying yes to the thing you’ve got to focus on. But that’s not what it means at all. It means saying no to the hundred other good ideas that there are. You have to pick carefully.”**

STEVE JOBS

If it is really that simple, why do we still get so many products that fail? An important part of the answer is that organizations are reluctant to kill projects even when it is clear that they will never deliver exceptional outcomes. Innovators just do not want to hear that customers are not in love with the idea they have invested so much in. That kind of rejection can be a painful experience, almost like a romantic break-up. To avoid this pain, people may avoid, ignore, or try to negate negative responses and continue with projects long after they should have been canceled.

Emotional investment and cognitive bias (listening to positive feedback while ignoring the negative) cloud decisions about whether new products are viable. Clinging to ideas that can only ever lead to products that are good enough is as harmful as failing to persevere with those that can be exceptional.

**“It’s easy to kill bad ideas; great companies also kill great ideas early.”**

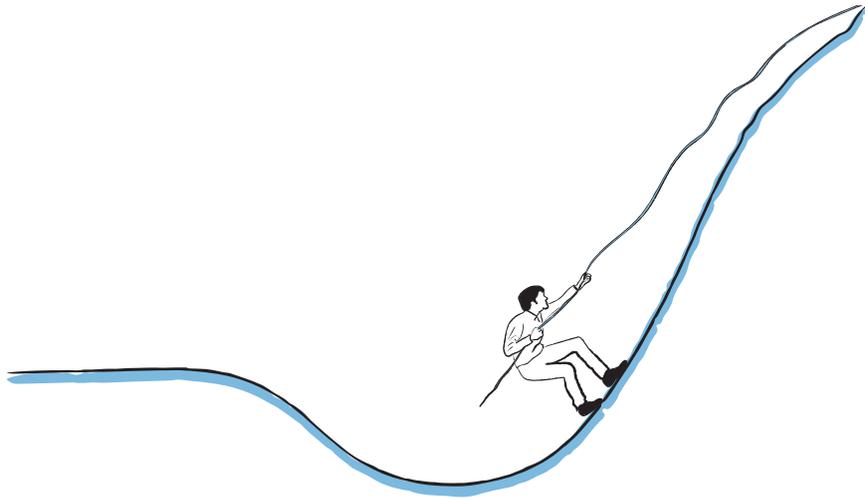
JACK WELCH

Studies have repeatedly shown the value of killing projects that do not provide real value. A paper published in the Harvard Business Review in 2017 found that one of the things that distinguished successful companies was that, compared to unsuccessful companies, they killed **nine times** more projects before they even got to the detailed design stage: *“They eliminate the walking dead to save the living.”*

Smart quitting, quitting projects at the right time and for the right reasons, is a critical element of innovation success. But how do you recognize the right time and the right reasons for quitting?

## Three Kinds of Quitting.

In the book *The Dip*, author Seth Godin introduced the notion of the dip. Godin explains that any new activity starts out easy because you are doing the simple stuff, learning quickly, making progress, and getting lots of feedback. After a while, it gets harder, progress is slower, and it all begins to feel like hard work. That is the dip.



Getting through the dip and out the other side is what separates ideas from products. Getting past this point is not easy, but if you do not, you will never get to where you want to be. Precisely because it is not easy, getting past the dip creates scarcity and value. Opportunity lies on the far side of the dip.

You need to persevere to get through the dip, but on some projects, you will quit. Godin defines three different types of quitting:

**Quitting in the dip.** Being in the dip is not fun. It is hard work for little immediate return. But it is also where you learn the most, and getting through offers the greatest potential rewards in the long-term.

**Tactical quitting.** Sometimes you realize that the direction you are going in is wrong. You may have a viable product, but you learn to pivot it in a new direction to produce something commercially viable.

**Strategic quitting.** You are making progress with a new product, but you realize that it is not doing a job that people are willing to pay for, and it is never going to be exceptional. That is when you decide to quit to focus instead on other projects that have a better chance of leading to exceptional products.

