# Group 5 – Alphabet –X, carbon sea

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| *Assignment:** *Answer the following questions :*
	+ *Slide 1 - Present Carbon-Neutral Fuel moonshot*
	+ *Slide 2 - What are the open innovation practices used?*
	+ *Slide 3 - Any other interesting takeaways*

*Remark : use the articles to construct the answer but you can also develop answer we ideas that are not in the articles** *Each group has to present their answers in 15 minutes max*
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# Adele Peters (2022) [Why Alphabet’s Moonshot Factory Killed Off A Brilliant Carbon-Neutral Fuel](https://www.fastcompany.com/3064457/why-alphabets-moonshot-factory-killed-off-a-brilliant-carbon-neutral-fuel)

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*The project Foghorn was meant to take CO2 from the oceans and turn it into fuel, but it was a dream too far ahead of its time.*

It seemed like game-changing technology: Take carbon dioxide out of the ocean, and turn it into a carbon-neutral fuel that could be used in today’s current gas tanks.

When scientists at X–formerly Google X, [the “moonshot factory” at Alphabet](https://www.fastcoexist.com/3058866/how-googles-moonshot-x-division-helps-its-employees-embrace-failure) known for driverless cars and Wi-Fi on balloons–learned about a new process for turning seawater into fuel, they partnered with the researchers behind it to try to make it real. Two years later, despite the fact that the technology worked, they killed the project.

Like some other projects at X, it started when someone happened to read a study on the new technology. Someone else invited the researcher behind it to come to the X lab to give a tech talk. After he went into more detail, they were even more interested.



“We knew if you could make a carbon-neutral fuel at a price point that would allow for commercialization, you would have such potential for impact,” says Kathy Cooper, team lead for the project, which was eventually named Foghorn.

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Transportation makes up around 14% of global greenhouse gas emissions, and while electric cars are very slowly becoming more common, other transportation sectors–like airplanes or cargo ships–don’t yet have a simple way to stop polluting. What is key is that the new fuel could be used in existing vehicles.

The fuel makes use of the rising carbon dioxide levels in the ocean; as CO2 increases in the atmosphere, concentrations of dissolved CO2 also rise in the sea, ending up in a form called bicarbonate that makes the ocean more acidic.

“The process that we’re using, in short, essentially shifts the pH of the ocean,” says Matt Eisaman, one of a team of [PARC scientists](https://www.parc.com/) who originally developed the technology. By sucking ocean water into a tank and making it more acidic, it’s possible to collect CO2 as a gas. Using another process, it’s possible to also pull hydrogen from the water. If the CO2 and hydrogen are reacted together, they become a liquid fuel.

After the X team heard Eisaman’s initial presentation, they asked him to do some quick calculations to see if making fuel like this might be commercially viable. The analysis had a huge degree of uncertainty, but it seemed like it might actually be something they could sell some day. X formed a partnership with the PARC researchers.



“We decided it was worth digging a little deeper, doing more experiments and prototyping to really understand if we could make it work,” says Cooper. “And thus Foghorn was born.”

Most ideas at X [don’t last long](https://www.fastcoexist.com/3058866/how-googles-moonshot-x-division-helps-its-employees-embrace-failure); some are killed within hours. To be viable for the innovation lab, projects have to help solve a problem that affects millions or billions of people, the technology has to be “audacious,” and there has to be a chance that it can make it to market within roughly 5 to 10 years.

If something could be commercialized sooner, the reasoning is that another company is probably working on it already; if it takes longer, it might not provide a good return, and the technology also might become outdated by the time it’s ready.

Often, back-of-the-envelope calculations make it clear that an idea couldn’t become financially viable quickly enough. When something passes the earliest tests–like Project Foghorn–it moves on to a next stage, with a “rapid evaluation” team that tries to understand the potential project’s biggest risks.

When Foghorn started, the researchers from PARC already had a working proof-of-concept; the biggest uncertainties were around cost. The team got to work building a bigger prototype in the lab, planning to take more accurate measurements that it could plug into an economic model.

It didn’t take long before a problem appeared: They realized that if the machine ran for a longer period of time, minerals would build up on the machine’s membrane, basically destroying the system. “So we had to invent our way out of that problem, which we did,” says Eisaman.

The team also found another important solution. If they partnered with desalination plants, they realized, they could avoid the expense of building pipes in the ocean, helping bring the cost down. It brought the process much closer to their target–$5 for a gallon of seawater fuel, within five years.



Still, it wasn’t quite that cheap, and the scientists realized that there was another problem; there just aren’t that many desalination plants in the ocean today. Even if they partnered with all of them, it would only be possible to produce a relatively small amount of fuel, only enough to offset about four coal plants’ worth of emissions.

Eventually–about two years after the project began–the team pulled the plug. Renewable hydrogen, a key ingredient used with their carbon dioxide to make the fuel, was too expensive to produce; the carbon dioxide itself was a little too expensive to pull out of the seawater.

“In the end, it was just a question of opportunity costs,” says Cooper. “For Matt and I, we’re always looking for what can X put its resources into in order to make the largest impact. We especially care about the issue of climate change, so we tend to look into that sector, though we’ve also evaluated others. We just thought the resources that we would put into this project we’d probably have a greater impact if it was put toward something else.”

