

Founders Fund Manifesto

– Written by Bruce Gibney

We invest in smart people solving difficult problems, often difficult scientific or engineering problems.

Here's why.

A. INTRODUCTION

The Problem

We have two primary and related interests:

1. Finding ways to support technological development (technology is the fundamental driver of growth in the industrialized world).
2. Earning outstanding returns for our investors.

We have somewhat greater incentives than many other firms to figure out the answers to these questions as the partners and employees of FOUNDERS FUND are collectively the largest investors in our funds (by contrast, industry convention only requires VCs to put up 1% of the total capital of the fund - the perhaps misleadingly named GP "commitment"). At FF, about 20% of the total capital we manage is our own capital.

From the 1960s through the 1990s, venture capital was an excellent way to pursue these twin interests. From 1999 through the present, the industry has posted negative mean and median returns, with only a handful of funds having done very well. What happened?

VC's Long Nightmare

To understand why VC has done so poorly, it helps to approach the future through the lens of VC portfolios during the industry's heyday, comparing past portfolios to portfolios as

they exist today. In the 1960s, venture closely associated with the emerging semiconductor industry (INTEL, e.g., was one of the first – and is still one of the greatest – VC investments).

Although success now makes these investments seem blandly sensible, even obvious, the industries and companies backed by venture were actually extraordinarily ambitious for their eras.

In the 1970s, computer hardware and software companies received funding; the 1980s brought the first waves of biotech, mobility, and networking companies; and the 1990s added the Internet in its various guises. Although success now makes these investments seem blandly sensible, even obvious, the industries and companies backed by venture were actually extraordinarily ambitious for their eras. Although all seemed at least possible, there was no guarantee that any of these technologies could be developed successfully or turned into highly profitable businesses.

When H-P developed the pocket calculator in 1967, even H-P itself had serious doubts about the product's commercial viability and only intervention by the founders saved the calculator.

Later, when the heads of major computing corporations (IBM, DEC) openly questioned whether any individual would ever want or need a computer – or even that computers themselves would be smaller than a VW – investment in companies like Microsoft and Apple in the mid-1970s seemed fairly bold. In 1976, when Genentech launched, the field of recombinant DNA technology was less than five years old and no established player expected that insulin or human growth hormone could be cloned or commercially manufactured, much less by a start-up. But VCs backed all these enterprises, in the hope of profiting from a wildly more advanced future. And in exchange for that hope of profit, VC took genuine risks on technological development.

In the late 1990s, venture portfolios began to reflect a different sort of future. Some firms still supported transformational technologies (e.g., search, mobility), but venture investing shifted away from funding transformational companies and toward companies that solved incremental problems or even fake problems (e.g., having [Kozmo.com](https://www.kozmo.com) messenger Kit-Kats to the office). This model worked for a brief period, thanks to an enormous stock market bubble. Indeed, it was even economically rational for VCs to fund these ultimately worthless companies because they produced extraordinary returns – in fact, the best returns in the industry's history. And there have been subsequent bubbles – acquisition bubbles, the secondary market, etc. – which have continued to generate excellent returns

for VCs lucky enough to tap into them.

We believe that the shift away from backing transformational technologies and toward more cynical, incrementalist investments broke venture capital.

But these bubbles are narrower and the general market more demanding, so VCs who continue the practices of the late 1990s (a surprising number) tend to produce very weak returns. Along the way, VC has ceased to be the funder of the future, and instead has become a funder of features, widgets, irrelevances. In large part, it also ceased making money, as the bottom half of venture produced flat to negative return for the past decade.

We believe that the shift away from backing transformational technologies and toward more cynical, incrementalist investments broke venture capital. Excusing venture's nightmare decade as a product of adverse economic conditions ignores the industry's long history of strong, acyclical returns for its first forty years, as well as the consistently strong performance of the top 20% of the industry. What venture backed changed and that is why returns changed as well.

Not Everything With A Plug Is Technology

Not all technology is created equal: there is a difference between Pong and the Concorde or, less glibly, between Intel and [Pets.com](#). Microprocessing represents real technological development, peddling pet food on-line, less so. Conversely, things that may be dismissed as fake technologies (Amazon and Facebook occasionally receive this critique) often resolve very challenging technological problems. Among its many innovations, Amazon helped develop intelligent customer recommendations and logistical efficiencies that allow you to order almost anything, anytime, and get it the next day; Facebook developed ways to manage large numbers of connections in a computationally efficient way, create an effective developer ecosystem, and to make it pleasurable to administer your on-line relationships. These also deserve attention, of course: though the Internet is no longer the virgin field it once was, we dismiss as bunk the idea that the Internet is tapped out. Web companies that fail are the companies that fail to exploit the true power of the medium.

