Notebook - How Big Things Get Done

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Flyvbjerg, Bent, Gardner, Dan

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> I call this pattern “Think fast, act slow,” for reasons I’ll explain later. It is a hallmark of failed projects.

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> I call the pattern followed by the Empire State Building and other successful projects “Think slow, act fast.”

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> their preliminary numbers were so dismal that they said it would take a big improvement for IT projects to rise to the level of awfulness of transportation projects.

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> the “Iron Law of Megaprojects”: over budget, over time, under benefits, over and over again.

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> In total, only 8.5 percent of projects hit the mark on both cost and time. And a minuscule 0.5 percent nail cost, time, and benefits.

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> A complex systems theorist might describe what happened by saying that the dynamic interdependencies among the parts of the system—the wind, the canal, the ship, and supply chains—created strong nonlinear responses and amplification.

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> a project can’t be completed instantly, so we can’t close the window entirely. But we can make the opening radically smaller by speeding up the project and bringing it to a conclusion faster. That is a

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> main means of reducing risk on any project. In sum, keep it short!

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> A project begins with a vision that is, at best, a vague image of the glorious thing the project will become. Planning is pushing the vision to the point where it is sufficiently researched, analyzed, tested, and detailed that we can be confident we have a reliable road map of the way forward.

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> Abraham Lincoln is reputed to have said that if he had five minutes to chop down a tree, he'd spend the first three sharpening the ax.[29] That's exactly the right approach for big projects: Put enormous care and effort into planning to ensure that delivery is smooth and swift. Think slow, act fast: That's the secret of success.

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> PROJECTS DON'T GO WRONG, THEY START WRONG

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> That is, sadly, typical. On project after project, rushed, superficial planning is followed by a quick start that makes everybody happy because shovels are in the ground. But inevitably, the project crashes into problems that were overlooked or not seriously analyzed and dealt with in planning. People run around trying to fix things. More stuff breaks. There is more running around. I call this the "break-fix cycle." A project that enters it is like a mammoth stuck in a tar pit.

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> I'll take a close look at what I call "Pixar planning," how the movie studio and others use simulation and iteration to produce a plan that is creative, rigorous, detailed, and reliable—and highly likely to make delivery smooth and swift. I'll use "Pixar planning" as a name and a model for planning, not just at Pixar but for any planning that develops a tested and tried plan; that is, a plan worthy of its name.

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> “Here was another example of acting before thinking,”

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> Purposes and goals are not carefully considered. Alternatives are not explored. Difficulties and risks are not investigated. Solutions are not found. Instead, shallow analysis is followed by quick lock-in to a decision that sweeps aside all the other forms the project could take. “Lock-in,” as scholars refer to it, is the notion that although there may be alternatives, most people and organizations behave as if they have no choice but to push on, even past the point where they put themselves at more cost or risk than they would have accepted at the start. This is followed by action.

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> Happy endings are rare when projects start in a rush based on the commitment fallacy.

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> what I call “strategic misrepresentation,” the tendency to deliberately and systematically distort or misstate information for strategic purposes.[5] If you want to win a contract or get a project approved, superficial planning is handy because it glosses over major challenges, which keeps the estimated cost and time down, which wins contracts and gets projects approved. But as certain as the law of gravity, challenges ignored during planning will eventually boomerang back as delays and cost overruns during delivery. By then the project will be too far along to turn back. Getting to that point of no return is the real goal of strategic misrepresentation. It is politics, resulting in failure by design.

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> YOU WANT THE FLIGHT ATTENDANT, NOT THE PILOT, TO BE AN OPTIMIST

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> We are a deeply optimistic species. That makes us an overconfident species.[9] The large majority of car drivers say that their driving skills are above average.

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> The sheer pervasiveness of optimism and overconfidence suggests that they are useful for us, individually and collectively, and there's plenty of research and experience to support that conclusion. We definitely need optimism and a can-do attitude to inspire big projects and see them through. Or to get married and have kids.

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> My key heuristic for managing optimism on projects is "You want the flight attendant, not the pilot, to be an optimist."

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> "Optimism is widespread, stubborn, and costly," observed Kahneman.