You may also discover that you are not really in a dip at all but in something quite different. Godin describes two other curves that describe project progress; the *cul-de-sac* and the *cliff*. The *cul-de-sac* is a never-ending dip. No matter how much effort you put in or how long you work on it, things will not get any better or any worse. The *cliff* is a dip that just keeps on getting deeper and deeper, using up more and more time and resources without ever leading to an upslope on the other side.

Neither of these will lead to a good outcome. Cul-de-sacs ultimately lead nowhere and cliffs soak up resources for no appreciable return.

## Good (and Bad) Reasons for Quitting.

Quitting simply because you are in the dip is generally a bad idea, as this is where you learn the most and getting to the other side is where the real opportunities lie. You have to balance short-term pain against the long-term goal. The exception is if you realize that the dip is so deep and likely to be so prolonged that you just do not have the resources to see it through. In those circumstances, quitting in the dip is a reasonable response.

Quitting because you realize that you are actually in a cul-de-sac or on a cliff or for tactical or strategic reasons are all valid. However, there are some key questions you need to ask first:

**Am I panicking?** Quitting because things are getting scary or stressful is never a good idea. It wastes all the work you have done up to that point, and it is often a knee-jerk reaction to short-term problems.

**Who am I trying to influence?** Do you have a line manager to whom you have to justify a lack of progress? Can you keep that person focused on the end result and willing to commit to what may seem like the investment of resources for little tangible outcome? Is the customer feedback on your planned product less than enthusiastic? Can you use further development to improve the product?

**Am I making measurable progress?** Slogging through the dip can be hard work, but you need to recognize whether you are moving forward. Progress can be slow, but you should recognize the difference between moving forward, albeit slowly, and standing still or even going backward.

By considering these questions honestly, you can arrive at a better answer to the overall question of whether it is the right time to quit. Panic is never a good reason for taking any kind of decision. Planning and setting parameters and timescales for review and potential quitting, can avoid this. If you are making progress, use that to convince your line manager and do whatever you need to improve feedback. If you are not, or if feedback suggests that the product may be good enough but not exceptional, those are good reasons to quit.

## How to Become a Winner by Quitting.

Smart quitting is part of being an innovation winner. Quit too early, and you may miss potential benefits. Quit too late, and you risk wasting resources that could have been better used elsewhere. To make sure that you are quitting for the right reasons and at the right time, you need to consider a few more things about the project and yourself:

**Am I biased?** The answer to this is almost certainly yes because we are all biased. We discussed cognitive dissonance and confirmation bias in Chapter 18. The important thing is to understand these biases and make sure they are not unduly influencing the decision to quit. Be objective.

**Is there a job-to-be-done?** This is fundamental. We discussed jobs-to-be-done back in Chapter 6. If you cannot identify the job-to-be-done for your product, kill the project.

**Are people willing to pay for it?** This is also fundamental. Customers must be willing to pay to get the job done if your product is going to make a profit. We talked about how to get customer feedback in Chapter 19. If people are not willing to pay for it, kill the project.

**Will they pay enough for it?** Are there alternative products that potential customers could choose? How much will they be willing to pay for your product? Is that enough to make it profitable? If customers are not willing to pay enough to make the product profitable, kill the project.

Asking the questions in this and the preceding sections are so vital that they cannot be left to chance. You cannot wait until you are in the grind of the dip before you start asking these questions. Instead, you need to set **Kill Points** and be prepared for **Real Kills**.

Knowing when and why to quit is so important that these things must be part of project planning right from the start. You need to decide in advance when you are going to consider quitting. That is not an acceptance of potential failure; it is a recognition that knowing when to kill a project is a vital element of planning.

It means building kill points into the project schedule. These are the times at which the project will be rigorously assessed according to agreed criteria. If it fails to meet those criteria, it will be killed. Setting this out in advance makes the process transparent, and having pre-set times for review means that it gets done regularly.