The Internet is one of the most revolutionary technologies ever developed. If all we ever used x-rays for was in shoe shop fluoroscopes, we would be tempted to dismiss X-rays as infertile and irrelevant. The Internet cannot be judged on the follies of the late 90s alone.

Over time, the market tends to call out fake technologies and companies, which makes it a

risky proposition to invest in them – it's possible to flip a born loser and make a handsome return, but you need to get lucky with timing (i.e., sell into a bubble).

| And then there are the matters of conscience and reputation.

Real technology companies tend to create durable returns, making timing much less important. If you invested in webvan.com, your window of opportunity was measured in months; if you backed Intel, your window of opportunity was measured in decades. Therefore, as investors, we should seek companies developing real technologies.

Are There Any Real Technologies Left?

Have we reached the end of the line, a sort of technological end of history? Once every last retailer migrates onto the Internet, will that be it? Is the developed world really developed, full stop? Again, it may be helpful to revisit previous conceptions of the future to see if there are any areas where VC might yet profitably invest.

In 1958, Ford introduced the Nucleon, an atom-powered, El Camino-shaped concept car. From the perspective of the present, the Nucleon seems audacious to the point of idiocy, but consider at the time Nautilus, the first atomic submarine, had just been launched in 1954 (and that less than ten years after the first atomic bomb). The Nucleon was ambitious – and a marketing gimmick, to be sure – but it was not entirely out of the realm of reason.

Ten years later, in 1968, Arthur C. Clarke predicted imminent commercial space travel and genuine (if erratic) artificial intelligences. “2001: A Space Odyssey” was fiction, of course, but again, its future didn't seem implausible at the time; the Apollo program was ready to put Armstrong on the moon less than a decade after Gagarin, and computers were becoming common place just a few years after Kilby and Noyce dreamed up the integrated circuit.

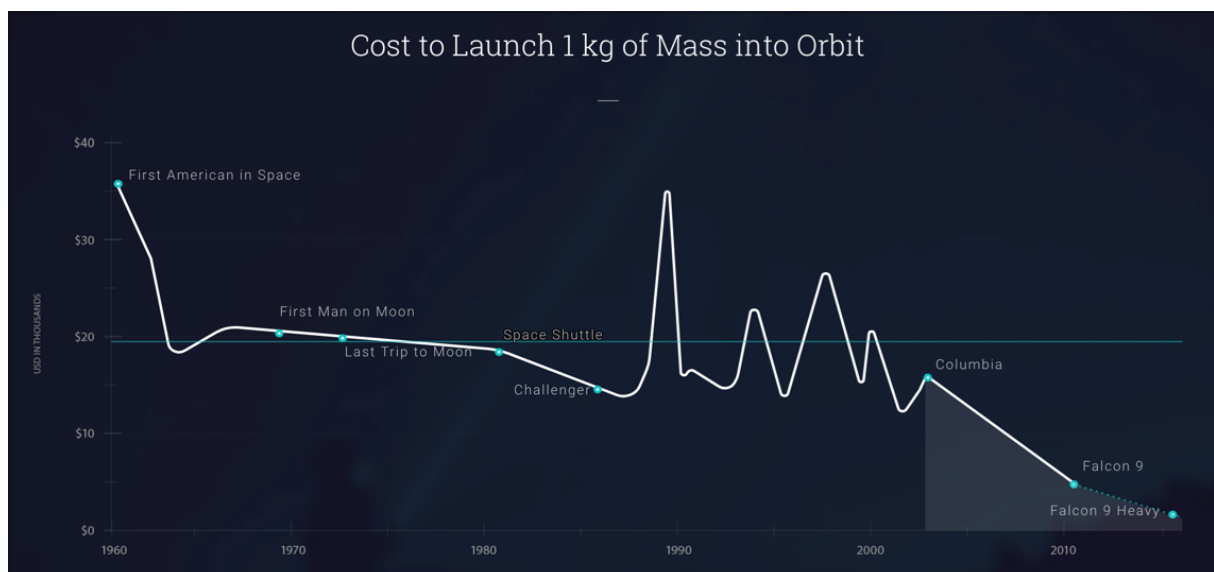
The future envisioned from the perspective of the 1960s was hard to get to, but not impossible, and people were willing to entertain the idea. We now laugh at the Nucleon and Pan Am to the moon while applauding underpowered hybrid cars and Easyjet, and that's sad. The future that people in the 1960s hoped to see is still the future we're waiting for today, half a century later.. Instead of Captain Kirk and the USS Enterprise, we got the Priceline Negotiator and a cheap flight to Cabo.