While it was hard to pull away, the culture of the lab supports the idea of failure–and even hands out bonuses when teams agree that it’s time to give up on something.

“Because X is premised on the idea of pursuing highly risky projects, there’s just an understanding that a lot of them aren’t going to work,” says Cooper. “So it’s not seen as surprising or the fault of anyone if something doesn’t work. It’s just sort of seen as the nature of the work. And that depersonalizes it in a way that’s very helpful.”

It’s possible that the viability of the technology could change sooner than expected. A price on carbon, an R&D breakthrough, and increasing fossil fuel prices could help. But without those things, it’s hit a dead end for now.

The researchers are working on a peer-reviewed paper about the technology–and costs–that they can share with other scientists. There’s little data now about the costs of “negative emissions” technologies like this, which suck carbon out of the atmosphere rather than just slowing down its buildup.

At some point, most climate scientists think that we’ll need to use those technologies to meet the goals of the Paris climate agreement. It’s easier to pull CO2 from the ocean than the air, so there’s a good chance that Foghorn, or something like it, may eventually be used.

“In some sense [we’ll] kick it back to the research community,” says Eisaman. “Then if we wait maybe 5 to 10 years and let that research and development take place, I think eventually something like this will be commercialized. It was just a bit ahead of its time.”

## Derek Thompson (2017), theatlantic, “Google X and the Science of Radical Creativity

## ” < https://www.theatlantic.com/magazine/archive/2017/11/x-google-moonshot-factory/540648/>

# Google X and the Science of Radical Creativity: How the secretive Silicon Valley lab is trying to resurrect the lost art of invention

### I. The Question

A snake-robot designer, a balloon scientist, a liquid-crystals technologist, an extradimensional physicist, a psychology geek, an electronic-materials wrangler, and a journalist walk into a room. The journalist turns to the assembled crowd and asks: Should we build houses on the ocean?

The setting is X, the so-called moonshot factory at Alphabet, the parent company of Google. And the scene is not the beginning of some elaborate joke. The people in this room have a particular talent: They dream up far-out answers to crucial problems. The dearth of housing in crowded and productive coastal cities is a crucial problem. Oceanic residences are, well, far-out. At the group’s invitation, I was proposing my own moonshot idea, despite deep fear that the group would mock it.

Like a think-tank panel with the instincts of an improv troupe, the group sprang into an interrogative frenzy. “What are the specific economic benefits of increasing housing supply?” the liquid-crystals guy asked. “Isn’t the real problem that transportation infrastructure is so expensive?” the balloon scientist said. “How sure are we that living in densely built cities makes us happier?” the extradimensional physicist wondered. Over the course of an hour, the conversation turned to the ergonomics of Tokyo’s high-speed trains and then to Americans’ cultural preference for suburbs. Members of the team discussed commonsense solutions to urban density, such as more money for transit, and eccentric ideas, such as acoustic technology to make apartments soundproof and self-driving housing units that could park on top of one another in a city center. At one point, teleportation enjoyed a brief hearing.

X is perhaps the only enterprise on the planet where regular investigation into the absurd is not just permitted but encouraged, and even required. X has quietly looked into space elevators and cold fusion. It has tried, and abandoned, projects to design hoverboards with magnetic levitation and to make affordable fuel from seawater. It has tried—and succeeded, in varying measures—to build self-driving cars, make drones that deliver aerodynamic packages, and design contact lenses that measure glucose levels in a diabetic person’s tears.

These ideas might sound too random to contain a unifying principle. But they do. Each X idea adheres to a simple three-part formula. First, it must address a huge problem; second, it must propose a radical solution; third, it must employ a relatively feasible technology. In other words, any idea can be a moonshot—unless it’s frivolous, small-bore, or impossible.

The purpose of X is not to solve Google’s problems; thousands of people are already doing that. Nor is its mission philanthropic. Instead X exists, ultimately, to create world-changing companies that could eventually become the *next* Google. The enterprise considers more than 100 ideas each year, in areas ranging from clean energy to artificial intelligence. But only a tiny percentage become “projects,” with full-time staff working on them. It’s too soon to know whether many (or any) of these shots will reach the moon: X was formed in 2010, and its projects take years; critics note a shortage of revenue to date. But several projects—most notably Waymo, its [self-driving-car company](https://www.theatlantic.com/technology/archive/2017/08/inside-waymos-secret-testing-and-simulation-facilities/537648/), recently valued at $70 billion by one Wall Street firm—look like they may.

X is extremely secretive. The company won’t share its budget or staff numbers with investors, and it’s typically off-limits to journalists as well. But this summer, the organization let me spend several days talking with more than a dozen of its scientists, engineers, and thinkers. I asked to propose my own absurd idea in order to better understand the creative philosophy that undergirds its approach. That is how I wound up in a room debating a physicist and a roboticist about apartments floating off the coast of San Francisco.