Kill must really mean kill. It is tough to quit a project in which you have an emotional commitment. It can feel like admitting that you made a mistake in starting the project in the first place. The most common way to avoid confronting this issue is to allow the project to continue but with reduced priority. That never works. Even projects consigned to the “*back burner*” continue to soak up resources that could be better used elsewhere. Give a project 100% commitment or kill it.

Killing a project is a bold and decisive move. It may be painful, but without this discipline, you will not focus on the projects that matter. Kill points are just as important in project planning as any other milestones.

Quitting a project is not the end of the story. Winners capture the learning that comes even from projects that are killed and use this to inform future projects. Learning what does not work can be just as important as discovering what does. You must have in place systems that capture, record, and share the learning derived from all projects, even those that do not deliver.

## Do Not Be Afraid to Quit.

Something that has become almost a mantra in business and other realms of life is the expression “*Winners don’t quit*”. My own experience and repeated studies have shown this to be a dangerous fallacy. A willingness to persevere even in the face of setbacks is essential. Still, the truth is that the most successful organizations not only quit, but they also do it even more often than their competitors.

**“Winners quit all the time. They just quit the right stuff at the right time.”**

SETH GODIN, *THE DIP*

You need to quit mediocre projects to focus resources on the projects that can deliver something exceptional.

For a while, I was the lead mentor at the European Innovation Academy. They have over 17,000 alumni, and most have gone through their extreme 21-day acceleration program. We investigated what separated top teams from other teams and identified puppy killing as a core skill. Today, “*puppy killer*” is a recommended role in every group.

Smart quitting is a winning strategy! Losers allow projects to stumble on long after it is apparent that they will yield only marginal value. Be bold. Be decisive. Be disciplined. Shoot those puppies and focus your efforts on the things that really matter!

We all need to get over our feeling that quitting represents failure. We especially need to stop hiding behind the idea that we do not need to quit, that we can just put something aside until the time is right. I know I do this, and I suspect that you do too. When we start something new, and it does not work out, it is less painful to tell ourselves that we have not really given up on it. It is much easier to put those new running shoes in the back of the cupboard. We can tell ourselves that maybe, “*when the weather’s better*” or “*in the vacation*”, we will use them. These excuses are more comfortable to accept than recognizing that we do not have the ability or discipline to run regularly.

It would be much more useful to accept that we are never going to be a runner, quit, and look for another means of improving fitness. But all the time that those running shoes are in the back of the cupboard, we can fool ourselves that we do not need to do anything else. That is why quitting, really quitting, is essential. If we quit, we cannot pretend that we are dealing with something when we are not. Quitting also takes away the focus from things that are not going to deliver and allows us to give 100% of our energy and attention to the things that will.

Let me leave you with a final quote from *The Dip*:

“You’re Astonishing. How dare you waste it? You and your organization have the power to change everything. To create remarkable products and services. To over-deliver. To be the best in the world. How dare you squander that resource by spreading it too thin. How dare you settle for mediocre just because you’re busy coping with too many things on your agenda, racing against the clock to get it all done. The lesson is simple: If you’ve got as much as you’ve got, use it. Use it to become the best in the world, to change the game, to set the agenda for everyone else. You can only do that by marshaling all of your resources to get through the biggest possible Dip. In order to get through that Dip, you’ll need to quit everything else.”

## Key Take-Aways



- **Quitting is not failing** – smart quitting is an essential business strategy that leads to improved focus.
- **Knowing when to quit is as important as being willing to persevere** – resources are finite. The only way to succeed is by focusing what you have on what will deliver the best results. That means killing projects that will not deliver or will only deliver something good enough.
- **Quit for the right reasons** – quitting because you are in the dip, tired, stressed, or plain nervous is a bad idea. Quitting because you realize that a project is not going anywhere or will not lead to an exceptional product is an excellent idea.
- **Know your biases** – emotional investment leads to cognitive dissonance and confirmation bias. Do not let these cloud decisions about whether projects should continue.
- **Be systematic** – build regular kill points into project planning and make real kills when necessary.
- **Really kill** – do not hide behind deferment or giving a project a lower priority. Give each project 100% commitment or kill it.