There are major exceptions: as we've seen, computers and communication technologies advanced enormously (even if Windows 2000 is a far cry from Hal 9000) and the Internet has evolved into something far more powerful and pervasive than its architects had ever hoped for. But a lot of what seemed futuristic then remains futuristic now, in part because these technologies never received the sustained funding lavished on the electronics industries. Commercializing the technologies that have languished seems as good a place as any to start looking for ideas.

B. AEROSPACE & TRANSPORTATION

In 1961, Alan Shepard became the first American in space. In 1969, Neil Armstrong became the first person on the moon. We have not been back to the moon since 1972 and with the final Shuttle flight in 2011, the US will be without the ability to send a person into orbit for the first time since it began its manned space program. For an industry that supposedly defines the future, space isn't doing so well.

One of the major barriers to making use of space is the sheer cost of getting material into orbit: about \$19,000 per kilogram (depending on the orbit), a price that has hardly changed since the 1960s. The elasticity of demand for getting into space at very high price ranges looks basically flat - people who have to go, go (the government, telecommunications providers), and almost no one else chooses to. Were prices to decline, the economic potential of space could be more fully realized.

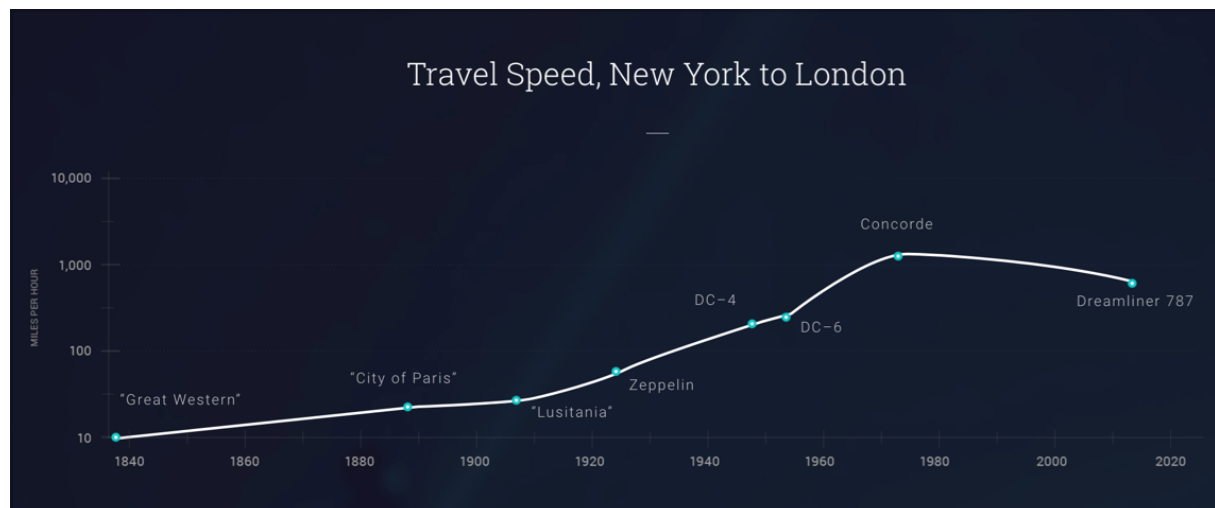


Imagine if it cost you \$500 every time you drove to the Apple store. You'd be inclined to replace your computer and phone much less frequently, even though these devices get

radically better every year. If there were a vastly cheaper way of getting to Best Buy – or work, the gym, or wherever – you’d consume more of that good.

