Why Moonshots Compound and Increments Cannot

The modern smartphone contains a touchscreen developed at CERN for particle-physics control rooms (Stumpe and Sutton), a GPS chip descended from Cold War navigation-satellite programs tied to nuclear submarines and missile-test tracking (Parkinson and Powers), a voice assistant whose algorithms trace back to speech-recognition research funded by the Defense Advanced Research Projects Agency (Austin), and lithium-ion batteries tracing back to U.S. Department of Energy research on alternative energy storage in the 1970s (Brescher Shea). Many of these technologies emerged from public or publicly backed research programs before they became commercially valuable. None of them existed because someone correctly predicted that they would one day fit in a pocket. Every one of them was produced, directly or indirectly, by governments spending money on projects whose stated goals had almost nothing to do with the commercial products that eventually emerged. The device that sits in the average person's hand is the accumulation of a dozen speculative public bets, most of which were supposed to be about something else entirely.

Speculative projects are the only form of public investment that compounds across generations, and they do so through a paradox that cost-benefit analysis cannot capture. The very feature that makes moonshots look irresponsible, the fact that their difficulty is systematically underestimated at the outset, is the precondition for them producing value at all. Incremental research solves the problems of the present. Moonshots, even ones that fail, build the institutional memory, talent pipelines, and technological infrastructure that determine what a nation is capable of attempting fifty years from now.

Private capital cannot fill this gap. Venture funds operate on seven-to-ten-year horizons and require capturable intellectual property to justify their bets, two conditions that moonshots violate by definition. A general-purpose technology like the transistor or the Internet protocol generates value because it diffuses freely across the economy, which is the opposite of what a return-seeking investor needs. The early-stage research that produces such technologies is therefore systematically under-supplied by markets, regardless of how large or sophisticated those markets become. This is a structural feature of how speculative, long-horizon, non-excludable knowledge is produced. If governments do not fund this work, no one will.

The case for moonshot funding rests on a paradox first identified by the economist Albert Hirschman in his 1967 study of World Bank development projects. Hirschman observed that ambitious public projects almost never unfolded the way their planners had expected — costs ran over, obstacles emerged that no one had anticipated, and timelines collapsed. Yet the same projects also benefited from a second, equally consistent pattern: once they were underway, the people running them proved unexpectedly creative at improvising solutions to problems the planners had failed to foresee. Hirschman called this the "Hiding Hand": an invisible tendency by which human overconfidence at the start of a project is offset by human resourcefulness in the middle of it (Hirschman 9–34). If the real difficulty of ambitious projects were accurately priced at the outset, no rational government would ever begin one. Civilization's most consequential public works exist not because they passed a cost-benefit test but because their sponsors failed to correctly imagine how hard they would be.

This is the deepest reason why the cost-benefit analysis used to evaluate incremental research cannot be applied to moonshots. The analysis assumes planners can accurately estimate what a project will cost and what it will produce, the very assumption the Hiding Hand denies. When DARPA funded the ARPANET in the late 1960s, no one involved in the decision correctly predicted the project's difficulty, and certainly no one predicted that the resulting network would eventually become the substrate of global commerce (Abbate 43–81, 181–220). Had DARPA's planners known in advance what they were committing to, the project might well have been rejected as too expensive and too uncertain. The Internet exists because the Hiding Hand was allowed to do its work. Judging a moonshot by whether it achieves its stated goal is like evaluating a university solely by whether its graduates remember their coursework. It misses the deeper transformation in capability.

The first mechanism through which moonshots generate compounding returns is institutional capacity-building. Large-scale speculative projects require governments to construct new organizational architectures such as agencies, laboratories, regulatory frameworks, and international partnerships that persist long after the original mission concludes. The Human Genome Project, launched in 1990 with an estimated cost of three billion dollars, necessitated the creation of bioinformatics databases, ethical review protocols for genetic research, and multinational data-sharing agreements that now form the backbone of contemporary genomics

(Collins et al. 286-90). These institutional structures were not the project's intended output, yet they have proven more durably valuable than the genome sequence itself, which was merely a starting point for the research they enabled.

On the other hand, incremental research adds new knowledge or capabilities within the existing boundaries of a field or problem. Such research extends the known boundaries of conceptual or empirical work in a field rather than challenging its basic premises. While individual increments can appear large for a variety of reasons, the overall path of incremental research is expected to lead to predictable results based on existing knowledge and ideas about how to build on it. This is not a weakness. It is precisely what makes incremental science efficient and reliable. But it means that a research portfolio built entirely on incremental funding cannot expand a nation's institutional frontier. Without periodic moonshot investments that force the creation of new organizational capacities, a country's research infrastructure gradually hardens, becoming optimized for solving yesterday's problems rather than discovering tomorrow's questions.