I’d expected the team at X to sketch some floating houses on a whiteboard, or discuss ways to connect an ocean suburb to a city center, or just inform me that the idea was terrible. I was wrong. The table never once mentioned the words *floating* or *ocean*. My pitch merely inspired an inquiry into the purpose of housing and the shortfalls of U.S. infrastructure. It was my first lesson in radical creativity. Moonshots don’t begin with brainstorming clever answers. They start with the hard work of finding the right questions.

Creativity is an old practice but a new science. It was only in 1950 that J. P. Guilford, a renowned psychologist at the University of Southern California, introduced the discipline of creativity research in a major speech to the American Psychological Association. “I discuss the subject of creativity with considerable hesitation,” he began, “for it represents an area in which psychologists generally, whether they be angels or not, have feared to tread.” It was an auspicious time to investigate the subject of human ingenuity, particularly on the West Coast. In the next decade, the apricot farmland south of San Francisco took its first big steps toward becoming Silicon Valley.

Yet in the past 60 years, something strange has happened. As the academic study of creativity has bloomed, several key indicators of the country’s creative power have turned downward, some steeply. Entrepreneurship may have grown as a status symbol, but America’s start-up rate has been falling for decades. The label *innovation* may have spread like ragweed to cover every minuscule tweak of a soda can or a toothpaste flavor, but the rate of productivity growth has been mostly declining since the 1970s. Even Silicon Valley itself, an economic powerhouse, has come under fierce criticism for devoting its considerable talents to [trivial problems](https://www.theatlantic.com/magazine/archive/2017/11/hampton-creek-josh-tetrick-mayo-mogul/540642/), like [making juice](https://www.theatlantic.com/business/archive/2017/04/juicero-lessons/523896/) or hailing a freelancer to pick up your laundry.

Breakthrough technology results from two distinct activities that generally require different environments—*invention* and *innovation*. Invention is typically the work of scientists and researchers in laboratories, like the transistor, developed at Bell Laboratories in the 1940s. Innovation is an invention put to commercial use, like the transistor radio, sold by Texas Instruments in the 1950s. Seldom do the two activities occur successfully under the same roof. They tend to thrive in opposite conditions; while competition and consumer choice encourage innovation, invention has historically prospered in labs that are insulated from the pressure to generate profit.

The United States’ worst deficit today is not of incremental innovation but of breakthrough invention. Research-and-development spending has declined by two-thirds as a share of the federal budget since the 1960s. The great corporate research labs of the mid-20th century, such as Bell Labs and Xerox Palo Alto Research Center (parc), have shrunk and reined in their ambitions. America’s withdrawal from moonshots started with the decline in federal investment in basic science. Allowing well-funded and diverse teams to try to solve big problems is what gave us the nuclear age, the transistor, the computer, and the internet. Today, the U.S. is neglecting to plant the seeds of this kind of ambitious research, while complaining about the harvest.

No one at X would claim that it is on the verge of unleashing the next platform technology, like electricity or the internet—an invention that could lift an entire economy. Nor is the company’s specialty the kind of basic science that typically thrives at research universities. But what X is attempting is nonetheless audacious. It is investing in both invention and innovation. Its founders hope to demystify and routinize the entire process of making a technological breakthrough—to nurture each moonshot, from question to idea to discovery to product—and, in so doing, to write an operator’s manual for radical creativity.

### II. The Inkling

Inside X’s Palo Alto headquarters, artifacts of projects and prototypes hang on the walls, as they might in a museum—an exhibition of alternative futures. A self-driving car is parked in the lobby. Drones shaped like Jedi starfighters are suspended from the rafters. Inside a three-story atrium, a large screen renders visitors as autonomous vehicles would see them—pointillist ghosts moving through a rainbow-colored grid. It looks like Seurat tried to paint an Atari game.

Just beyond the drones, I find Astro Teller. He is the leader of X, whose job title, captain of moonshots, is of a piece with his piratical, if perhaps self-conscious, charisma. He has a long black ponytail and silver goatee, and is wearing a long-sleeved T‑shirt, dark jeans, and large black Rollerblades. Fresh off an afternoon skate?, I ask. “Actually, I wear these around the office about 98 percent of the time,” he says. I glance at an X publicist to see whether he’s serious. Her expression says: *Of course he is*.

Teller, 47, descends from a formidable line of thinkers. His grandfathers were Edward Teller, the father of the hydrogen bomb, and Gérard Debreu, a mathematician who won a Nobel Prize in Economics. With a doctorate in artificial intelligence from Carnegie Mellon, Teller is an entrepreneur, a two-time novelist, and the author of a nonfiction book, *Sacred Cows*, on marriage and divorce—co-written with his second wife. His nickname, Astro, though painfully on the nose for the leader of a moonshot factory, was bestowed upon him in high school, by friends who said his flattop haircut resembled Astroturf. (His given name is Eric.)

In 2010, Teller joined a nascent division within Google that would use the company’s ample profits to explore bold new ideas, which Teller called “moonshots.” The name X was chosen as a purposeful placeholder—as in, *We’ll solve for that later*. The one clear directive was what X would *not* do. While almost every corporate research lab tries to improve the core product of the mother ship, X was conceived as a sort of anti–corporate research lab; its job was to solve big challenges anywhere except in Google’s core business.