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> One of the basic insights of modern psychology is that quick and intuitive "snap judgments" are the default operating system of human decision making—"System One," to use the term coined by the psychologists Keith Stanovich and Richard West and made famous by Kahneman. Conscious reasoning is a different system: System Two.[14] A key difference between Systems One and Two is speed. System One is fast, so it always delivers first. System Two is slow; it can get involved only after System One delivers. Both systems may be right or wrong.

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> But another basic insight of psychology is that when we have a strong intuitive judgment we seldom subject it to slow, careful, critical scrutiny. We just go with it, spontaneously settling for whatever System One decided.

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> But the intuitive judgments generated by System One are not experienced as emotions. They simply "feel" true. With the truth in hand, it seems perfectly reasonable to act on it. As Kahneman wrote, System One is "a machine for jumping to conclusions."

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> as Klein demonstrated, people ordinarily take the first option that occurs to them and quickly run it through a mental simulation. If it seems to work, they go with that option. If it doesn't, they search for another and repeat the process. This method tends to work well for familiar decisions, especially when there is little time to make them, and it can work brilliantly when done by an expert, as we will see later. But in the wrong circumstances, it's a mistake.

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> For us to be so consistently wrong, we must consistently ignore experience.

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> Using a best-case scenario as the basis for an estimate is a really bad idea because the best case is seldom the most likely way the future will unfold. It's often not even probable.

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> Bezos carefully limited the bias for action to decisions that are "reversible." Don't spend lots of time ruminating on those sorts of decisions, he advises. Try something. If it doesn't work, reverse it, and try something else. That's perfectly reasonable. It is also inapplicable to many decisions on big projects because they are so difficult or expensive to reverse that they are effectively irreversible:

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> managers feel more productive executing tasks than planning them,"

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> It isn't the bias for action promoted by Jeff Bezos; it's a bias against thinking.

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> Planning is working on the project. Progress in planning is progress on the project, often the most cost-effective progress you can achieve. We lose sight of these facts at our peril.

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> The French architect Jean Nouvel, a winner of the Pritzker Architecture Prize, architecture's Nobel, was blunt about the purpose of most cost estimates for signature architecture. "In France, there is often a theoretical budget that is given because it is the sum that politically has been released to do something. In three out of four cases this sum does not correspond to anything in technical terms. This is a budget that was made because it could be accepted politically. The real price comes later. The politicians make the real price public where they want and when they want."[24] That's a long way of saying that estimates aren't intended to be accurate; they are intended to sell the project. In a word, they are lies—or spin, to use more polite language.

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> In the world of civic projects, the first budget is really just a down payment. If people knew the real cost from the start, nothing would ever be approved"

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> 'if we gave the true expected outcome costs nothing would be built.'

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> With contracts signed, the next step is to get shovels in the ground. Fast. "The idea is to get going," concluded Willie Brown. "Start digging a hole and make it so big, there's no alternative to coming up with the money to fill it in."

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> often the costs of out-of-control projects do not fall on those who bring them about.

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> "lock-in" or "escalation of commitment."[33] If escalation of commitment comes after the commitment fallacy, there is overcommitment to the second degree. This typically spells disaster, or at least a result vastly inferior to what could have been achieved with a more thoughtful approach.

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> the “sunk cost fallacy.”

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> sticking with a fallacy is politically safer than making a logical decision.

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> Projects are often started by jumping straight to a solution, even a specific technology. That’s the wrong place to begin. You want to start by asking questions and considering alternatives. At the outset, always assume that there is more to learn. Start with the most basic question of all: Why?

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> good planning explores, imagines, analyzes, tests, and iterates. That takes time. Thus, slow is a consequence of doing planning right, not a cause. The cause of good planning is the range and depth of the questions it asks and the imagination and the rigor of the answers it delivers. Notice that I put “questions” before “answers.” It’s self-evident that questions come before answers. Or rather, it should be self-evident. Unfortunately, it’s not. Projects routinely start with answers, not questions.

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> the “starchitect” phenomenon,

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> Projects are not goals in themselves. Projects are how goals are achieved. People don’t build skyscrapers, hold conferences, develop products, or write books for their own sakes. They do these things in order to accomplish other things.

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> Developing a clear, informed understanding of what the goal is and why—and never losing sight of it from beginning to end—is the foundation of a successful project.

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> What's natural is WYSIATI—What You See Is All There Is

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> People are terrible at getting things right the first time. But we're great at tinkering. Wise planners make the most of this basic insight into human nature. They try, learn, and do it again. They plan like Pixar and Frank Gehry do.