It strikes us then that finding ways to get launch costs down is not only lucrative in its own right, but would vastly increase the size and potential of the space industry, a latter day version of the railroads opening up the West. NASA believes that the commercial market would increase substantially were launch costs reduced by a rough order of magnitude. SpaceX appears to be on track to reduce costs by that order of magnitude, which would make it an enormously valuable company in its own right.

If it succeeds, there should at last be plenty to do in space, from telecommunications to power generation to high-precision microgravity fabrication – if investors with cash are ready to fund that innovation.



Another major area of improvement is overcoming the tyranny of distance. Cheaper, faster transportation has been a major lubricator of trade and wealth creation. For almost two centuries, technology has improved transportation relentlessly. Unfortunately, over the past thirty years, there have been no radical advances in transportation technology (in-flight DVD units are nice, but not revolutionary); take, for example, the travel time across the Atlantic which, for the first time since the Industrial Revolution, is getting longer rather than shorter.

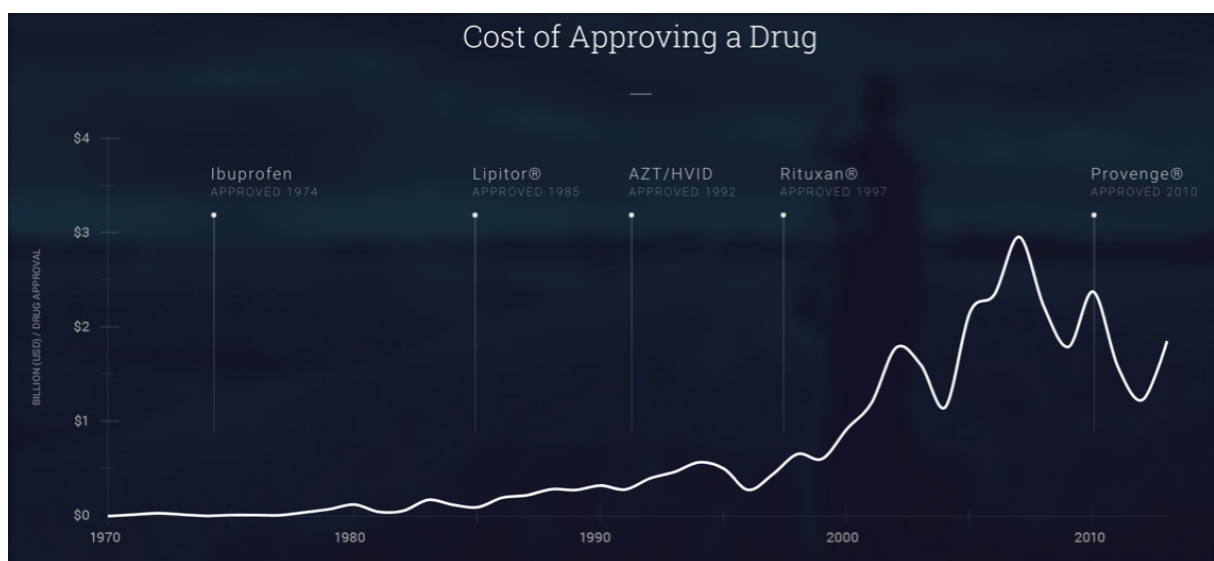
C. BIOTECHNOLOGY

Medicine has been the beneficiary of two radical developments over the past sixty years:

the discovery of the structure of DNA in 1952 and the rise of information technologies in the 1960s. One would expect that the discovery of life's code, combined with the power of computing, would have radically increased the quality and length of human life-spans. But life-spans aren't getting longer as quickly as they used to, and in some places they're even getting shorter. Worse, the number of new drugs introduced each year – especially important new drugs (which you can measure by FDA fast-tracking) – is surprisingly low and well below the quarter-century average. That's not to say that biotechnology can't progress quickly.

Biotechnology has already created one revolution. It can certainly create another.

Less than twenty-five years after Watson and Crick published the structure of DNA, venture capitalist Robert Swanson and biochemist Herbert Boyer founded Genentech, which went on to synthesize insulin far faster and more cheaply than almost anyone believed possible. And in a great revolution in the FDA approval process in the 1980s following pressure from the AIDS lobby, the agency acted almost nimbly to approve a huge number of important new drugs for many maladies. But the revolution in innovation and regulatory efficiency has not been sustained.