When Teller took the helm of X (which is now a company, like Google, within Alphabet), he devised the three-part formula for an ideal moonshot project: an important question, a radical solution, and a feasible path to get there. The proposals could come from anywhere, including X employees, Google executives, and outside academics. But grand notions are cheap and abundant—especially in Silicon Valley, where world-saving claims are a debased currency—and actual breakthroughs are rare. So the first thing Teller needed to build was a way to kill all but the most promising ideas. He assembled a team of diverse experts, a kind of Justice League of nerds, to process hundreds of proposals quickly and promote only those with the right balance of audacity and achievability. He called it the Rapid Evaluation team.

In the landscape of ideas, Rapid Eval members aren’t vertical drillers but rather oil scouts, skillful in surveying the terrain for signs of pay dirt. You might say it’s Rapid Eval’s job to apply a kind of future-perfect analysis to every potential project: If this idea succeeds, what *will have been* the challenges? If it fails, what *will have been* the reasons?

The art of predicting which ideas will become hits is a popular subject of study among organizational psychologists. In academic jargon, it is sometimes known as “creative forecasting.” But what sorts of teams are best at forecasting the most-successful creations? Justin Berg, a professor at the Stanford Graduate School of Business, set out to answer this question in [a 2016 study](http://http:/journals.sagepub.com/doi/abs/10.1177/0001839216642211) focused on, of all things, circus performances.

Berg found that there are two kinds of circus professionals: creators who imagine new acts, and managers who evaluate them. He collected more than 150 circus-performance videos and asked more than 300 circus creators and managers to watch them and predict the performers’ success with an audience. Then he compared their reactions with those of more than 13,000 ordinary viewers.

Creators, Berg found, were too enamored of their own concepts. But managers were too dismissive of truly novel acts. The most effective evaluation team, Berg concluded, was a group of creators. “A solitary creator might fall in love with weird stuff that isn’t broadly popular,” he told me, “but a panel of judges will reject anything too new. The ideal mix is a panel of creators who are also judges, like the teams at X.” The best evaluators are like player-coaches—they create, then manage, and then return to creating. “They’re hybrids,” Berg said.

**R**ich DeVaul is a hybrid. He is the leader of the Rapid Eval team but he has also, like many members, devoted himself to major projects at X. He has looked into the feasibility of space elevators that could transport cargo to satellites without a rocket ship and modeled airships that might transport goods and people in parts of the world without efficient roads, all without ever touching the ground. “At one point, I got really interested in cold fusion,” he said. “Because why not?”

One of DeVaul’s most consuming obsessions has been to connect the roughly 4 billion people around the world who don’t have access to high-speed internet. He considers the internet the steam engine or electrical grid of the 21st century—the platform technology for a long wave of economic development. DeVaul first proposed building a cheap, solar-powered tablet computer. But the Rapid Eval team suggested that he was aiming at the wrong target. The world’s biggest need wasn’t hardware but access. Cables and towers were too expensive to build in mountains and jungles, and earthbound towers don’t send signals widely enough to make sense for poor, sparsely populated areas. The cost of satellites made those, too, prohibitive for poor areas. DeVaul needed something inexpensive that could live in the airspace between existing towers and satellites. His answer: balloons. Really big balloons.

The idea struck more than a few people as ridiculous. “I thought I was going to be able to prove it impossible really quickly,” said Cliff L. Biffle, a computer scientist and Rapid Eval manager who has been at X for six years. “But I totally failed. It was really annoying.” Here was an idea, the team concluded, that could actually work: a network of balloons, equipped with computers powered by solar energy, floating 13 miles above the Earth, distributing internet to the world. The cause was huge; the solution was radical; the technology was feasible. They gave it a name: Project Loon.

At first, Loon team members thought the hardest problem would be sustaining an internet connection between the ground and a balloon. DeVaul and Biffle bought several helium balloons, attached little Wi‑Fi devices to them, and let them go at Dinosaur Point, in the Central Valley. As the balloons sluiced through the jet stream, DeVaul and his colleagues chased them down in a Subaru Forester rigged with directional antennae to catch the signal. They drove like madmen along the San Luis Reservoir as the balloons soared into the stratosphere. To their astonishment, the internet connection held. DeVaul was ecstatic, his steampunk vision of broadband-by-balloon seemingly within grasp. “I thought, *The rest is just ballooning!*” he said. “*That’s not rocket science*.”

He was right, in a way. Ballooning of the sort his team imagined isn’t rocket science. It’s harder.

Let’s start with the balloons. Each one, flattened, is the size of a tennis court, made of stitched-together pieces of polyethylene. At the bottom of the balloon hangs a small, lightweight computer with the same technology you would find at the top of a cell tower, with transceivers to beam internet signals and get information from ground stations. The computer system is powered by solar panels. The balloon is designed to float 70,000 feet above the Earth for months in one stretch. The next time you are at cruising altitude in an airplane, imagine seeing a balloon as far above you as the Earth is far below.

The balloons have to survive in what is essentially an alien environment. At night, the temperature plunges to 80 degrees below zero Celsius, colder than your average evening on Mars. By day, the sun could fry a typical computer, and the air is too thin for a fan to cool the motherboard. So Loon engineers store the computer system in a specially constructed box—the original was a Styrofoam beer cooler—coated with reflective white paint.