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> a Latin verb, experiri. Experiri means "to try," "to test," or "to prove." It is the origin of two wonderful words in English: experiment and experience.

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> A bad plan is one that applies neither experimentation nor experience. The plan for the Sydney Opera House was very bad.

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> First, iteration frees people to experiment,

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> Second, the process ensures that literally every part of the plan, from the broad strokes to the fine details, is scrutinized and tested.

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> In bad planning, it is routine to leave problems, challenges, and unknowns to be figured out later.

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> Third, an iterative process such as Pixar's corrects for a basic cognitive bias that psychologists call the "illusion of explanatory depth."

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> “People feel they understand complex phenomena with far greater precision, coherence, and depth than they really do,” researchers concluded.

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> When people try and fail to explain what they mistakenly think they understand, the illusion dissolves.

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> Planning is cheap. Not in absolute terms, perhaps.

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> a big project problems are inevitable. The only question is, When will they arise? An iterative process greatly increases the probability that the answer to that question is “in planning.”

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> Planning, as I see it, is not merely sitting and thinking, much less a rule-based bureaucratic exercise of programming. It is an active process. Planning is doing: Try something, see if it works, and try something else in light of what you’ve learned. Planning is iteration and learning before you deliver at full scale, with careful, demanding, extensive testing producing a plan that increases the odds of the delivery going smoothly and swiftly.

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> When a minimum viable product approach isn’t possible, try a “maximum virtual product”—a hyperrealistic, exquisitely detailed model like those that Frank Gehry made for the Guggenheim Bilbao and all his buildings since and those that Pixar makes for each of its feature films before shooting.

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> make the conceptual shift to planning as an active, iterative process of trying, learning, and trying again, all sorts of ways to “play” with ideas, as Gehry and Pixar do, will suggest themselves.

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> Experience is invaluable. But too often it is overlooked or dismissed for other considerations. Or it is simply misunderstood and marginalized.

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> Big projects involve big money and big self-interest. And since “who gets what” is the core of politics, there is politics in every big project, whether public or private.

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> “uniqueness bias,” which means they tend to see their projects as unique, one-off ventures that have little or nothing to learn from earlier projects.

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> being first to market can confer advantages in certain specific circumstances, but it comes at the terrible cost of an inability to learn from the experience of others.

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> If decision makers valued experience properly, they would be wary of a technology that is new, because it is inexperienced technology.

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> The Olympics are forever planned and delivered by beginners—a crippling deficiency I call “Eternal Beginner Syndrome.”

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> And in both planning and delivery, there is no better asset for a big project than an experienced leader with an experienced team.

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> In short, if you have phronesis, you've got it all. Therefore, a project leader with abundant phronesis is the single greatest asset a project can have. If you have a project, hire a leader like that.

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> If there is a design—or a system, process, or technique—that has delivered many times before, use it, or tweak it, or mix-and-match it with similarly proven designs.

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> the less proven something is, the more it must be tested.

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> SO YOU THINK YOUR PROJECT IS UNIQUE? Think again. Understanding that your project is "one of those" is key to getting your forecasts right and managing your risks.

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> When delivery fails, efforts to figure out why tend to focus exclusively on delivery. That's understandable, but it's a mistake, because the root cause of why delivery fails often lies outside delivery, in forecasting, years before delivery was even begun.

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> In psychology, the process Caro used to create his forecast is known as "anchoring and adjustment."[6] Your estimate starts with some fixed point, twelve chapters of three weeks each in Caro's case. That's the "anchor." Then you slide the figure up or down as seems reasonable, to one year for Caro. That's "adjustment."

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> That means the quality of the anchor is critical. Use a good anchor, and you greatly improve your chance of making a good forecast; use a bad anchor, get a bad forecast.

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> Much subsequent research revealed that people will anchor in almost any number they happen to be exposed to prior to making their forecast.

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> so realism was seen as pessimism and ignored.

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> When we experience delays and cost overruns, we naturally go looking for things that are slowing the project down and driving up costs. But those delays and overruns are measured against benchmarks. Are the benchmarks reasonable?

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> Logically, that should be the first question that is asked, but it rarely comes up at all. Once we frame the problem as one of time and money overruns, it may never occur to us to consider that the real source of the problem is not overruns at all; it is underestimation.