Biotechnology has already created one revolution. It can certainly create another. There are presently three major and related obstacles facing biotechnology (or biotechnology investment at any rate): lack of data, capital intensity, and a medieval approach to therapeutic discovery. The first major problem is that genetic sequencing, which provides us with the body of knowledge we require to create genomic therapies, is extremely slow, expensive, and inaccurate.

Present methods of sequencing (which use fluorescence) can only sequence about 95% of

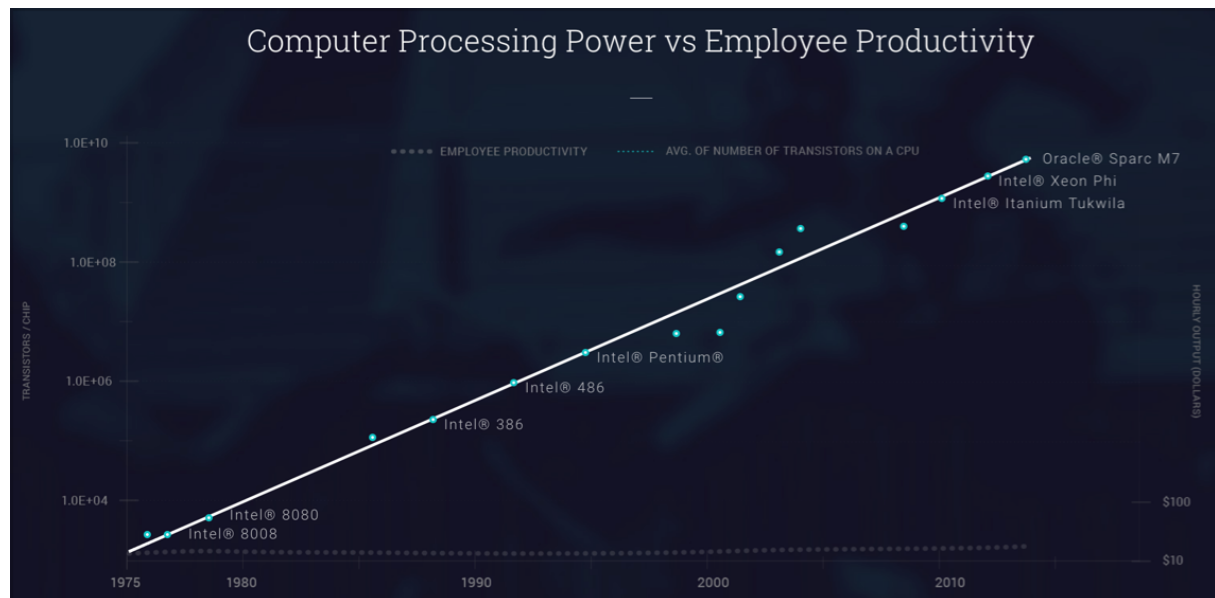
larger genomes, take forever to do so, and cost a fortune. The second problem is capital intensity: it simply takes far too much time and money before a company has any real indication that a drug might work with animal/human trials fantastically expensive despite the help of computer modeling. The final problem is an extremely slow drug discovery process: fundamentally, discovery still proceeds by enlightened guesswork, rather than as a disciplined process – and there is no good way for investigators to share data. Biotechnology companies that can overcome these stumbling blocks will create enormous value for their investors and society.

D. ADVANCED MACHINES / SOFTWARE

The exponential growth of computational power (represented by Moore's law), storage capacity (Kryder's law), data transmission (e.g., Butters' law), and other physical embodiments of computing is familiar. What is equally familiar is the somewhat slower rate of development in the utility of computers – software has gotten more powerful, but the rate of improvement doesn't seem to be as swift as in hardware, though measuring improvements in software is somewhat impressionistic. Nevertheless, as anyone who has used a Bloomberg or Lexis can attest, the amount of data we collect clearly outstrips our ability to make easy use of it. One way to look at this is to compare increases in computing power (as measured by the density of transistors on a chip) versus the change in productivity. Few technologies have ever improved as quickly and consistently as computer processors and yet the impact of computing in the (admittedly wildly overdetermined) productivity statistics is difficult to detect. This suggests that however fast hardware improves, software might be running behind.

We certainly don't have anything approaching a general artificial intelligence, a lack many futurists 30 years ago would have found rather surprising.