**Next Steps:** This canvas is about being honest. Can you ever be profitable? If no, shoot the puppy. Can you ever be the best? If not, consider rethinking your market. If still not possible, shoot the puppy. Everyone, every business, goes through difficult times. Are you in a dip or a dead-end? If a dead-end, shoot the puppy. Use the canvas that follows to think through these questions.

# Chapter 20

Shoot your puppies and move on: Why killing ideas is as important as building new ideas and how to do it.



60  
Minutes

## Objectives

To help you ask the hard questions about the future of your solution. "Do you have a puppy that needs to be shot?"

## Deliverables

Help to make the decision "Should you shoot your puppy or move on"

## How to

The question here is "Can you make it" even if people want your solution and are willing to pay. Can you ever be profitable? If no, shoot the puppy. Can you ever be the best? If not, consider rethinking your market. If still not possible, shoot the puppy. Everyone, every business, goes through difficult times. Are you in a dip or a dead-end? If a dead-end, shoot the puppy.

## The Full Communicate/Check Canvas

This Chapter

### Communicate And Check Canvas

Move left to change issue of alignment with Analysis below

4 Can you make it work?

**Analysis**

WTP - Yes (Continue)

WTP - No (Kill)

WTP Enough Continue and optimize

Can you make it work?

Dead End (Kill)

Dip (Continue)

**Analysis**

**Dead End**

Can you ever be profitable?

Can you be the best?

**Test : Ability to extract value**

If you're in a dip. Can you make it through the dip? How?

Source: The Dip by Seth Godin

**Workplace**

**How Forward?**

The right next steps and why

How do you know if you have done this canvas right ?



## CHECK LIST

- Be honest. Recognize projects with limited or no potential and shoot those puppies.
- If you plan to continue, make sure you are continuing for the right reasons. (Sunk costs are not the right reason)
- Really kill or really continue. Do not hide behind deferment or giving a project a lower priority.
- Give your idea 100% or kill it..



tiny.cc/m-cc-template



**Next Up:** If you've decided that your idea is worth pursuing, it is time to improve your ideas systematically. In the next section, you'll learn about the importance of systems, how to face your biggest fears, and move forward, step by step.

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Dr. Reinhard Ematinger invites executives to set and achieve daring goals and to “think” products and services from customers’ point of view by applying tools from business model innovation and customer value creation, lego serious play® and playmobil®pro, service design thinking, and objectives and key results. He has 25 years of industry experience in consulting and training, business development and corporate universities plus more than 140 terms of university lectures and several books care for relevance of his work.



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Urs is a Corporate Innovation and Entrepreneurship Consultant, recognized Startup Expert, Serial Entrepreneur and Investor, who helps Corporations like Bayer, Allianz, HP, Toyota, B. Braun, Suez, Clariant or Migros launch successfully new products and services using Lean Startup Methodology and Mentorship. He has specialized in accompanying corporations in open innovation processes through the identification, evaluation and integration of external opportunities with startups and scaleups, in order to accelerate their innovation processes and increase their competitiveness. He has become an expert on Intrapreneurship as well as Startup Innovation Strategies, specifically on the changes big corporations need to make to allow for new ventures to be built in corporate settings.

As a Director of the Founder Institute, the world’s largest acceleration program for early stage entrepreneurs, he initiated and leads the chapters in Barcelona and Andalusia, where he has trained and accompanied dozens of entrepreneurs from the idea stage to the launch phase of their ventures.

## ABOUT THE LEAD AUTHOR



### LEAD AUTHOR

**BRYAN CASSADY**

(Oostende, Belgium)

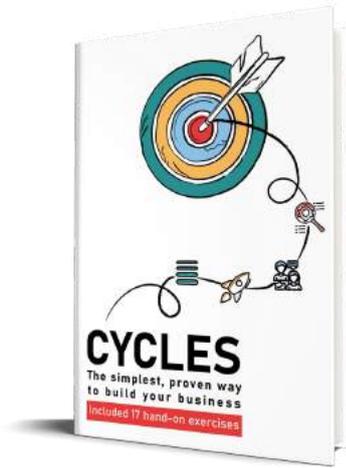
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Bryan is a passionate believer in anyone's ability to innovate and build new businesses. He has personally built 11 companies in 6 countries, with eight of these making money.