The second compounding mechanism is human capital formation. Moonshot programs attract and train researchers at a scale and intensity that routine funding cannot replicate. The Manhattan Project produced a generation of physicists, engineers, and project managers whose expertise subsequently powered advances in nuclear energy, medical imaging, and materials science (Rhodes). More recently, the CERN Large Hadron Collider has trained thousands of doctoral researchers in detector physics, data science, and large-scale engineering — skills that have migrated into fields ranging from medical diagnostics to financial modelling (Boisot et al. 201–25).

This talent-pipeline effect is particularly important because human capital, unlike physical infrastructure, is self-replicating. A researcher who has taken part in a moonshot program will remain engaged by mentoring others, establishing labs, and helping to shape their research agenda for many years. As a result, they will help to accumulate knowledge across their field of study and continue to build as part of the larger scientific research community.

Additionally, there is technological spillover, which is the tendency of moonshot programs to generate general-purpose technologies whose eventual applications aren't similar to

the original mission. The World Wide Web was invented at CERN in 1989 as an internal tool for physicists to share particle collision data (Berners-Lee and Fischetti). No cost-benefit analysis of the Large Hadron Collider could have anticipated the world wide web's impact and scale because the value of general-purpose technologies is unpredictable. Similarly, the semiconductor industry that now exists traces its origins to Cold War era military contracts for miniaturized electronics in ballistic missiles, not to any vision of personal computing (Lécuyer).

This pattern is what Nassim Taleb calls “convexity”, a payoff structure in which losses are bounded but gains are not (Taleb 273). A failed moonshot costs a government the money it spent. However, a successful one can reshape entire economy. Incremental research rarely produces these asymmetric payoffs. A targeted study of soil may produce a better fertilizer, but it will not yield a technology that transforms shipping, finance, and medicine. Only moonshot-scale investment in new capabilities tends to generate the kind of platform technologies on which entire economic sectors are built.

The objection to prioritizing moonshot funding is that it diverts resources from urgent, solvable problems. When millions lack access to clean water, basic vaccines, or climate-resilient agriculture, investing billions in interstellar travel or aging reversal can appear as a waste of resources. As Daniel Sarewitz argues, publicly funded science is most legitimate when it addresses identifiable social needs through accountable, evidence-based processes (Sarewitz 141–68). Governments have a fiduciary obligation to their citizens, and speculative investment must not come at the expense of basic welfare.

A related objection says that if moonshots succeed only because governments systematically underestimate their difficulty, then institutionalizing moonshot funding amounts to institutionalizing bad planning. The Hiding Hand is an argument for insulating a portion of public research from the kinds of accountability metrics that would kill ambitious projects at the proposal stage. The alternative, pricing every project correctly at the outset, would eliminate not just the failures but also the successes, because the two are generated by the same mechanism.

The tension between moonshot and incremental funding is less sharp than it appears. Moonshot programs frequently generate technologies that dramatically reduce the cost of solving incremental problems. GPS, a Cold War–era military moonshot, now underpins precision

agriculture that feeds hundreds of millions; the mRNA platform, developed over decades of speculative basic research, enabled COVID-19 vaccines to be designed in days rather than years (Karikó et al.). The question is not whether to fund incremental research, that remains essential, but whether incremental funding alone is sufficient to sustain a nation's long-term scientific capacity. The evidence suggests it is not.

A second aspect that needs to be considered in the moonshot debate is the geopolitical one. Nowadays, scientific capability plays a critical role in a nation's sovereignty, and those countries that are investing the most into speculative research endeavors are the ones that are reshaping the balance of global influence. China's recent scientific rise reflects a state-backed strategy to build technological power and reduce dependence on foreign innovation (Cao 3-5). Projects such as Chang'e-5's lunar sample return, EAST's 1,066-second plasma record, and China's large-scale genomics sector show how scientific ambition can become a tool of national prestige and strategic capacity. Those countries that restrict their research activities only to incremental pursuits might be more efficient in the short run, but they are essentially giving up on cutting-edge technologies (and the power to set the rules that come with them) to their competitors who are prepared to take on speculative risks.

The argument of whether to use moonshot or incremental funding is really a question about time and governance. If we fund only incremental research, we are essentially saying that we should govern for the present, and that we should focus only on the problems that we already know how to solve with the tools we already have. Instead, if we choose to fund a moonshot, we are making a bet for the people who will live and vote in the future, but who are unable to lobby or advocate for their future needs. This is not a careless gamble, but a way to understand what it means to govern in a way that spans generations.The cathedrals of medieval Europe took centuries to build, and the architects and stonecutters who began them knew they would never see them finished. What they left behind was not merely a building but a demonstration of what patient, sustained ambition could achieve across generations. In a world that is more interconnected than ever before, modern governments that are unable to fund the equivalent of a cathedral of science will not leave themselves with manageable problems. Rather, they will leave their successors with diminished capability of confronting this already dangerous world. The greatest risk of the moonshot trajectory is not that we fail, but that we never really try.

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