Indeed, until fairly recently, it was difficult to find a stable operating system. At the least grandiose level, we need analytical software much more powerful and much easier to use than the current state of the art. Most analytical platforms are exceedingly arcane, requiring lengthy experience with that exact platform to acquire mastery, and yet the quality of analysis remains fairly poor. It does society no good to collect huge amounts of data that only a small minority can analyze, and even then only partially.



Moving up an order of difficulty, robotics represents another area of underachievement. Industrial robots can be very good at what they do (welding car parts, e.g.), but are extremely expensive and of limited versatility. At the highest end, the industry remains over-focused on producing vanity robots with hyper-specific capability – clunky simulacra that play the violin or smile pointlessly – rather than solving more general problems, like locomotion. And few manufacturers are devoted to making commodity-like robots at low price points, which is essential to a genuine robotic revolution.

There are fewer than a million industrial robots, most of which reside in Japan, a country whose demographic constraints dispose it to see robots more as necessities than other advanced nations.

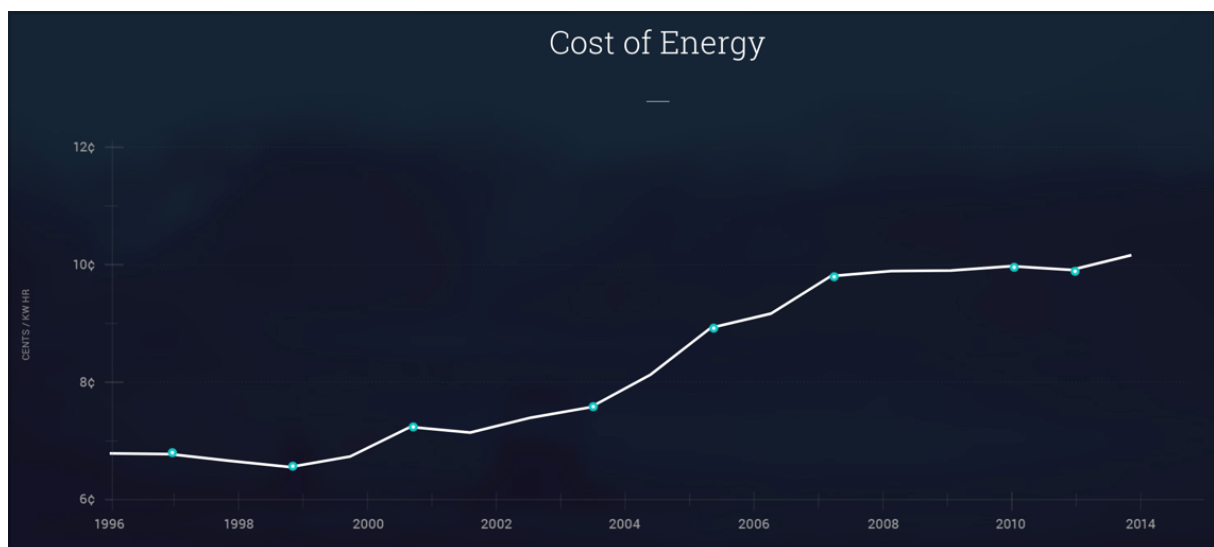
True general artificial intelligence represents the highest form of computing. Whether and when a general artificial intelligence arrives is less critical for the near future than whether we are able to create machines that can replicate components of human intelligence – as we are now doing reasonably well with voice recognition and hopefully will be able to do with visual pattern recognition. At a higher level, machine learning also represents another compelling opportunity, with the potential to create everything from more intelligent game AIs to Watson.

While we have the computational power to support many versions of AI, the field remains relatively poorly funded, a surprising result given that the development of powerful AIs (even if they aren't general AIs) would probably be one of the most important and lucrative technological advances in history.

E. ENERGY

The correlation between wealth and energy use is extremely high and whichever direction the causality runs, a future world of greater material comfort is going to be one that uses more energy (certainly in the aggregate). Unfortunately, conventional sources of energy are extremely problematic, tangled up with political and environmental costs, and in the case of oil, significant geologic constraints. Alternative sources of energy represent a tremendous opportunity, but as the persistently rising real cost of energy shows, we have made little progress in generating more energy more cheaply.

Rising energy costs can reflect many factors, including the internalization of externalities, but as a general matter, real progress would result in a downwardly sloped curve even so, either because new sources of energy were cheaper or because they came with fewer externalities, or preferably, both.