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> To understand what a reference class is, bear in mind that there are two fundamentally different ways to look at a project. The first is to see it as its own special undertaking. All projects are special to some degree.

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> people not only tend to naturally look at their projects this way, they tend to exaggerate just how unusual their specific project really is. This is the “uniqueness bias”

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> “You’re absolutely unique, just like everyone else.” Projects are like that. Whatever sets a project apart, it shares other characteristics with projects in its class.

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> Kahneman and Tversky dubbed these two perspectives the “inside view” (looking at the individual project in its singularity) and the “outside view” (looking at a project as part of a class of projects, as “one of those”). Both are valuable. But they’re very different. Although there’s little danger that a forecaster will ignore the inside view, overlooking the outside view is routine. That’s a fatal error. To produce a reliable forecast, you need the outside view.

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> Many small probabilities added together equal a large probability that at least some of those nasty surprises will actually come to pass.

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> things almost never go according to plan. On big projects, they don’t even come close.

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> I came to call this process “reference-class forecasting” (RCF).

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> Daniel Kahneman wrote in Thinking, Fast and Slow that using reference-class forecasting is “the single most important piece of advice regarding how to increase accuracy in forecasting through improved methods.”

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> These are experience-based, real-world outcomes, not estimates, so they’re not distorted by psychology and strategic misrepresentation. Use them to anchor your forecast, and you will create an estimate that is rooted in reality, undistorted by behavioral biases, making it a better estimate.

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> All we need to know is that the numbers for the reference class do reflect how common and how big the unknown unknowns really were for those projects, which means that your forecast will

> reflect those facts, too.

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> The first is that for a lot of people and organizations, the fact that RCF eliminates biases is a bug, not a feature. As I discussed in chapter 2, shoddy forecasting is the bread and butter of countless corporations. They do not want the people who authorize projects and pay the bills to have a more accurate picture of what projects will cost and how long they will take. They're going to stick with the status quo, at least until they're forced to change—for instance, by assigning legal liability for blatantly biased forecasts, which is increasingly happening.[18]

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> A second challenge to overcome is the strength of uniqueness bias. Kahneman writes about a time when he and some colleagues set out to compose a textbook together. They all agreed that it would take roughly two years. But when Kahneman asked the only member of the group with considerable experience in producing textbooks how long it usually takes, that expert said he couldn't recall any project taking less than seven years. Worse, about 40 percent of such projects are never finished, he said. Kahneman and his colleagues were briefly alarmed—but then they moved on just as if they had never heard those unwelcome facts because, well, their project felt different. It always does. "This time is different" is the motto of uniqueness bias. The textbook was ultimately finished eight years later.[19] If the greatest living student of cognitive bias can be suckered by uniqueness bias, it's small wonder that the rest of us are vulnerable, too, or that avoiding the trap requires awareness and sustained mental effort. The third reason RCF is still not as widely used as it should be is the simplest. It's the data. Calculating an average is easy, but only once you have the numbers in hand. That is the hard part.

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> "regression to the tail."[22] In that situation, relying on the mean and assuming that your result will be close to it is a dangerous mistake.[23]

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> If you face a fat-tailed distribution, shift your mindset from forecasting a single outcome ("The project will cost X") to forecasting risk ("The project is X percent likely to cost more than Y"), using the full range of the distribution.

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> But the tail outcomes—the “black swans”—cover about 20 percent of the distribution. That means a 20 percent chance of ending up in the tail, which is too much risk for most organizations.

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> You can do that with risk mitigation. I call it “black swan management.”

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> Some tails are simple to cut. Tsunamis are fat-tailed, but if you build well inland or erect a high enough seawall, you eliminate the threat. Earthquakes are also fat-tailed, but build to an earthquake-proof standard, as we did with the schools in Nepal, and you are covered. Other tails require a combination of measures; for a pandemic, for instance, a blend of masks, tests, vaccines, quarantines, and lockdowns to prevent infections from running wild.[25] That’s black swan management.

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> For big projects, black swan management typically requires a combination of measures. I started this book with one: “Think slow, act fast.” We saw that delivery is when things can go horribly and expensively wrong. Exhaustive planning that enables swift delivery, narrowing the time window that black swans can crash through, is an effective means of mitigating this risk. Finishing is the ultimate form of black swan prevention; after a project is done, it can’t blow up, at least not as regards delivery.