For the last eight years, Bryan has been coaching startups and scale-ups and trying to help larger companies regain the spirit of startups. He has taught at the KU Leuven, The Solvay business school, EDHEC, ESCLA, and been a guest lecturer at the University of Chicago, INSEAD, Cornell, Berkeley, and many other schools.

He has also led programs like Founder Institute Brussels and the European Innovation Academy. This book results from four years of research with over 400 companies on what really drives innovation success.



# BOOK CYCLES

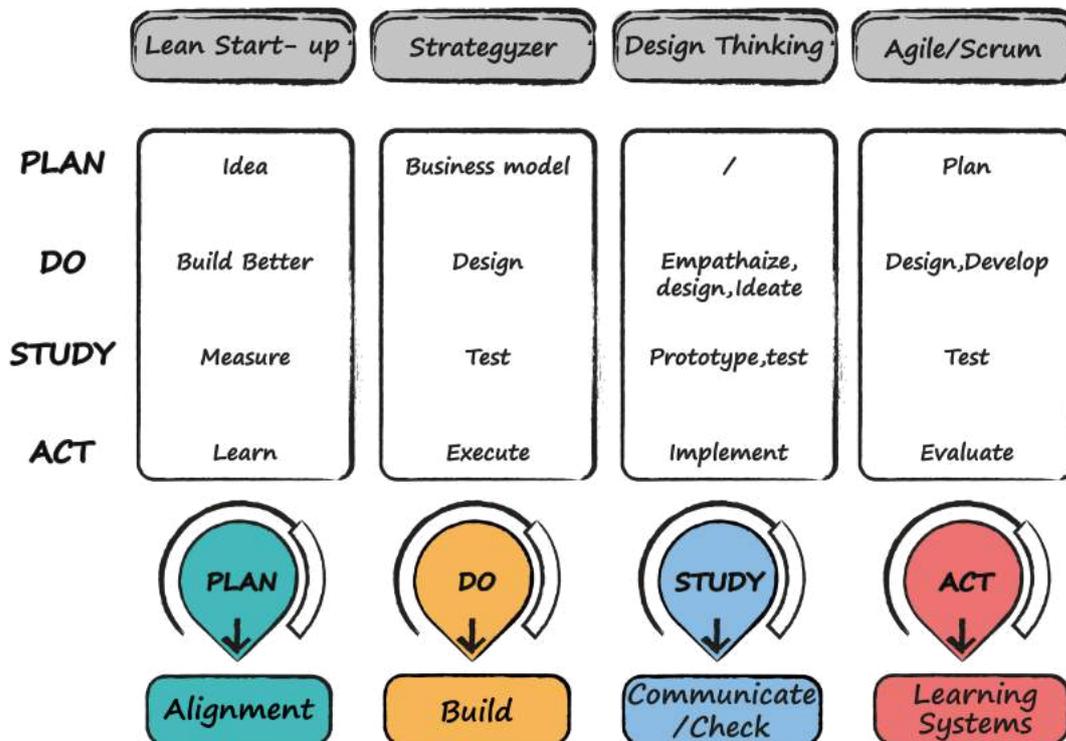
Standing on the shoulders of giants...

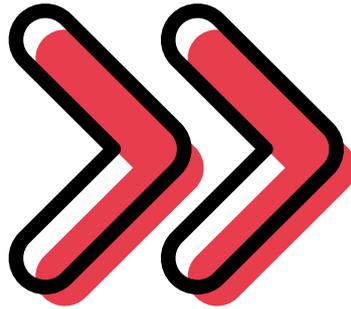
Many of the leading methods- lean start up, scrum, agile are built around the ideas of PDSA (Plan, do , study, act)

This book builds on these ideas, but thanks to the expertise of 22 co-authors, goes one step further.

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