The computer system, guided by an earthbound data center, can give the balloon directions (“Go northeast to Lima!”), but the stratosphere is not an orderly street grid in which traffic flows in predictable directions. It takes its name from the many strata, or layers, of air temperatures and wind currents. It’s difficult to predict which way the stratosphere’s winds will blow. To navigate above a particular town—say, Lima—the balloon cannot just pick any altitude and cruise. It must dive and ascend thousands of feet, sampling the gusts of various altitudes, until it finds one that is pointing in just the right direction. So Loon uses a team of balloons to provide constant coverage to a larger area. As one floats off, another moves in to take its place.

Four years after Loon’s first real test, in New Zealand, the project is in talks with telecommunications companies around the world, especially where cell towers are hard to build, like the dense jungles and mountains of Peru. Today a network of broadband-beaming balloons floats above rural areas outside of Lima, delivering the internet through the provider Telefónica.

Improving internet access in Latin America, Africa, and Asia to levels now seen in developed countries would generate more than $2 trillion in additional GDP, according to a recent study by Deloitte. Loon is still far from its global vision, but capturing even a sliver of one percentage point of that growth would make it a multibillion-dollar business.

### III. The Fail

Astro Teller likes to recount an allegorical tale of a firm that has to get a monkey to stand on top of a 10-foot pedestal and recite passages from Shakespeare. Where would you begin? he asks. To show off early progress to bosses and investors, many people would start with the pedestal. That’s the worst possible choice, Teller says. “You can always build the pedestal. All of the risk and the learning comes from the extremely hard work of first training the monkey.” An X saying is “#MonkeyFirst”—yes, with the hashtag—and it means “do the hardest thing first.”

But most people don’t want to do the hardest thing first. Most people want to go to work and get high fives and backslaps. Despite the conference-keynote pabulum about failure (“Fail fast! Fail often!”), the truth is that, financially and psychologically, failure sucks. In most companies, projects that don’t work out are stigmatized, and their staffs are fired. That’s as true in many parts of Silicon Valley as it is anywhere else. X may initially seem like a paradise of curiosity and carefree tinkering, a world apart from the drudgery required at a public company facing the drumbeat of earnings reports. But it’s also a place immersed in failure. Most green-lit Rapid Eval projects are unsuccessful, even after weeks, months, or years of one little failure after another.

At X, Teller and his deputies have had to build a unique emotional climate, where people are excited to take big risks despite the inevitability of, as Teller delicately puts it, “falling flat on their face.” X employees like to bring up the concept of “psychological safety.” I initially winced when I heard the term, which sounded like New Age fluff. But it turns out to be an important element of X’s culture, the engineering of which has been nearly as deliberate as that of, say, Loon’s balloons.

Kathy Hannun told me of her initial anxiety, as the youngest employee at X, when she joined in the spring of 2012. On her first day, she was pulled into a meeting with Teller and other X executives where, by her account, she stammered and flubbed several comments for fear of appearing out of her depth. But everyone, at times, is out of his or her depth at X. After the meeting, Teller told her not to worry about making stupid comments or asking ignorant questions. He would not turn on her, he said.

Hannun now serves as the CEO of Dandelion, an X spin-off that uses geothermal technology to provide homes in New York State with a renewable source of heating, cooling, and hot water. “I did my fair share of unwise and inexperienced things over the years, but Astro was true to his word,” she told me. The culture, she said, walked a line between patience and high expectations, with each quality tempering the other.

X encourages its most successful employees to talk about the winding and potholed road to breakthrough invention. This spring, André Prager, a German mechanical engineer, delivered a 25-minute presentation on this topic at a company meeting, joined by members of X’s drone team, called Project Wing. He spoke about his work on the project, which was founded on the idea that drones could be significant players in the burgeoning delivery economy. The idea had its drawbacks: Dogs may attack a drone that lands, and elevated platforms are expensive, so Wing’s engineers needed a no-landing/no-infrastructure solution. After sifting through hundreds of ideas, they settled on an automatic winching system that lowered and raised a specialized spherical hook—one that can’t catch on clothing or tree branches or anything else—to which a package could be attached.

In their address, Prager and his team spent less time on their breakthroughs than on the many failed cardboard models they discarded along the way. The lesson they and Teller wanted to communicate is that simplicity, a goal of every product, is in fact extremely complicated to design. “The best designs—a bicycle, a paper clip—you look and think, *Well of course, it always had to look like that*,” Prager told me. “But the less design you see, the more work was needed to get there.” X tries to celebrate the long journey of high-risk experimentation, whether it leads to the simplicity of a fine invention or the mess of failure.

Because the latter possibility is high, the company has also created financial rewards for team members who shut down projects that are likely to fail. For several years, Hannun led another group, named Foghorn, which developed technology to turn seawater into affordable fuel. The team appeared to be on track, until the price of oil collapsed in 2015 and its members forecast that their fuel couldn’t compete with regular gasoline soon enough to justify keeping the project alive. In 2016, they submitted a detailed report explaining that, despite advancing the science, their technology would not be economically viable in the near future. They argued for the project to be shut down. For this, the entire team received a bonus.