A lot of money has poured into clean technologies. Investments that have focused on efficiency improvements have done well as a financial matter, but investments in alternative technologies for actually generating energy have not produced particularly good returns.

We believe that this is because many companies pursue the wrong model – they seek to be almost as good as the default product, rather than (as should be the case generally) so much better than the default that customers will rush to switch.

Imagine, if you will, if [Amazon.com](#) were somewhat less convenient than going into, and offered similar prices to, a bricks-and-mortar store. Would you use it? Probably not – people only flocked to Amazon when it became substantially better, in selection and convenience, than physical retailers. What we need are companies developing sources of energy that are as good as, or better than, conventional sources at lower prices and at scale. Unfortunately, relatively few companies research such sources, preferring instead incremental improvements on long-established alternative technologies (wind, solar) whose physical limitations mean they cannot satisfy these requirements. But there is no reason to believe that we can't invent an alternative to alternatives.

F. THE INTERNET

It's become fashionable among VCs to say that the Internet is dead, paradoxically even as venture portfolios become more and more concentrated in the same few consumer internet companies (and their clones). The problem with web-bashing, of course, is that the Internet is one of the most powerful technologies ever created and the idea that we have exhausted its potential two decades after we started exploiting it commercially is as ridiculous as saying that there was nothing left to do with electricity after the light bulb.

Companies like Facebook, Spotify, and YouTube demonstrate that there is life after [pets.com](#).

Advances in cloud and other computing technologies radically reduce the costs of starting and running new businesses, creating opportunities for even larger returns. As a general matter, Internet companies that will outperform are the companies that take the Internet seriously – as a technology for transferring information on a scale and at a level of convenience that can't be replicated elsewhere – and that have a plan for translating those advantages into cash. They probably won't look anything like the companies that exist today; all great companies, internet and otherwise, tend to be sui generis.

G. CONCLUSION

Our list is by no means exhaustive. The best companies create their own sectors. As a general matter, the most promising companies (at least from our perspective as investors) tend to share a few characteristics:

1. They are not popular (popular investments tend to be pricey; e.g., Groupon at so many dozens of billions).
2. They are difficult to assess (this contributes to their lack of popularity).
3. They have technology risk, but not insurmountable technology risk.
4. If they succeed, their technology will be extraordinarily valuable.

We have no idea what these companies might look like, only that they probably will share these characteristics. Entrepreneurs often know better than we do what might be enormously valuable in the future.

Not All Real Technology Companies Make Serious Money

Often, even great technologies fail to earn the inventors or investors a return (see, e.g., Nikola Tesla). In our experience, it really does matter who runs the business, because the world does not beat a path to the door of the better mousetrap. Shockley Semiconductor, Fairchild Semiconductor, and Intel all successfully resolved roughly similar technical problems, but only Intel truly prospered – poor management consigned the other two to “also-ran” status.

| Technology matters, but so do teams.

A curious point: companies can be mismanaged, not just by their founders, but by VCs who kick out or overly control founders in an attempt to impose ‘adult supervision.’ VCs boot roughly half of company founders from the CEO position within three years of investment. FOUNDERS FUND has never removed a single founder – we invest in teams we believe in, rather than in companies we’d like to run – and our data suggest that finding good founding teams and leaving them in place tends to produce higher returns overall.

Indeed, we have often tried to ensure that founders can continue to run their businesses through voting control mechanisms, as Peter Thiel did with Mark Zuckerberg and Facebook. This approach, we believe, accords with common sense. No entrepreneur, however good, knows precisely how their company’s business model will evolve over time. When investing in a start-up, you invest in people who have the vision and the flexibility to create a success. It therefore makes no sense to destroy the asset you’ve just bought.

As a corollary, it makes no sense to shackle a company to the Procrustean bed of its

original business model. Businesses really do evolve over time and changing models in the early years is anything but a sign of weakness. PayPal went through five different business models before arriving at one that worked. We do not expect that the first business model for a company will be the final or best business model and do not see evolution as a negative. The most powerful minds are the ones that can be changed.

| Swinging For The Fences Is Probably Less Risky Than People Think

VC usually depends on a few runaway hits to drive returns, supplemented with a few smaller successes and a lot of failures. It seems unlikely, as a general proposition, that a company with limited ambitions will evolve into a runaway hit – i.e., a company that aspires to crank out a single app for the iPhone probably never turns into an Oracle. So we need to invest in at least some ambitious companies – but how many? Our answer is that substantially all of the capital in our portfolio should be directed to companies with audacious vision seeking enormous markets.