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> projects seldom nosedive for a single reason.

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> Interestingly, early delays are not seen as a big deal by most project leaders. They figure they have time to catch up, precisely because the delays happen early. That sounds reasonable. But it’ s dead wrong. Early delays cause chain reactions throughout the delivery process. The later a delay comes, the less remaining work there is and the less the risk and impact of a chain reaction. President Franklin Roosevelt got it right when he said, “Lost ground can always be regained—lost time never.”[28] Knowing this, we advised measures that would cut the probability of early delays

> and chain reactions.

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> As with reference-class forecasting, the big hurdle to black swan management is overcoming uniqueness bias. If you imagine that your project is so different from other projects that you have nothing to learn from them, you will overlook risks that you would catch and mitigate if you instead switched to the outside view.

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> Recall what Benjamin Franklin wrote in 1758: “A little neglect may breed great mischief.” This is why high safety standards are an excellent form of risk mitigation and a must on all projects. They’re not just good for workers; they prevent little things from combining in unpredictable ways into project-smashing black swans.

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> In this complex world, we can and must move the probabilities in our favor, but we can never achieve certainty. Good risk managers know this in their bones and are prepared for it.

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> So now we can put reference-class forecasting and risk management into the toolbox, along with experience, Pixar planning, and thinking from right to left. These are the essential tools for thinking slow in planning before acting fast in delivery.

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> CAN IGNORANCE BE YOUR FRIEND? Planning ruins projects, some say. Just get going! Trust your ingenuity! It’s a wonderful sentiment backed by superb stories. But is it true?

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> “Necessity is the mother of invention,” after all.

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> For that, Hirschman argued, we must thank ignorance; it's our friend in getting projects started. He called it "providential ignorance."

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> This ignorance makes us too optimistic. And that's a good thing, according to Hirschman. "Since we necessarily underestimate our creativity," he wrote, "it is desirable that we underestimate to a roughly similar extent the difficulties of the tasks we face, so as to be tricked by these two offsetting underestimates into undertaking tasks which we can, but otherwise would not dare, tackle."

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> He suggested a name for this principle: "Since we are apparently on the trail here of some sort of Invisible or Hidden Hand that beneficially hides difficulties from us, I propose 'the Hiding Hand.' "

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> In the social sciences, "survivorship bias" is the common mistake of noting only those things that made it through some selection process while overlooking those that didn't.

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> Put simply, the typical project is one in which costs are underestimated and benefits are overestimated. Picture a big project that costs more than it was supposed to and delivered less than expected:

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> Kahneman identified optimism bias as "the most significant of the cognitive biases."[

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> stress is particularly corrosive in two circumstances: when we feel that the situation is mostly beyond our control and when we feel that others are judging our competence.

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> Imaginative leaps belong in planning, not delivery. When stakes and stress are low, we are freer to wonder, try, and experiment. Planning is creativity's natural habitat.

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> The value of experienced teams cannot be overstated, yet it is routinely disregarded.

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> "design for manufacture and assembly," is how the hyperefficient car industry operates.

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> We perform at our best when we feel united, empowered, and mutually committed to accomplishing something worthwhile.

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> But by giving companies only positive incentives to perform well—including bonuses for meeting and beating benchmarks—it ensured that the interests of the many different companies working on the project were not pitted against one another.

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> "lowest bid" does not necessarily mean "lowest cost,"

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> construction workers are sharp as knives at understanding what's happening on their worksites.

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> WHAT'S YOUR LEGO? Get a small thing, a basic building block. Combine it with another and another until you have what you need. That's how a single solar cell becomes a solar panel, which becomes a solar array, which becomes a massive megawatt-churning solar farm. Modularity

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> delivers faster, cheaper, and better, making it valuable for all project types and sizes. But for building at a truly huge scale—the scale that transforms cities, countries, even the world—modularity is not just valuable, it's indispensable.

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> If you build like this, you build only one thing. By definition, that thing is one of a kind. To put that in the language of tailors, it is bespoke: no standard parts, no commercial off-the-shelf products, no simple repetition of what was done last time.

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> First, you can't build a nuclear power plant quickly, run it for a while, see what works and what doesn't, then change the design to incorporate the lessons learned. It's too expensive and dangerous. That means that experimentation—one-half of the experiri I discussed in chapter 4—is out. You have no choice but to get it right the first time.