Some might consider these so-called failure bonuses to be a bad incentive. But Teller says it’s just smart business. The worst scenario for X is for many doomed projects to languish for years in purgatory, sucking up staff and resources. It is cheaper to reward employees who can say, “We tried our best, and this just didn’t work out.”

Recently, X has gone further in accommodating and celebrating failure. In the summer of 2016, the head of diversity and inclusion, a Puerto Rican–born woman named Gina Rudan, spoke with several X employees whose projects were stuck or shut down and found that they were carrying heavy emotional baggage. She approached X’s leadership with an idea based on Mexico’s *Día de los Muertos*, or Day of the Dead. She suggested that the company hold an annual celebration to share stories of pain from defunct projects. Last November, X employees gathered in the main hall to hear testimonials, not only about failed experiments but also about failed relationships, family deaths, and personal tragedies. They placed old prototypes and family mementos on a small altar. It was, several X employees told me, a resoundingly successful and deeply emotional event.

No failure atX has been more public than Google Glass, the infamous head-mounted wearable computer that resembled a pair of spectacles. Glass was meant to be the world’s next great hardware evolution after the smartphone. Even more quixotically, its hands-free technology was billed as a way to emancipate people from their screens, making technology a seamless feature of the natural world. (To critics, it was a ploy to eventually push Google ads as close to people’s corneas as possible.) After a dazzling launch in 2013 that included a [12-page spread in *Vogue*](https://www.vogue.com/article/the-final-frontier-google-glass-and-futuristic-fashion), consumers roundly dissed the product as buggy, creepy, and pointless. The last of its dwindling advocates were branded “glassholes.”

I found that X employees were eager to talk about the lessons they drew from Glass’s failure. Two lessons, in particular, kept coming up in our conversations. First, they said, Glass flopped not because it was a bad consumer product but because it wasn’t a consumer product at all. The engineering team at X had wanted to send Glass prototypes to a few thousand tech nerds to get feedback. But as buzz about Glass grew, Google, led by its gung-ho co-founder Sergey Brin, pushed for a larger publicity tour—including a [ted Talk](https://www.ted.com/talks/sergey_brin_why_google_glass) and [a fashion show](https://techcrunch.com/2012/09/10/diane-von-furstenberg-models-wear-google-glass-on-the-catwalk/) with Diane von Furstenberg. Photographers captured Glass on the faces of some of the world’s biggest celebrities, including Beyoncé and Prince Charles, and Google seemed to embrace the publicity. At least implicitly, Google promised a product. It mailed a prototype. (Four years later, Glass has reemerged as a tool for factory workers, the same group that showed the most enthusiasm for the initial design.)

But Teller and others also saw Glass’s failure as representative of a larger structural flaw within X. It had no systemic way of turning science projects into businesses, or at least it hadn’t put enough thought into that part of the process. So X created a new stage, called Foundry, to serve as a kind of incubator for scientific breakthroughs as its team develops a business model. The division is led by Obi Felten, a Google veteran whose title says it all: head of getting moonshots ready for contact with the real world.

“When I came here,” Felten told me, “X was this amazing place full of deep, deep, deep geeks, most of whom had never taken a product out into the world.” In Foundry, the geeks team up with former entrepreneurs, business strategists from firms like McKinsey, designers, and user-experience researchers.

One of the latest breakthroughs to enter Foundry is an energy project code-named Malta, which is an answer to one of the planet’s most existential questions: Can wind and solar energy replace coal? The advent of renewable-energy sources is encouraging, since three-quarters of global carbon emissions come from fossil fuels. But there is no clean, cost-effective, grid-scale technology for storing wind or solar energy for those times when the air is calm or the sky is dark. Malta has found a way to do it using molten salt. In Malta’s system, power from a wind farm would be converted into extremely hot and extremely cold thermal energy. The warmth would be stored in molten salt, while the cold energy (known internally as “coolth”) would live in a chilly liquid. A heat engine would then recombine the warmth and coolth as needed, converting them into electric energy that would be sent back out to the grid. X believes that salt-based thermal storage could be considerably cheaper than any other grid-scale storage technology in the world.

The current team leader is Raj B. Apte, an ebullient entrepreneur and engineer who made his way to X through parc. He compares the project’s recent transition to Foundry to “when you go from a university lab to a start-up with an A-class venture capitalist.” Now that Apte and his team have established that the technology is viable, they need an industry partner to build the first power plant. “When I started Malta, we very quickly decided that somewhere around this point would be the best time to fire me,” Apte told me, laughing. “I’m a display engineer who knows about hetero-doped polysilicon diodes, not a mechanical engineer with a background in power plants.” Apte won’t leave X, though. Instead he will be converted into a member of the Rapid Eval team, where X will store his creative energies until they are deployed to another project.

Thinking about the creation of Foundry, it occurred to me that X is less a moonshot factory than a moonshot studio. Like MGM in the 1940s, it employs a wide array of talent, generates a bunch of ideas, kills the weak ones, nurtures the survivors for years, and brings the most-promising products to audiences—and then keeps as much of the talent around as possible for the next feature.