Several factors command that conclusion. First, plenty of capital already pursues companies with more moderate ambitions and a lower (perceived) degree of risk. This tends to push up valuations for those companies and correspondingly depresses returns – which, of course, increases overall portfolio risk. Also, less ambitious firms, almost by definition, do not change the world.

| We believe our purpose as venture capitalists is to earn an attractive return by funding positive transformation.

Another, paradoxical reason, is that companies pursuing transformational ideas are somewhat likelier to succeed in them than less ambitious companies. A company with a readily obtainable goal (checkers, for the iPhone!) lacks a technological barrier to entry because, of course, the original problem was easy. And their end markets are typically quite limited, meaning that they may not achieve the scale necessary for exit. But most importantly, we believe the brightest and most creative problem solvers seek the hardest and most interesting problems, and gathering the best technical talent is obviously a major competitive advantage.

The Vision Thing

This brings us to another counterintuitive point: the best founders want to radically change the world for the better. To many investors, visionary entrepreneurs come off as naïve or

worse – isn't it safer/easier/more profitable to create a(nother) social network for cat fanciers than to try to cure cancer, defeat terrorism, or organize the world's information? The problem is that all start-ups are difficult – long hours, low pay, and fierce competition wear on even the most dedicated teams.

The entrepreneurs who make it have a near-messianic attitude and believe their company is essential to making the world a better place. It doesn't matter whether everyone agrees with the entrepreneur about the world-historical nature of the project – if the entrepreneur seeks an impact beyond his own payday and can convince employees of the same, the project is much more likely to get done. The engineers at SpaceX are passionate about commercializing and colonizing space; profit is a significant byproduct of their extraordinary effort to achieve that goal but not enough to get them to pull the thousandth all-nighter. The same is true of Jobs at Apple, or the programmers at Palantir, or the researchers at new drug companies. Early in a company's life, an entrepreneur can make enough money to satisfy his own needs (though often not much of a return for the investor); to take a company from \$50 million to \$50 billion requires singular vision and dedication. Wild-eyed passion is not a bad thing by any means.

It Pays To Be Different

People frequently say that contrarian investments outperform conformist investments. Is that true? It's difficult to make the case directly, but the indirect evidence is suggestive: as we've seen, whatever the bottom 80% of the VC industry is doing now is losing money for investors. Clearly, the mainstream VC model does not work very well. (Even if it did, the problem with consensus investments is that their prices reflect broad agreement, so even if they work, they tend to produce unspectacular returns. That is not the present problem, of course, because the consensus doesn't work at all).

And what does it mean to be contrarian? It does not mean simply doing the opposite of what the majority does – that's just consensus thinking by a different guise, a minus sign before the conventional wisdom. The problems of reactive contrarianism are the same as those of following the herd. The most contrarian thing to do is to think independently. It is not without its risks, because there is no cover from the crowd and because it frequently leads to conclusions with which no one else agrees.

Investing in companies doing things that are breathtakingly new and ambitious is provocative. It is not what our industry is best at doing, at least, not in the past decade. And there is no way to assure a positive return – but at least it has a chance of working.

Simply doing what everyone else does is not enough.

You Have To Run The Experiment

There are unknowables. Venture investments mature over long periods and there are many confounding variables, from variable economic conditions to a shifting legal landscape – everything is overdetermined. Venture is a secretive industry and legal strictures cramp down on disclosures. We have data that suggests what doesn't work (the status quo) and implies what might. But we have no direct evidence for the proposition that we ought to be investing in smart people solving difficult technical problems. In this sense, we are in the same position as our companies, which also operate with imperfect information. SpaceX had three failed launches before making history with its fourth. PayPal went through five business models before it found something that worked, and the history of Facebook's initiatives is by no means an unalloyed record of success. Still, you have to run the experiment.

We do believe that our method should outperform, and we also believe it's the shortest route to social value. So, we will continue to invest in very talented entrepreneurs who are pursuing ambitious, challenging tasks. We will treat them with respect and hope for the best.