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> Second, there's a problem with experience—the other half of experiri. If you are building a nuclear power plant, chances are that you haven't done much of that before for the simple reason that few have been built and each takes many years to complete, so opportunities to develop experience are scarce. Yet with no experimentation and little experience, you still have to get it exactly right the first time. That's difficult, if not impossible.

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> Even if you do have some experience building nuclear power plants, you probably won't have experience building this particular nuclear plant because, with few exceptions, each plant is specifically designed for a specific site, with technology that changes over time. Like Monju, it is bespoke, one of a kind. Anything bespoke is expensive and slow to make, like a tailored suit. But imagine a tailor who has little experience with suits making a bespoke suit for you and having to get it right on the first try.

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> Obstacles that were unknown are encountered. Solutions thought to work don't. And you cannot

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> make up for it by tinkering or starting over with revised plans. Operations experts call this “negative learning”: The more you learn, the more difficult and costly it gets.

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> which, given the bespokeness, the complexity, the lack of experimentation, your lack of experience, the negative learning, and the need to get everything right the first time, will probably be a very long time.

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> Modularity is a clunky word for the elegant idea of big things made from small things.

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> The core of modularity is repetition. Put down one Lego block. Snap on another. And another. And another. Repeat, repeat, repeat. Click, click, click. Repetition is the genius of modularity; it enables experimentation. If something works, you keep it in the plan. If it doesn’t, you “fail fast,” to use the famous Silicon Valley term, and adjust the plan. You get smarter. Designs improve. Repetition also generates experience, making your performance better. This is called “positive learning,” as we saw earlier. Repetition rockets you up the learning curve, making each new iteration better, easier, cheaper, and faster.

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> modularity is a matter of degree. The Empire State Building wasn’t modular to the extent that a Lego model of the Empire State Building is modular, but its floors were designed to be as similar as possible, with many being identical, which meant that workers often repeated work, which helped them learn and work faster.

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> Manufacturing in a factory and assembling on-site is far more efficient than traditional construction because a factory is a controlled environment designed to be as efficient, linear, and predictable as possible.

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> The technical term for this property is “scale free,” meaning that the thing is basically the same no matter what size it is. This gives you the magic of what I call “scale-free scalability,” meaning you can scale up or down following the same principles independently of where you are scalewise, which is exactly what you want in order to build something huge with ease.

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> Only proven technologies—those with a high degree of “frozen experience”—were used.

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> The pattern is clear: Modular projects are in much less danger of turning into fat-tailed disasters. So modular is faster, cheaper, and less risky. That is a fact of immense importance.

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> transforming the construction site into an assembly site, which is rightly seen as the key to success.

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> Heuristics are fast and frugal rules of thumb used to simplify complex decisions.

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> But a word of warning: Heuristics should never be used like thoughtless paint-by-numbers rules. Check whether my heuristics resonate with your own experience before using them in practice. Even more important, use them as a source of inspiration for investigating, trying new things, and developing your own heuristics—which is what really matters.

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> HIRE A MASTERBUILDER I sometimes say that this is my only heuristic because the masterbuilder—named after the skilled masons who built Europe’s medieval cathedrals—possesses all the phronesis needed to make your project happen. You want someone with deep domain experience and a proven track record of success in whatever you’re doing, whether it’s a

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> home renovation, a wedding, an IT system, or a skyscraper. But masterbuilders aren't always available or affordable, in which case you need to think further and consider some of the following.

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> "Give a good idea to a mediocre team, and they will screw it up. Give a mediocre idea to a great team, and they will either fix it or come up with something better. If you get the team right, chances are they will get the ideas right."

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> When possible, hire a masterbuilder. And the masterbuilder's team.

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> "I check myself constantly by asking whether my present actions effectively contribute to the result on the right."

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> You do it by taking all the time necessary to create a detailed, tested plan. Planning is relatively cheap and safe; delivering is expensive and dangerous.

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> unless you are doing what has literally never been done before—building a time machine, engineering a black hole—it is not unique; it is part of a larger class of projects.

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> opportunity is as important as risk. That's false. Risk can kill you or your project. No upside can compensate for that.

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kindle

> Staying focused is essential for getting projects done.

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> “I’m actually as proud of the things we haven’t done as the things we have done,”