### IV. The Invention

Technology is feral. It takes teamwork to wrangle it and patience to master it, and yet even in the best of circumstances, it runs away. That’s why getting invention right is hard, and getting commercial innovation right is hard, and doing both together—as X hopes to—is practically impossible. That is certainly the lesson from the two ancestors of X: Bell Laboratories and Xerox parc. Bell Labs was the preeminent science organization in the world during the middle of the 20th century. From 1940 to 1970, it gave birth to the solar cell, the laser, and some 9 percent of the nation’s new communications patents. But it never merchandised the vast majority of its inventions. As the research arm of AT&T’s government-sanctioned monopoly, it was legally barred from entering markets outside of telephony.

In the 1970s, just as the golden age at Bell Labs was ending, its intellectual heir was rising in the West. At Xerox parc, now known as just parc, another sundry band of scientists and engineers laid the foundation for personal computing. Just about everything one associates with a modern computer—the mouse, the cursor, applications opening in windows—was pioneered decades ago at parc. But Xerox failed to appreciate the tens of trillions of dollars locked within its breakthroughs. In what is now Silicon Valley lore, it was a 20‑something entrepreneur named Steve Jobs who in 1979 glimpsed parc’s computer-mouse prototype and realized that, with a bit of tinkering, he could make it an integral part of the desktop computer.

Innovators are typically the heroes of the story of technological progress. After all, their names and logos are the ones in our homes and in our pockets. Inventors are the anonymous geeks whose names lurk in the footnotes (except, perhaps, for rare crossover polymaths such as Thomas Edison and Elon Musk). Given our modern obsession with billion-dollar start-ups and mega-rich entrepreneurs, we have perhaps forgotten the essential role of inventors and scientific invention.

The decline in U.S. productivity growth since the 1970s puzzles economists; potential explanations range from an aging workforce to the rise of new monopolies. But John Fernald, an economist at the Federal Reserve, says we can’t rule out a drought of breakthrough inventions. He points out that the notable exception to the post-1970 decline in productivity occurred from 1995 to 2004, when businesses throughout the economy finally figured out information technology and the internet. “It’s possible that productivity took off, and then slowed down, because we picked all the low-hanging fruit from the information-technology wave,” Fernald told me.

The U.S. economy continues to reap the benefits of IT breakthroughs, some of which are now almost 50 years old. But where will the next brilliant technology shock come from? As total federal R&D spending has declined—from nearly 12 percent of the budget in the 1960s to 4 percent today—some analysts have argued that corporate America has picked up the slack. But public companies don’t really invest in experimental research; their R&D is much more *D* than *R*. A 2015 study from Duke University found that since 1980, there has been a “shift away from scientific research by large corporations”—the triumph of short-term innovation over long-term invention.

The decline of scientific research in America has serious implications. In 2015, MIT published [a devastating report](https://dc.mit.edu/sites/default/files/Future%20Postponed.pdf) on the landmark scientific achievements of the previous year, including the first spacecraft landing on a comet, the discovery of the Higgs boson particle, and the creation of the world’s fastest supercomputer. None of these was an American-led accomplishment. The first two were the products of a 10-year European-led consortium. The supercomputer was built in China.

As the MIT researchers pointed out, many of the commercial breakthroughs of the past few years have depended on inventions that occurred decades ago, and most of those were the results of government investment. From 2012 to 2016, the U.S. was the world’s leading oil producer. This was largely thanks to hydraulic fracturing experiments, or fracking, which emerged from federally funded research into drilling technology after the 1970s oil crisis. The recent surge in new cancer drugs and therapies can be traced back to the War on Cancer announced in 1971. But the report pointed to more than a dozen research areas where the United States is falling behind, including robotics, batteries, and synthetic biology. “As competitive pressures have increased, basic research has essentially disappeared from U.S. companies,” the authors wrote.

It is in danger of disappearing from the federal government as well. The White House budget this year proposed cutting funding for the National Institutes of Health, the crown jewel of U.S. biomedical research, by $5.8 billion, or 18 percent. It proposed slashing funding for disease research, wiping out federal climate-change science, and eliminating the Energy Department’s celebrated research division, arpa-e.

The Trump administration’s thesis seems to be that the private sector is better positioned to finance disruptive technology. But this view is ahistorical. Almost every ingredient of the internet age came from government-funded scientists or research labs purposefully detached from the vagaries of the free market. The transistor, the fundamental unit of electronics hardware, was invented at Bell Labs, inside a government-sanctioned monopoly. The first model of the internet was developed at the government’s Advanced Research Projects Agency, now called darpa. In the 1970s, several of the agency’s scientists took their vision of computers connected through a worldwide network to Xerox parc.

“There is still a huge misconception today that big leaps in technology come from companies racing to make money, but they do not,” says Jon Gertner, the author of *The Idea Factory*, a history of Bell Labs. “Companies are really good at combining existing breakthroughs in ways that consumers like. But the breakthroughs come from patient and curious scientists, not the rush to market.” In this regard, X’s methodical approach to invention, while it might invite sneering from judgmental critics and profit-hungry investors, is one of its most admirable qualities. Its pace and its patience are of another era.

### V. The Question, Again

Any successful organization working on highly risky projects has five essential features, according to Teresa Amabile, a professor at Harvard Business School and a co-author of *The Progress Principle*. The first is “failure value,” a recognition that mistakes are opportunities to learn. The second is psychological safety, the concept so many X employees mentioned. The third is multiple diversities—of backgrounds, perspectives, and cognitive styles. The fourth, and perhaps most complicated, is a focus on refining questions, not just on answers; on routinely stepping back to ask whether the problems the organization is trying to solve are the most important ones. These are features that X has self-consciously built into its culture.

The fifth feature is the only one that X does not control: financial and operational autonomy from corporate headquarters. That leads to an inevitable question: How long will Alphabet support X if X fails to build the next Google?

The co-founders of Google, Brin and Larry Page, clearly have a deep fondness for X. Page once said that one of his childhood heroes was Nikola Tesla, the polymath Serbian American whose experiments paved the way for air-conditioning and remote controls. “He was one of the greatest inventors, but it’s a sad, sad story,” Page said in a 2008 interview. “He couldn’t commercialize anything, he could barely fund his own research. You’d want to be more like Edison … You’ve got to actually get [your invention] into the world; you’ve got to produce, make money doing it.”

Nine years later, this story seems like an ominous critique of X, whose dearth of revenue makes it more like Tesla’s laboratory than Edison’s factory. Indeed, the most common critique of X that I heard from entrepreneurs and academics in the Valley is that the company’s prodigious investment has yet to produce a blockbuster.

Several X experiments *have* been profitably incorporated into Google already. X’s research into artificial intelligence, nicknamed Brain, is now powering some Google products, like its search and translation software. And an imminent blockbuster may be hiding in plain sight: In May, Morgan Stanley analysts told investors that Waymo, the self-driving-car company that incubated at X for seven years, is worth $70 billion, more than the market cap of Ford or GM. The future of self-driving cars—how they will work, and who exactly will own them—is uncertain. But the global car market generates more than $1 trillion in sales each year, and Waymo’s is perhaps the most advanced autonomous-vehicle technology in the world.

What’s more, X may benefit its parent company in ways that have nothing to do with X’s own profits or losses. Despite its cuddly and inspirational appeal, Google is a mature firm whose 2017 revenue will likely surpass $100 billion. Growing Google’s core business requires salespeople and marketers who perform ordinary tasks, such as selling search terms to insurance companies. There is nothing wrong with these jobs, but they highlight a gap—perhaps widening—between Silicon Valley’s world-changing rhetoric and what most people and companies actually do there.

X sends a corporate signal, both internally and externally, that Page and Brin are still nurturing the idealism with which they founded what is now basically an advertising company. Several business scholars have argued that Google’s domination of the market for search advertising is so complete that it should be treated as a monopoly. In June, the European Union slapped Google with a $2.7 billion antitrust fine for promoting its own shopping sites at the expense of competitors. Alphabet might use the projects at X to argue that it is a benevolent giant willing to spend its surplus on inventions that enrich humanity, much like AT&T did with Bell Labs.

All of that said, X’s soft benefits and theoretical valuations can go only so far; at some point, Alphabet must determine whether X’s theories of failure, experimentation, and invention work in practice. After several days marinating in the company’s idealism, I still wondered whether X’s insistence on moonshots might lead it to miss the modest innovations that typically produce the most-valuable products. I asked Astro Teller a mischievous question: Imagine you are participating in a Rapid Eval session in the mid-1990s, and somebody says she wants to rank every internet page by influence. Would he champion the idea? Teller saw right through me: I was referring to PageRank, the software that grew into Google. He said, “I would like to believe that we would at least go down the path” of exploring a technology like PageRank. But “we might have said no.”

I then asked him to imagine that the year was 2003, and an X employee proposed digitizing college yearbooks. I was referring to Facebook, now Google’s fiercest rival for digital-advertising revenue. Teller said he would be even more likely to reject that pitch. “We don’t go down paths where the hard stuff is marketing, or understanding how people get dates.” He paused. “Obviously there are hard things about what Facebook is doing. But digitizing a yearbook was an observation about connecting people, not a technically hard challenge.”

X has a dual mandate to solve huge problems and to build the next Google, two goals that Teller considers closely aligned. And yet Facebook grew to rival Google, as a platform for advertising and in financial value, by first achieving a quotidian goal. It was not a moonshot but rather the opposite—a small step, followed by another step, and another.

Insisting on quick products and profits is the modern attitude of innovation that X continues to quietly resist. For better and worse, it is imbued with an appreciation for the long gestation period of new technology.

Technology is a tall tree, John Fernald told me. But planting the seeds of invention and harvesting the fruit of commercial innovation are entirely distinct skills, often mastered by different organizations and separated by many years. “I don’t think of X as a planter or a harvester, actually,” Fernald said. “I think of X as building taller ladders. They reach where others cannot.” Several weeks later, I repeated the line to several X employees. “That’s perfect,” they said. “That’s so perfect.” Nobody knows for sure what, if anything, the employees at X are going to find up on those ladders. But they’re reaching. At least someone is.

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